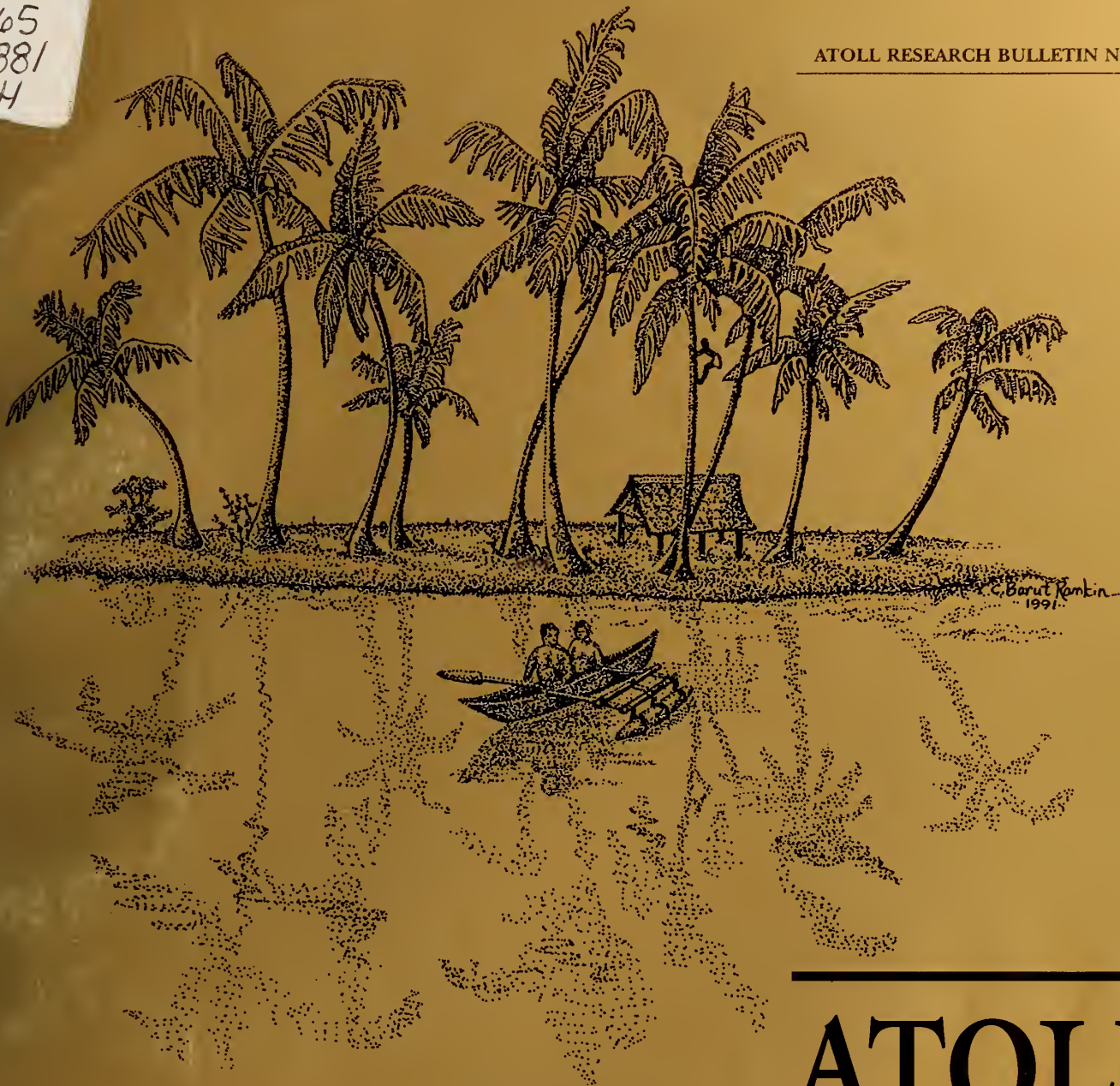


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ATOLL RESEARCH BULLETIN
GOLDEN ISSUE 1951-2001

Edited by
Ian G. Macintyre

ATOLL RESEARCH BULLETIN

Issued by

NATIONAL MUSEUM OF NATURAL HISTORY
SMITHSONIAN INSTITUTION
WASHINGTON, D.C. U.S.A.
DECEMBER 2001



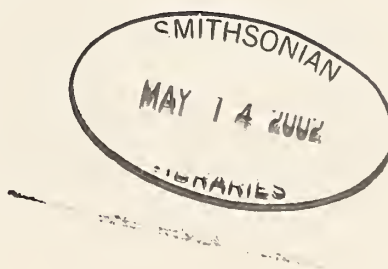
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NO. 494

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Articles submitted for publication in the Atoll Research Bulletin should be original papers in a format similar to that found in recent issues of the Bulletin. First drafts of manuscripts should be typewritten double spaced and can be sent to any of the editors. After the manuscript has been reviewed and accepted, the author will be provided with a page format with which to prepare a single-spaced camera-ready copy of the manuscript.

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F. Raymond Fosberg
1986

Photo V. Krantz



Marie-Hélène Sachet
1979

Photo V. Krantz

ATOLL RESEARCH BULLETIN GOLDEN ISSUE

PREFACE

This Special Golden Issue of the *Atoll Research Bulletin* commemorates the fiftieth year of the publication of this journal. Since 1951 there have been 97 issues with a total of 493 numbered articles.

The journal was the brainchild of F. Raymond Fosberg, who saw it as an outlet for information on coral islands. Ray was particularly concerned that all data collected on Pacific islands during the many postwar projects sponsored by the Pacific Science Board, including the Coral Atoll Program, would be lost in unpublished reports. Eventually he persuaded the Board to support the publication of the *Atoll Research Bulletin*.

The initial issues contained articles describing work supported by the Coral Atoll Program on Arno, Onotoa, Ifalik, and Raroia. Ray was the first editor, but as time went on Marie-Hélène Sachet, his research assistant, took on much of the work of editing and assembling this journal.

The *Bulletin* gradually expanded to include reports on research in the Indian Ocean and the Western Atlantic. For a time, it was supported by the National Academy of Sciences until both Ray and Marie-Hélène left the Academy in 1966 to work at the Smithsonian Institution under the auspices of the Tropical Biology Program. It now continues to be supported by the Director's Office of the National Museum of Natural History.

David R. Stoddart, formerly with Cambridge University, England, and now with the University of California, Berkeley, joined the editorial board in 1969, followed by me in 1979. This editorial board has gradually expanded to include authorities in a number of disciplines who are involved in tropical island and coral reef research. Their names appear on our acknowledgment page. Sadly, Marie-Hélène died in 1986 and Ray seven years later in 1993. The history of the *Atoll Research Bulletin*, and the contributions of Ray and Marie-Hélène, are documented in Mary McCutcheon's excellent index issue (No. 347) and memorial issues for Marie-Hélène (Nos. 293-305) and Ray (Nos. 390-96).

It seems fitting on this historic occasion of the fiftieth anniversary of the *Atoll Research Bulletin* to publish some personal overviews of various fields of research centered on tropical islands and coral reefs. These essays not only help put a face on those scientific endeavors but also remind us of the many accomplishments that have occurred largely during the period that this journal has been published. With a total freedom of format, each author has discussed his or her own field of interest in their own unique way.

Two people have provided valuable assistance in assembling this special issue. Kay Clark-Bourne has helped with the editorial chores and William T. Boykins has played a critical role in scanning and printing illustrations and helping with the final layout. On the production side, this journal has benefited enormously from the advice and assistance of A. Alan Burchell of Smithsonian press for the last 24 years. In addition, special thanks go to my wife, Venka V. Macintyre, for suggesting the personal historical review topic for this issue and assisting with the final editing.

Ian G. Macintyre
Editor



Frank C. Whitmore Jr. 1965

THE PACIFIC ISLAND MAPPING PROGRAM OF THE U.S. GEOLOGICAL SURVEY

BY

FRANK C. WHITMORE, JR.

A little-known aspect of the U.S. Geological Survey history is its involvement in preparing terrain intelligence reports during World War II. The Survey's Military Geology Unit was in charge of compiling these reports on areas of possible military operations. Maps were the main part of these studies; they were compiled from existing maps, geologic and soils literature, aerial and ground photographs, and travelers' accounts. Compiled geologic and soils maps served as basic data for interpretative maps on such subjects as construction materials, water supply, road and airfield siting and construction, and cross-country movement by tanks.

Many of the Strategic Engineering Studies, as these intelligence reports were called, dealt with islands in the western Pacific Ocean. In the course of preparing these, it became apparent that geologic, soils, and even topographic information on the islands was sparse. Furthermore, American engineers were not well informed about some aspects of the tropical environment, such as the behavior of laterite soil when disturbed by earth-moving equipment.

After the war, the Office of the Engineer, Far East Command, U.S. Army, was responsible for the construction and maintenance of bases in the western Pacific. Also, the United States had assumed responsibility for the Trust Territory of the Pacific Islands, including the Caroline, Gilbert, Marshall, and Palau Groups and the islands of Yap. Members of the U.S. Geological Survey, assigned to the occupation forces in Tokyo, were consulted about construction and water-supply problems in the islands.

The Army's continued interest in Pacific island geology coincided with a long-cherished hope of Harry S. Ladd, who in 1946 was Assistant Chief Geologist of the USGS. In the 1920's Ladd, with J. Edward Hoffmeister, had mapped the geology of Fiji. Ladd, an invertebrate paleontologist, had become interested in the Tertiary paleogeography of the Pacific Basin and in the origin and history of atolls; the latter was demonstrated in subsequent years by his involvement in deep drilling on Bikini and Eniwetok.

In 1945 Ladd conceived the idea of a long-term geologic mapping program in the islands of the western Pacific, and in January, 1946, he went to Tokyo to discuss the idea with officers of the Office of the Engineer, Far East Command. Their response was encouraging, and Ladd returned to Washington, where the approval of the Office of the

Chief of Engineers was soon given. A mechanism for undertaking the program was available in the form of an agreement, reached in 1942, between the Corps of Engineers and the Geological Survey, that established the Military Geology Unit, to be supported by funds transferred from the Corps to the Survey.

It was agreed that the mapping would start with Okinawa in the spring of 1946. From the Army's point of view the choice was obvious: Okinawa was slated to continue as a major U.S. base in the western Pacific. Geologically, it was a difficult first step in a long-range program: it was large and extremely varied—from Tertiary sediments in the south to complex Paleozoics in the north. The beginnings of a logistic backup organization were present in Tokyo with the Office of the Engineer and a contingent of USGS geologists who comprised the Mining and Geology Division, Natural Resources Section, General Headquarters, Supreme Commander for the Allied Powers.

Thomas A. Hendricks of the U.S. Geological Survey was serving as Chief, Mining and Geology Division, Natural Resources Section. When the Pacific Island Mapping Program was approved, Hendricks published a notice in the U.S. Army Newspaper, *Pacific Stars and Stripes*, asking geologists in uniform to take their discharges in the Pacific Theatre and join the Okinawa mapping party as civilian members of the U.S. Geological Survey. Four did so. One of them, Gilbert Corwin, was already on Okinawa. The other three, Delos E. Flint, Raymond A. Saplis, and Warren Fuller, took their discharges from the service in Japan. McClelland G. Dings, an experienced USGS mapping geologist, was sent from Washington to become the first chief of the Okinawa field party.

My connection with the island mapping program began on July 1, 1946, when I became Geologist in Charge, Military Geology Unit, USGS. I had been serving in the Natural Resources Section in Tokyo, but in the spring of 1946 was on temporary duty with the XXIV Corps in Korea, where I carried out reconnaissance surveys of ports, railroads, and highways. When I was summoned from Korea to Washington for my new job, I faced the problem of the Okinawa party and of planning, staffing, and supporting other island mapping projects.

My preparation for the job of branch chief was unusual. I received the Ph. D. in vertebrate paleontology from Harvard in 1942, with a minor in geomorphology. From 1942 to 1944 I was the one-man geology department at Rhode Island State College (now the University of Rhode Island). While there, I instituted a course in engineering geology. In 1944 I joined the Military Geology Unit, USGS, as an editor, and became chief editor of the Unit. In 1945 I was assigned to the Office of the Engineer, Southwest Pacific Area, in Manila, and in October, 1945 entered Tokyo with the occupation forces. There I located sand and gravel for extension of Japanese airfields and made a study of precious metal resources. My background was notable for a lack of mapping experience: my mapping was (and still is) limited to a brief field course while in graduate school. This deficiency was ameliorated by my two years' experience editing geologic maps. I was chief of the Military Geology Branch from 1946 to 1959, when I returned to paleontology with the Paleontology and Stratigraphy Branch, USGS, stationed at the National Museum of Natural History. There I have concentrated on the study of fossil whales.

The island program was originally projected to take ten years; eventually it took about fifteen. The mission of the new program was far different from that of the Natural Resources Section, which was devoted to the inventory of Japanese minerals, soils, agriculture and fisheries as a basis for policy decisions aimed at putting the Japanese economy on a self-supporting level. The island mapping program, on the other hand, included not only the Ryukyu islands, which were eventually recognized as part of Japan proper, but also Guam, a U.S. possession, and, most extensive, the Trust Territory of the Pacific Islands, including the Mariana Islands other than Guam, the Marshall Islands, Caroline Islands, Yap, Gilbert Islands and Palau. It was therefore decided that a separate office would be established in Tokyo to oversee the island mapping program. This office, established in 1948, was placed in the Office of the Engineer, Far East Command, U.S. Army, rather than under General Headquarters, Supreme Commander for the Allied Powers. It was staffed by personnel of the U.S. Geological Survey. Sherman K. Neuschel (Fig. 1), a geologist with mapping experience in the Mineral Deposits Branch, USGS, was made chief of the Tokyo Office. The office functioned as a unit of the Army command in the Far East; its mapping program was initiated by the USGS, approved by the Engineer, Far East Command, and funded by the Office of the Chief of Engineers in Washington, DC.



Figure 1. Sherman K. Neuschel talks to native of Yap Island, 1948.

The initial function of the Tokyo office was to provide logistic support for the field parties. Field-party personnel were recruited by the Military Geology Branch, USGS, in Washington. They were then sent to Tokyo, where their travel within the Pacific area was arranged by the Army (Fig. 2). But as the program developed, the Tokyo office became much more than a staging area or travel agency.

As Chief of the Military Geology Branch (formerly the Military Geology Unit), I established a policy for our mapping programs: we would prepare maps and reports to meet the needs of military agencies, and at the same time we would publish fundamental scientific papers in the appropriate literature. Many of the scientific contributions were published as Professional Papers of the USGS; some of these were descriptions and analyses of the geology of the islands, while others were paleontologic monographs. The military geology reports were published in Tokyo by the Office of the Engineer, Far East Command. Each report included a geologic map and a soils map, with accompanying text. These were followed by interpretative sections, each with one or more maps: engineering geology, including construction materials, foundation conditions, road construction and maintenance, and airfield siting; water supply, terrain analysis, including suitability for cross-country movement by tracked vehicles, and landing beaches. A list of military reports on Pacific islands was published as Appendix 2 by Corwin (1998); Bonham (1997) compiled a list of all publications, both military and scientific, of the Military



Figure 2. New recruit being welcomed to the Tokyo office. David B. Doan (3rd from left), Frank C. Whitmore (5th from left), Helen L. Foster (6th from left), Gilbert Corwin (far right).

Geology Branch.

In deciding which islands to map, the main criterion was diversity, a consideration that met both military and scientific needs. As far as I know, there was never any consideration of strategic importance in choosing islands to be mapped; I think that MacArthur's march across the Pacific, including many unlikely atoll battlegrounds, was too fresh in our memories to allow us to attempt prediction of the course of future operations. So we chose island types: high complex islands such as Okinawa, which contains some Paleozoic rocks and which we now know to be related to continental plates; other high islands of more oceanic nature and with a heavy volcanic component, such as the Marianas; and then the series illustrating Darwin's theory of atoll formation, from the cone of Ponape (which was not mapped) to the sunken caldera of Truk to the atolls of the Marshalls. The list of islands to be mapped was made by the Military Geology Branch, and as far as I can remember our recommendations were always accepted by the Corps of Engineers.

It is not the purpose of this paper to present a chronologic account of the mapping of the islands; this can be gleaned from Corwin, 1998. Rather, I wish to discuss the organization of the work, how it was undertaken, and how its philosophy evolved as the work went on.

In 1948 and the years immediately following, it was hard to hire young geologists for work abroad at government salaries. Most young men had just gotten out of the armed services and had already seen enough of the world; many had gone back to

school under the G.I. Bill. And the oil companies were hiring at salaries far higher than the government could offer.

USGS geologists sent overseas during the war and the occupation of Japan had been on six-month assignments, although these were sometimes extended to as much as a year. This policy was followed during the first few years of the island mapping program, and it was soon obvious that the arrangement was inefficient. Most of the geologists, although well trained, were not experienced in mapping; furthermore, in islands of continental aspect such as the Ryukyus and Marianas, comparisons had to be made with Japan, Taiwan, and the well established Dutch East Indies Tertiary section. So even with a full complement, mapping went more slowly than planned. Also, the personnel supply line was long and slow: geologists were hired by the Branch Chief's office in Washington, sent to Tokyo on military orders, indoctrinated there and, after receiving Pacific Theater orders, flown to the island of their assignment. Corwin (1998) gives a pathetic picture of what it was like on Palau between the departure of one contingent and the arrival of the next.

Okinawa and Palau, the earliest projects, suffered from incomplete staffing. The Okinawa project took three years to complete and had three party chiefs. The Saipan operation (September, 1948 - August, 1949) was a turning point; under the dynamic leadership of Preston E. Cloud, Jr., it produced a military geology report and a voluminous Professional Paper (Cloud et al.). By the early 1950's, recruiting and logistic arrangements had improved to the point where field parties were fully staffed. Guam, Pagan, Ishigaki, Miyako and Truk were mapped by geologists most of whom stayed through the entire period of the field work, and a reconnaissance was carried out in the atolls of the Marshall Islands.

As the program progressed, the size of the Tokyo office increased. The office expedited the flow of people and supplies to and between the islands. Much of the existing geologic literature concerning the islands was in Japanese, so a translating office was established, staffed by Japanese geologists hired by the U.S. Army. A staff of Japanese draftsmen, supervised by a USGS cartographer, drafted the maps for the military geology reports, and editors sent from the USGS edited the text. In addition to geology, soil science was deemed important in the program because of its application to military operations, especially cross-country movement by tracked vehicles. Two senior soil scientists from the U.S. Department of Agriculture served six-month stints with the Okinawa party, and one served the entire term of the Guam party. Younger soil scientists also served on Okinawa and other islands. A hydrologist conducted extensive mapping on Guam. An engineering geologist visited almost every island to help the geologists in preparing the applied geology maps, on which units were defined according to engineering properties rather than geologic age. F. Raymond Fosberg, a world-famous authority on Pacific botany, served as a consultant to most of the island mapping parties, making vegetation maps and adding to his already huge herbarium at the United States National Museum of Natural History. Fosberg worked for the Military Geology Branch (MGB) under an informal (and unwritten) arrangement worked out between us. Each year, MGB was committed to submit terrain analyses of six countries to the Engineer Intelligence Division in Washington. Each report included a vegetation map. Ray

prepared the vegetation maps and worked as needed for the island mapping program. If there was any time left over (and there usually was, for Ray was a fast worker), Ray was free to do whatever he wished if he could raise the money to support his field work.

It was necessary to correlate Tertiary strata of the various islands, which led to dependence on paleontology and to the organization of a cadre of specialists on fossil mollusks, corals, foraminifera, diatoms, and other organisms. These specialists, mostly at universities, were hired by the USGS on a WAE (when actually employed) basis, which allowed payment for part-time work. Thus information could go directly to parties in the field. Many USGS Professional Papers resulted from this work, and some paleontologists visited the field parties.

CONCLUSIONS

Without the logistic support of the U.S. Army, the Pacific Island Mapping Program would have been at best difficult, and probably impossible. This cooperation between science and the military has a long tradition: the voyage of the *Beagle*, the *Discovery* expedition co-sponsored by the Royal Society of London, the American Wilkes Expedition, and the Wheeler Survey of the mid-nineteenth century in the western United States are some examples.

Many of the geologists in the program, although well trained, had little or no previous experience in geologic mapping. Chiefs of early parties, although they served short terms in the islands, were crucial in establishing mapping procedures and in training the younger men. On Okinawa, McClelland Dings was succeeded by F. Stearns MacNeil, an experienced Tertiary stratigrapher, who also served for a short time. MacNeil was succeeded by Delos E. Flint, a Caltech graduate who had mapped for the USGS in Cuba. Flint remained in charge of the project until its completion. On later projects, experienced geologists were assigned as party chiefs for the full period of the project: Preston E. Cloud, Jr. on Saipan, Joshua I. Tracey, Jr. on Guam (Fig. 3), and John Stark on Truk. An important part was played by Charles G. Johnson, an experienced groundwater geologist who served through the entire program. He mapped Yap single-handed, monitored many island projects, and eventually succeeded Neuschel as head of the Tokyo office. Toward the end of the program David B. Doan, who had been trained on Guam, was party chief on Miyako in the Ryukyu Islands. Helen L. Foster, a Ph. D. from the University of Michigan who had worked in the Tokyo office compiling a massive bibliography of Pacific Island geology, became party chief on Ishigaki, also in the Ryukyus. Gilbert Corwin (Fig. 4), who had mapped on Okinawa and been party chief on Palau, was named party chief for Pagan, a volcanic island in the Marianas. This project, which came late in the program, was unique in that, of its four members, two had extensive experience in preparing terrain intelligence reports in the Military Geology Branch. One was Lawrence D. Bonham, who later became chief of the branch; the other was Maurice J. Terman, who had spent several years preparing trafficability maps of Europe in the MGB office in Heidelberg, Germany.

The Pagan report was much larger than would have been expected from such a small island. This was because of more detailed analysis of the military engineering



Figure 3. Members of the Tokyo office make a field visit. Mt. Alutom, Guam, 1952. Left to right: Harold May, Joshua I. Tracey (Guam party chief), Sherman K. Neuschel (Chief, Tokyo Office), and Frank C. Whitmore (Chief, Military Geology Branch).



Figure 4. Gilbert Corwin (left) confers with Carl Stensland (soil scientist), Tokyo, 1956.

properties of the surficial deposits of the island, making this study a useful precursor for later terrain analyses.

A unifying factor in the program was the USGS tradition of geologic mapping—an art that is hard to reduce to the written word. It is taught in the field, and chief among many complex components of the technique is an awareness of scale, which governs the definition of map units, both for basic geology and for applied subjects such as location of construction materials. Most mapping was at scales of 1:25,000 or 1:50,000, depending on the size of the island. These large scales allowed separation of map units which would have to be lumped together at smaller scales. This multiplication of map units, together with heavy vegetative cover, slowed the field work.

The soil scientists did not have the benefit of the world-recognized time-rock classification available to geologists. In the United States, the Division of Soil Survey, U.S. Department of Agriculture, had gone to great pains to erect a soil-classification system with the catchy title *The Seventh Approximation*, but in foreign areas with

tropical soils the U.S. classification had to be extended. Even in the United States, when mapping of a new area began, a senior soils correlator would go first and set up a map legend for mappers to follow. This system was followed on the major Pacific islands: Edwin Templin, a senior soils correlator, set up the map legend for Okinawa and consulted on the soils mapping of other islands, and Roy Simonson, later assistant chief of the Soil Survey, also participated in the Okinawa mapping, to be followed by more junior soil scientists who finished the work and went on to map on other islands.

Although, as mentioned above, a philosophy of mapping and publication governed the program, there was never tight control as to how an island should be mapped. This control was left in the hands of the party chief, and there is no doubt that the interests and experience of these geologists affected the end product so that, although the military geology reports look uniform, they probably differed in ways that would be apparent to the geologists if not to the engineers. The military engineering maps were probably more uniform than were the geology and soils maps, because many of them were designed with the help of Allen H. Nicol, an engineering geologist who visited most of the island parties. The progress of mapping projects was monitored by means of visits by S.K. Neuschel, the chief of the mapping program, who headed the Tokyo office, and occasionally by the chief of the Military Geology Branch from Washington.

For the younger members of the program, the island mapping was a sort of graduate school. Most of them lacked advanced degrees; a young man who spent several years on an island would come home pretty well heeled, and many used their money to attend graduate school. For some, the experience instilled an interest in a specific geologic speciality. Two outstanding examples can be noted. Richard L. Hay, now a professor at the University of Illinois, was assigned as a Corps of Engineers private to the Truk Party under John Stark. There he began the interest in volcanic stratigraphy that led to his unravelling the stratigraphy of Olduvai Gorge and dating the famous hominid footprints at Laetoli. The late Seymour O. Schlanger, who at his death was professor at Northwestern University, began an interest in limestone petrology on Guam, and extended his work to global consideration of oceanic sedimentation.

Since the end of the Pacific Island Mapping Program, the theory of plate tectonics has revolutionized geology. Despite this, the program has bequeathed a database that will remain useful. Possibly this usefulness results from the pragmatic aim of the work, which encouraged the objectivity of direct observation, with little of the bias that comes with theoretical preconceptions. Perhaps work of this sort can be called exploration, in contrast to the analysis that will follow, for there was no effort to synthesize the results of the program. In light of subsequent developments in geology, this was probably just as well.

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Joshua I. Tracey, Jr. (Ifaluk Island, 1953)

WORKING IN THE PACIFIC

BY

JOSHUA I. TRACEY, JR.

INTRODUCTION

My education and training leading to my becoming a “coral reef specialist” did not proceed in any logical manner through courses in biology and the expected specialties. In fact, I graduated from Yale with a degree in physics and mathematics and I spent my first year in graduate school staggering through Leigh Page’s five-days per week, one-and-a-half hours per day course “Physics 100” in mathematical physics, which students claimed required 100 hours of study per week. This convinced me that I had no future in physics and I decided to turn to geology, although my background there was not promising. I had taken a first-year geology course in my junior year! So in my second year in graduate school I started with courses in geology that I should have taken as an undergraduate.

By the time the war erupted, I was on my way to becoming a geologist. I joined the U.S. Geological Survey in early March, 1941 in Eufaula, Alabama and was shortly sent to Richland, Georgia to prospect for bauxite — the ore of aluminum. For two months I walked the Wilcox- Midway contact, wherein lies the bauxite zone — if any. I plodded across two counties without adding to our nation’s supply of aluminum ore and was then told to get in my car and drive to Little Rock, Arkansas where I joined a growing group of young, eager but green, geologists who were watching a number of drill rigs drilling for bauxite south of Little Rock (an area called “Sweet Home”), and about 35 miles southwest of Little Rock near the company town of Bauxite. The drilling program was run by the U.S. Bureau of Mines and at its peak comprised 20 drill rigs, drilling two shifts per day. The holes were staked out by Bureau of Mines engineers and the cores were logged by U.S. Geological Survey (USGS) geologists.

As time went on, the drilling program declined and the drill rigs and the geologists were sent to other parts of the country to look for other critical minerals for the war effort. Eventually I was the lone geologist left in Little Rock.

In February 1946, I closed down the project and went to Washington, D.C. I took my first leave since joining the Survey and went to visit my parents in New Haven, Connecticut where I received a phone call from Harry Ladd, one of my bosses on the Survey, asking me if I would like to join a large group of scientists who were going to Bikini Atoll to study this area before and after two atomic-bomb test explosions. So I agreed and flew across the country to board the U.S.S. *Bowditch*, AGS-4, that was to be the base ship for the oceanographic group during the tests.

BIKINI ATOLL

Many of the scientists — oceanographers, biologists, and I, the lone geologist (until Harry Ladd arrived) — sailed in March 1946 from Mare Island, California, and used the long voyage to Honolulu and then down to Bikini Atoll to plan our programs. As most of us had never seen an atoll, there was little planning that could be done until we reached our destination.

At Bikini, our problem was to examine the reefs from seaward margin to lagoon at a number of traverses around the atoll and to examine all islets, large and small. We also made traverses at Eniwetok, Rongerik, and Rongelap atolls.

In the summer of 1947 we returned to Bikini for five weeks with a drill rig and drilled several holes, one to a depth of 2,556 feet, with all drill rods in the hole. A number of cores were attempted, but core recovery was very poor. During this period, Ladd and I revisited most of the traverses that we had made the previous year. Because of the radioactivity remaining from the two atomic bomb detonations in 1946, it was necessary that we always be accompanied by a monitor with a geiger counter wherever we went. We noticed little evidence of the nuclear explosions and the radioactivity was only noted by our monitors.

THE ISLAND OF GUAM

I spent over three years (from 1951 to 1954) on Guam, the largest and southernmost of the Mariana Islands (Fig. 1). It is 30 miles long, from 4 to 11 miles wide, and has an area of 212 square miles. The northern half of the island is a broad limestone plateau bounded by cliffs and the southern half is a dissected volcanic upland that is commonly deeply weathered (Figs. 2, 3). No streams flow on the porous limestone in contrast to the southern volcanic area, which contains numerous streams. Typhoons are common in the vicinity of Guam (Fig. 4) with the chances about one in three that one or more damaging typhoons will hit the island in any given year.

My main concern as field party chief was mapping the geology of this island, surveying the offshore reefs (Fig. 5), determining the water resources, and studying the soil profiles (Fig. 6). This was a major undertaking that involved a team of well known scientists including: K.O. Emery (marine geology); J.T. Stark (petrology of volcanic rocks); S.O. Schlanger (petrology of limestones); C. H. Stensland (soil studies); and W. Storrs Cole (Foraminifera). It was one of my most satisfying undertakings and resulted in many published reports, most notably the USGS Professional Paper 403 A-H published in 1964 *et seq.*

Working on Guam had its challenges, particularly dealing with steep cliffs and, in some areas, very dense vegetation. In one these heavily vegetated areas we were acutely aware of being watched by Japanese soldiers who had not surrendered and existed by scrounging from a U.S. Air Force dump at the base of a cliff. At one point an Army bus was stopped by one of these fellows who calmly removed his worn sandals and turned himself in.



Figure 1. Our headquarters on Guam, November, 1951.



Figure 2. Jack Stark standing on extensively weathered pillow lava, Guam.



Figure 3. Frank Whitmore on folded bedded tuffs and a remnant of a laterized plateau, June, 1952.



Figure 4. Large breakers on Cabras Island, Guam, following a typhoon, December 17, 1953.



Figure 5. With a dinghy on Hilaan Beach, Guam, where we were looking at off-shore reefs (Josh Tracey on right).



Figure 6. Soil-trench survey, Mt. Bataa, Guam, July, 1954.

During my assignment in Guam I made several side trips to other islands, most notably Pagan and Fais and the islands of Ifaluk Atoll, the fourth of five Pacific Science Board atoll surveys. On Pagan we surveyed a transect across the island ending up on the

edge of the volcanic crater at the north end of the island (Figs. 7, 8). Later we visited Fais (Fig. 9) where we again carried out a brief survey and noted some of the activities of the local inhabitants (Figs. 10, 11).



Figure 7. Pagan Island crater with inner cone.



Figure 8. On north wall of crater, Pagan Island.



Figure 9. Landing on Fais Island, September, 1953.



Figure 10. Native on Fais Island making sennet (a type of rope).



Figure 11. Copra bagged and ready for export, Fais Island.

arrived on June 22. Already at Ifaluk were Donald P. Abbott of Stanford University, Marston Bates of the University of Michigan, and Edwin Burrows of the University of

Ifaluk is a small atoll about 350 miles south of Guam. I arrived there with Theodore Arnow of the USGS, Robert R. Harry (now Rofen) of Stanford University and Frederick M. Bayer of the Smithsonian Institution, on the Trust Territory supply ship *Metomkim* on September 12, 1953. We joined members of the first phase of the program who

Connecticut. Because of limitations imposed by the U.S. Navy on the number of Americans on the atoll, Bates and Burrows were scheduled to leave on September 18, and Tracey and Arnow to remain only until September 26.

Ifaluk was such a small atoll that it received scant notice from either the Germans or Japanese, or the Americans who had successively administered the Pacific Islands. Few of the native inhabitants had had any experience with the outside world and the atoll had endured only minor acculturation. The Navy wanted to keep it that way. Previously, a pair of anthropologists, Edwin Burrows and Melford Spiro, had been sent to Ifaluk in 1947 by the Navy as part of its Co-ordinated Investigation of Micronesian Anthropology (CIMA) program sponsored by the National Research Council. This was especially beneficial for our project as Burrows already knew the people and was known by them.

Discovery of Ifaluk is credited to Wilson who in 1797 was aboard the missionary ship *Duff*, but who did not land on the atoll. The first Europeans to visit were Lüke in 1828 aboard *La Sényavine* and Sarfert in 1909 who was with the German Südsee-Expedition of 1908-10, but both Germans and Japanese maintained outposts there during their respective trusteeships.

The land area of Ifaluk is only about half of a square mile and supported a population of 260 inhabitants (Fig. 12) at the time of our visit. Although we interacted with several “commoners” who acted as interpreters and field assistants, our “official” hosts were the chiefs of the atoll: the high chief was Fagolier; the second was Maroligar (Fig. 13); the third, Toroman; the fourth, a woman whom we did not meet; and the fifth, Wolpaitik — minor in rank but major in bulk, as he was the only person on the atoll with a full-fledged paunch.



Figure 12. Ted Arnow photographing a group of children, Ifaluk, September, 1953.

With the help of two local young men, Talimeira and Sagolimar, we made a quick survey of the islands. I would peer down the plane table telescope (Figs. 14, a, b) and wave my hands to communicate with my two assistants who



Figure 13. Maroligar – the second chief of Ifaluk. (Photo F.M. Bayer)



Figures 14. Josh Tracey surveying on Ifaluk: (a) Setting up plane table with assistants looking on. (b) Close up of plane table with native assistant. (Photos F.M. Bayer)

took turns handling the surveying rod. In short order the coastlines were surveyed and contours added — not much to show as the islands were only about 15 feet above mean sea level.

It was during this survey that we discovered that the north end of Falarik Island had been a separate island up until relatively recent times. This was indicated by a conspicuous indentation on the windward side of the island, and vegetation patterns, and was confirmed by the old chief, Toroman. I also spent time looking at the reef topography on the reef flats and checking the quality of water in ponds (Fig. 15) and wells.



Figure 15. From left to right, Josh Tracey, Don Abbott, Bob Harry and Ted Arnow inspecting a pond on Ifaluk. (Photo F.M. Bayer)

When I look back at my experiences on Ifaluk, probably the most memorable ones were the times of using the open outhouse over the lagoon and casually greeting the various passers-by. Privacy was a minor concern on this small island.

DRILLING ON MIDWAY ATOLL (1965)

In 1960, in his Presidential Address to the Geological Society of Washington, Harry Ladd proposed the drilling of Midway Atoll in the Hawaiian Islands. His reasons for desiring a hole on Midway sprang from his idea that Tertiary faunal migration had proceeded westward from Pacific island centers towards Indonesia, following the tradewind and current patterns, rather than eastward from Indonesia into the Pacific, as was believed by most paleontologists. Ladd had noted three molluscan zones in the lower Miocene at Bikini and Enewetok, and thought if he could find these three zones at Midway, his ideas would be confirmed.

To justify an expensive drilling program at Midway required some assurance of a section above volcanic rock thick enough and old enough to contain the hoped-for lower Miocene faunal zones. Ladd persuaded George M. Schor and associates of the Scripps Institution of Oceanography to run two seismic lines in March, 1963 and December, 1964 along the southern and northern sides of the atoll. Their interpretations suggested that the sedimentary section over volcanic rock thickened from less than 1,000 feet south of Sand Island to 2,000 to 3,000 feet along the northern part of the lagoon, a section thick enough to justify the drilling.

Promoting a drilling program on Midway required support, and the Department of Defense and the Atomic Energy Commission, which had underwritten the Marshall Islands drilling, had little interest in the depth to volcanic rock at Midway. So Ladd joined with George P. Woollard, Director of the Hawaii Institute of Geophysics, University of Hawaii, and Gordon A. Macdonald, also with the university and formerly with the USGS Volcano Observatory, to apply to the National Science Foundation for a grant to support the drilling and they were successful in receiving the funds.

Geologists who participated in logging the drilling and in carrying out studies of the reefs, lagoons, and islands of Midway and Kure that were undertaken concurrently were: H.S. Ladd (Fig. 16) and J.I. Tracey, Jr., of the USGS; M. Grant Gross of the University of Washington; Ted Chamberlain, William Ebersole, and Ted Murphy of the University of Hawaii; and W. Storrs Cole of Cornell University.



Figure 16. Harry Ladd inspecting a large blow hole on the reef flat, Enewetok.

The drilling was carried out in July, 1965, under contract with Layne International, Inc., of Hawaii under the supervision of William Craddick. Two crews totaling seven men operated continuously on 12-hour shifts. The drill was a truck-mounted Failing 2500.

Coring below a depth of 70 feet was accomplished using a rubber-sleeve core barrel that attained a remarkable recovery of 72 percent in the Sand Island hole and 92 percent in the Reef

hole, even in friable or unlithified sections. The Sand Island hole, which was drilled on the north shore of Sand Island, contained 516 feet of post-Miocene and upper Miocene

(Tertiary *g*) sediments overlying 52 feet of basalt.

In order to drill the Reef hole, a site was prepared about 80 yards inside the northern reef where the lagoon floor was eight feet deep at high tide. The drill and all the necessary equipment were positioned in a Navy barge, which was towed to the site and sunk (Fig. 17). The drill tower when raised was guyed to nearby coral heads and to the "Reef Hotel," an abandoned loran station on pilings in the lagoon. Drill crews and

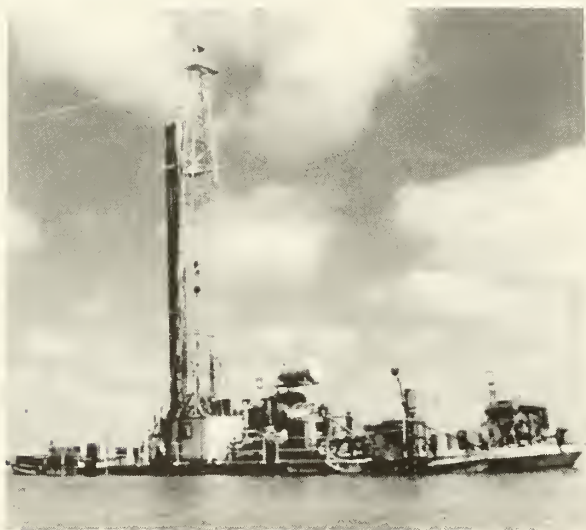


Figure 17. Drilling barge sunk in the lagoon of Midway Island, July, 1965.

geologists had to be brought by boat from Sand Island four miles away, a pleasant trip in good weather. In the Reef hole, 400 feet of post-Miocene limestone were penetrated, underlain by 75 feet of upper Miocene limestone and dolomite, followed by 370 feet of lower Miocene limestone and dolomite, 180 feet of marl, carbonaceous clay and volcanic clay, 40 feet of clay and conglomerate, and finally 393 feet (cored) of basalt.

Freeing and floating the barge took almost three days, after which the nearly three-month-long program was ended.

The drill rig and all equipment were left on the barge, which was towed to Pearl Harbor by a Navy ship, but was

swamped in heavy seas and sank before getting home to port. Fortunately all boxes of cores had been shipped separately!

CARMARSEL EXPEDITION (1967)



Scripps Institution of Oceanography organized the CARMARSEL Expedition with the objective to resolve the late Quaternary sea-level controversy in the Micronesian area. Of particular interest was the question of whether or not in the last 6,000 years there were sea levels in this area that were above present sea level. The work included probing for peat samples, shallow drilling into coral limestone (Fig. 18), bathymetric and seismic surveys, and searching for emergent reefs and rubble ridges. The investigators included

Figure 18. Drilling a shallow hole on one of the small Truk islands, January, 1967.

Francis P. Shepard, William A. Neuman, Arthur L. Bloom, Norman D. Newell and myself. I participated in the work on Truk and Ponape. It was indeed an interesting time working with this distinguished group of scientists.

The result of this work was that there was good evidence for a higher than the present Holocene sea level on tectonically active Guam but none at all on the more stable Caroline and Marshall Islands.

DEEP-SEA DRILLING (1970)

From October 8 to December 2, 1970, I was cochief investigator, with George H. Sutton of the University of Hawaii, of the Joint Oceanographic Institutions for Deep Earth Sampling (JOIDES) D/V *Glomar Challenger* 8th Leg, which extended from Johnston Atoll to Tahiti (Figs. 19, 20). It was very busy work on eight-hour shifts



Figure 19. D/V *Glomar Challenger* at the dock in Honolulu, Hawaii.



Figure 20. Research team for D/V *Glomar Challenger* Leg 8. Josh Tracey is standing third from the left.

during which I spent most of my time describing the cores with both binocular and petrographic microscopes (we had a technician on board who made thin sections).

My memories of this trip, other than the heavy work schedule, were of the outstanding food. Our Thanksgiving dinner included turkey, prime rib, steaks, and rabbit. One morning after getting off my shift the cook loaded up my plate with three poached eggs. When I protested, he assured me that you lose most of the egg when you poach it!

ENDERBURY ISLAND (1971)

In 1971 I spent some time drilling (Fig. 21) shallow holes to determine the recent history of the formation of this island. We drilled four holes ranging in depth from 54 to 103 feet. The very small drilling rig was mounted on a frame about 25 feet high (Fig. 21) that was carried from site to site by a helicopter. The water used in the drilling was likewise carried by helicopter. The small core barrel, about one inch inside



Figure 21. Brian Hackman watching drillers, Enderbury Island, 1971.

diameter, did not get good recovery in the crumbly limestone and gravelly phosphatic sands. Nevertheless we obtained a reasonable idea of the extent of the phosphatic material. Three of the four holes showed the presence of phosphatic sand from a few feet to 8 feet thick that average 5% to 10% P_2O_5 at depths of 12 to 45 feet.

DRILLING OPERATIONS ON ENEWETOK (1971-1974)

I was only involved in some of this work. First in 1972, I participated in the Pacific Atoll Coring Expedition (PACE 1971-1972) when we drill two holes into the Enewetok reef flat to determine the thickness of the Holocene reef section. Later I joined the Exploration Program on Enewetok (EXPOE 1973-1974) that carried out much more detailed coring operations across the island and to the outer edge of the reef flat. The objective was to obtain a better understanding of the reef history in this area preserved in both Pleistocene and Holocene reef sections. This was the last of my field work in the Pacific.



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

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, never married, 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

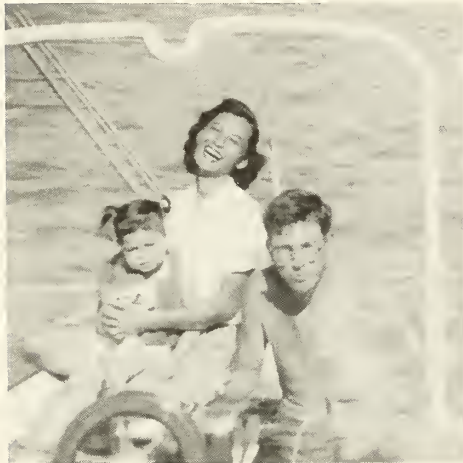


Figure 4. Jack, Helen, daughter Lori (2 1/2), and crew member Norman Baker shortly before sailing *Nani* to Tahiti, November, 1955. (Photo E. Nygren)

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.

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;



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

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.

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 zigzagged 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)

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 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 corridor of seagrass across the bare

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 laying 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 Maguëyez 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.

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

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



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 missile-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



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

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.

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), Maldives 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 organisms, 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 CO₂ 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)

CO₂ 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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Roger B. Clapp, November 13, 2001 (Photo C. Angle)

AUTOBIOGRAPHICAL NOTES

OR

FUN WITH FIELD WORK

BY

ROGER B. CLAPP¹

EARLY YEARS

My interest in birds developed early. My father had a habit of dragging his children along on hikes to various local points despite our (I am the eldest of five) basic disinterest in same. Once, when I was about five, I was riding along on my father's shoulders, while making faces at my sister Judy, who had just relinquished the favored position. Suddenly a brilliant red bird flew across in front of us in the forest ahead. I instantly demanded to know what it was, but my father, who had once worked as a forester's assistant, knew a good bit about trees but very little about birds. For some reason this irritated me unduly and I became very determined to be able to identify birds. Parenthetically, considering the locality (Connecticut) and habitat, the bird was almost certainly a male Scarlet Tanager (*Piranga olivacea*).

Not too long after that incident, my grandmother gave me one of the Chester A. Reed pocket guides to birds. At 3 x 5 inches and with red, blue, green, and yellow covers, these guides provided truly vile illustrations of birds and some descriptions, so bad that my ambitions were put on hold for some time, although my interest in the outdoors became greater as did my willingness to go on walks with my father, with whom otherwise, I never got on that well.

It was not until I was 11 (1949) and in sixth grade that my interest in birds once again became acute when a schoolmate on the bus showed me the first edition of Peterson's Field Guide to Eastern Birds. I knew that I had to have the book and later obtained it when I traded my entire stash of comic books, some 80, to obtain it. The schoolmate felt a bit cheated as some were coverless, but I wasn't about to return the book.

Thus enabled, I spent many long hours tramping through the fields and forests of Connecticut attempting to identify birds and often accompanied either by my dog, Rex, or by my cat, Sam. The frustration of trying to identify relatively common birds with any certainty was leavened by an occasional triumph such as my first encounter with Pine Grosbeaks (*Pinicola enucleator*). I was en route to visit some friends in Amesville (or Sugar Hill as it was known locally for the abundance of sugar maples there), tramping through 18" of snow. Suddenly I encountered some robin-sized birds, some wine colored (males as I later learned) and others more rusty, sitting atop the snow.

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What fascinated me was their incredible tameness that allowed me to get within a very few inches before they took flight.

I remember high school, which I regarded as only a little less dreary than grammar school, mostly because of various birding jaunts. During one winter my family was quarantined because all five of us had the mumps and I managed to spend nearly eight weeks watching a trolley feeder that I had made. The continual opportunity for reinfection led me to the distinction of having had mumps three times, first on one side, then the other, then again on the first side.

I have never liked school (or generally regimentation of any sort), but I have always liked learning new things so high school was not entirely boring. Among the various subjects taught, I found the courses in science and English the most interesting, the latter due to the skills of one teacher, Arthur Kobler ("The Cobra"), who lived at the far end of the street on which I lived in Amesville. He actually challenged the students to learn something and was a dynamic teacher in whose classes I actually did not read something else hidden inside my school books.

My interest in birds led me to the practice of taking off Fridays during my senior spring (my mother was always willing to write an excuse). The area in which I birded, along a stretch of road leading to Canaan, had much marshy ground and a swamp that made it fairly good for shorebirds, at least those few that frequented inland Connecticut, but was one along which a number of my teachers commuted to school. At first, I tended to duck out of sight if I saw one coming, but later became emboldened enough to wave to them as they went to work. Eventually I was called on the carpet by the principal who mildly asked me whether it might not be better if I came to school. I replied that I thought not as I was, as he knew, getting straight A's and was the school's first National Merit Scholar as well. He seemed a trifle irritated, but nothing more was said and I continued Friday birding. I eventually ended up as salutatorian, something I would have just as soon done without, but as I scored so highly on a general knowledge test given that senior year that I was pulled up from sixth to second in my class.

During that year I also invited Aretas A. Saunders to give a talk to our science club. Saunders, one of the first authors to produce a guide to the songs of wild birds (1951), and I had a fine time, particularly because I could identify many of the songs which he was a virtuoso at imitating. The rest of the science club was not so entertained, however, as most were geeks interested in rockets and other mechanical hardware. Following this meeting, unbeknownst to me, Saunders nominated me for a student membership in the American Ornithologists' Union whose periodical "The Auk" gave me a good notion of what constituted scientific ornithology in those days.

Because that was the first year of the National Scholarship Program, I received more publicity than I desired and my half-formed notions of going into conservation in some manner led to strangers accosting me in the A&P Store in Lakeville, where I worked summers to earn some spending money. All seemed seriously worried that I was not going into law or medicine or something that would make much money, but this has never been a major concern for me, and I managed to be polite to all my presumably misguided well-wishers.

COLLEGE DAYS

I had applied only to Cornell, which I knew had an ornithology program, despite the suggestions of the guidance counselor that I apply elsewhere as well. I felt, not unjustifiably, that my grades and National Merit Scholarship would ensure my acceptance there, which indeed occurred. What I did not expect, during my freshman orientation, was to discover press notices mentioning my presence there in Fernow Hall where the field vertebrate zoology classes were held. I had hoped to be relatively unnoticed, drop back to a gentleman's C, and spend more time pursuing my interests, but I found this was not to be.

During my first year, while I was taking introductory ornithology, mostly because I thought I knew enough about birds to get an easy A, I went out on one of the few class field trips led by a graduate student as well as our eminent professor, Charles G. Sibley. While on this trip and mostly talking with a friend, I kept calling out identifications of various birds that we heard, usually a split second before the doubtlessly somewhat annoyed professor did. Nonetheless, following the field trip he asked me to come to his office, which I did. He queried me about my future intentions, which were pretty nebulous and something on the order of becoming a forest ranger. Sibley convinced me that it would be worthwhile to attempt to become an ornithologist, a decision that changed my life forever.

As a freshman I much desired to do field work. Another student, Craig Smith, and I braced Sibley about taking a trip to the Amazon. (We lacked money, but Sibley always had a major talent for obtaining grants). Sibley was not convinced and suggested that we instead go to Mexico to collect hybrid towhees in the mountains and to take a look at the lowlands during the last week of our expedition. To lead this expedition, he chose Fred C. Sibley, a graduate student, who had previously worked for Charles Sibley collecting hybrid buntings and orioles in the Great Plains. Fred was a markedly skilled field man who was not only good at handling the logistics of the truck-based survey, but also unusually able at collecting and preparing birds.

This expedition undertaken during the summer of 1958 markedly changed my opinion about becoming an ornithologist because it turned out I liked field work extremely well. To my surprise, however, the mountains where we were collecting Collared Towhees (*Pipilo ocia*) and Spotted Towhees (*Pipilo erythrophthalmus maculatus*) and their hybrids were both high and cold and I remember being strongly impressed by a shower of pellets of frozen snow during one July morning. I was somewhat disappointed in the bird life at these heights of 9000-11000 feet because many of them were the same species (if not the same subspecies) as the dooryard birds, e.g. Eastern Bluebird (*Sialia sialis*) and American Robin (*Turdus migratorius*), that I had known in northwestern Connecticut.

The lowlands were another matter, however, and I was thrilled to see my first Amazon parrots flying to roost along the Rio Corona (Fig. 1) in Tamaulipas in the same area mentioned and then immortalized by George C. Sutton (1951, 1972) in books reporting his visits there. In other areas one could easily become fascinated by exotic fare such as motmots, trogons, jacanas, thickknees, and an astonishing variety of hummingbirds.

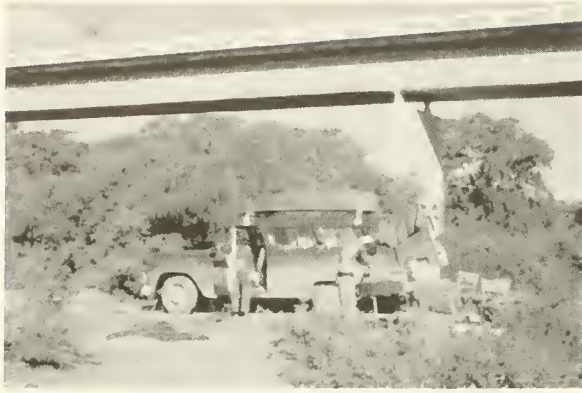


Figure 1. Camp on the highway near the Rio Corona. June 12, 1961.

The work in Mexico was not without hazard, however. During the following summer when Fred, Sid Tamm, and I returned for another session of collecting mountain hybrids, I mistakenly did not check my sneakers for visitors and an inch-long black scorpion jumped out and stung me in the chest. Not only was it extremely painful, but I found, to my horror, that it had also completely stopped my breathing reflex. I realized that I was in considerable trouble and went for help

from my colleagues, humping along on my back like a giant earthworm to where they were preparing breakfast outside the tent. To my panic and incredible disgust, neither of my colleagues could tell what was happening and kept asking “What is wrong, Roger?” when it was pretty obvious to me that I was not breathing and couldn’t tell them anything. My thoughts, considering I was afraid I might die, were not very charitable, but as everything was fading to black the breathing reflex kicked back in, much to my relief. I had a hard time even speaking to the others for the rest of the day, but I never again made the mistake of not inspecting my footgear with some care.

Later during the trip another incident led me to reflect on the hazards of field work. As we were preparing bird skins in our tent near a road on the Isthmus of Tehautepec, we heard a sudden popping sound which led me to turn to Fred and say “firecrackers?” just as Fred grabbed his leg with an exclamation. As it turned out, someone had emptied a .22-caliber rifle into our tent and had gotten Fred. Later I examined where I had been sitting and saw a hole about 6” above where my head had been. While Sid drove Fred to town I waited more than a little nervously for their return, shotgun in hand, but fortunately encountered no one. That humid night, with hundreds of enormous fireflies drawing large J’s through the air, is certainly one that I never will forget.

Sibley’s interests had gradually turned from the study of hybrid birds to the study of egg- white proteins as a better indicator of the relationships of higher taxonomic categories of birds. As one of his students I took to collecting eggs for him with some enthusiasm, but perhaps not with the deviousness that led him to some trouble with British authorities. Nonetheless, I remember once anchoring David West, then a graduate student, while he with his great height and long arms removed Cliff Swallow (*Hirundo pyrrhonota*) eggs from their nests under the eaves of a New York barn, while Sibley kept the farmer engaged in conversation where he could not see our depredations.

During the spring I continued to get eggs for Sibley with some enthusiasm, and quite openly, which led to me becoming persona non grata with the staff of the Laboratory of Ornithology at Sapsucker Woods where it was believed I was the one who emptied all their nest boxes one fine day. Actually, I was innocent of their accusations,

although I did know who had taken the eggs. At the time, I thought taking eggs from nest boxes was really not sporting.

Nonetheless, I was guilty of removing all the Common Grackle (*Quiscalus quiscula*) eggs from their nests in the park at the head of Lake Cayuga, later learning that this colony was part of a doctoral study by Robert W. Ficken. I also collected a clutch of Black Tern (*Chlidonias niger*) eggs from a floating nest at Spencer Marsh to which I had to swim and return with the eggs in my mouth. I later learned that this was apparently the first known nesting there in some years. Thus, one can see the basis for the suspicion with which I was greeted by the less collecting-oriented ornithologists and students.

On the last of our trips from Cornell to Mexico (and British Honduras) our goals were to collect egg-white protein for Sibley's studies. To this end the three of us, myself, John S. Weske, and Martin Michener, proceeded southward in the spring of 1961. The others' knack for finding nests and eggs was not great, unfortunately, but we had a few successes, including one stop in the lowlands at Isla, Veracruz (Fig. 2), where the fence posts were composed largely of small living trees that had begun regrowth at the top, providing an almost perfect nesting place for nesting birds (Fig 3). Almost one of every five posts had a small flycatcher's nest, mostly Vermillion Flycatchers (*Pyrocephalus rubinus*), Tropical Kingbirds (*Tyrannus melancholicus*) and Social Flycatchers (*Myiozetetes similis*), but also an occasional nest of the Fork-tailed Flycatcher (*Tyrannus savana*), Derby Flycatcher (*Pitangus sulphuratus*) and Dusky-capped Flycatcher (*Myiarchus tuberculifer*). Here, Martin distinguished himself by finding a clutch of Double-striped Thick-knee (*Burhinus bistriatus*). Our take for the day was about 60 eggs (Fig. 4), less impressive than one might think because it was relatively late in the breeding season and most of the eggs, when opened, proved to be too incubated for use in protein studies.



Figure 2. Our campsite at Isla, Veracruz.



Figure 3. Flycatcher nesting habitat, Isla. Jun 17, 1961.

Another technique for getting nests was using heavy loads of shot shells to shoot down hanging bird's nests. One person would sever the branch on which the nest (usually Rose-throated Becards (*Platypsaris aglaiae*) hung, and another person would catch the nest before it hit the river or ground as the case might be (Fig. 5). This



Figure 4. Eggs collected on our best day, 17 June 1961, at Isla, Veracruz.

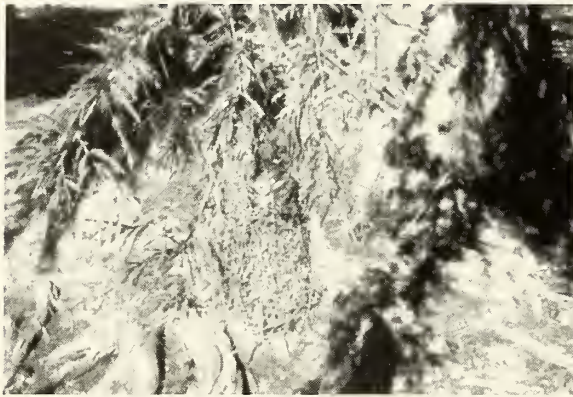


Figure 5. Becard nest obtained by shotgun, Rio Corona, Tamaulipas, Mexico. June 13, 1961.

procedure was effective, if one managed to catch the nest, but resulted in getting many nests with young becards that did not survive.

This trip perhaps was the one that solidified my interest in bird eggs and nesting biology, an interest I maintain to the present day. I often think that I was born about 50 years too late as I certainly would have been an enthusiastic oologist, back when such activities were more respectable.

As we traveled through Campeche on 16 July, we stopped to view a heron colony that had been mentioned in Edwards (1955) guide to finding birds in Mexico. Deciding to go into the colony, some distance from the road, to check for eggs we found not only American Egrets (*Casmerodius alba*) and Anhingas (*Anhinga anhinga*), but also Agami Herons (*Agamia agami*), then unknown for the area (Michener et al. 1964). I returned with a shotgun to obtain some of these herons. Once within the colony I quickly got lost and could not find my way out. No matter which direction I

turned it seemed the water was getting deeper so that, at one point, I was breathing with my upper lip under water as I held the shotgun over my head. Eventually I found higher ground, and my way out, but I had some nervous moments for awhile.

INTO THE PACIFIC WITH THE PACIFIC PROJECT

After college, I heard of a project at the Smithsonian Institution to study the fauna and flora of the central Pacific Ocean. I applied for a job and to my delight got one as a technician. Arriving at the Smithsonian in about July 1963, I worked on various preparations while anxiously awaiting a chance to go into the field. This chance came in October 1963 when I flew to Honolulu to participate on the third of the SIC (Southern Island Cruises). They were SIC(k) cruises for many indeed. Fred Sibley, once again our fearless leader, maintained a staunch, but slightly green color, on the foredeck (Fig. 6) but I was pleased to discover on this trip and others that I am apparently immune to seasickness. Instead of getting sick, I get sleepy, usually sleep for 10 or 11 hours, and then I am troubled no more.



Figure 6. Fred Sibley, exhibiting his usual greenish hue, as he scans the ocean for seabirds.

Some of my colleagues became a good bit more than marginally sick. The late Ralph W. Schrieber, later noted for work on the Brown Pelican (*Pelecanus occidentalis*) (e.g. Schrieber 1977, 1979), won the Pacific Project prize for our most seasick person. During a trip in 1966 when we were again visiting the “Southern Islands” (Howland, Baker, and the Phoenix and Line Islands), he was so ill that he was barely conscious. One evening on Howland Island, after we had completed our evening chores of banding birds, we walked back to camp to go to bed. Shortly we noticed that Ralph was no longer among us. On a hunch I followed the beach onward from our camp on the west side and finally caught up with Ralph who was walking knee-deep in the water off the south end of the island. I

managed to get him turned around and back to camp, but I could never figure out quite what he had in mind. Needless to say, after this voyage Ralph rarely if ever again went on an island cruise and instead concentrated his field work on islands like Christmas Island, Johnston and Wake Atolls to which one could fly.

During the October voyage to the Line, Leeward, Howland and Baker Islands, I encountered my first truly wild areas, rarely and seldom inhabited even by highly adept Polynesians. There were remnants of residence on Howland Island, but this island, with only slight plant growth, was evidently too harsh even for natives who either fled the island or died there. On Howland, I got my chances to census the various boobies and tropicbirds, but an infestation of cats had much reduced any ground nesters and had eliminated them almost entirely from nearby Baker Island. Although not required by our employers, we began a campaign of eliminating cats from this and the other islands where they had become established. They could often be found fishing out on the reef and shot at night, but we found that simply running them down with a large stick in hand was the most effective method. The cats were fleet but lacked stamina and these desert islands gave them few places to hide. After a long pursuit, they would become tired, make higher and higher bounces in the air, and then essentially fall over on their sides allowing their easy dispatch. I had little taste for this activity and left it to others, especially the killing of kittens that we dug out of holes in the guano. Young Larry Huber, however, who was in splendid condition, responded when we pointed out a cat and said “Larry kill” — off he would go to the misfortune of yet another feline.

Howland was also the site of one of my first “adventures” in the Pacific. One night as we were out banding birds as usual, the ship started sounding all its horns, put on all its spotlights, and waved lights around wildly. Needless to say, every bird on the island headed out to sea, pretty much cutting short our work. Fred Sibley, trip leader, got on the radio telephone and learned that we were presumably in the path of an oncoming tidal wave that would arrive so soon that the ship would not have time to retrieve us from the island. We were told to climb to the top of the Amelia Earhardt day beacon (Fig. 7) and tie ourselves together with our belts, which we dutifully did with a



Figure 7. The Amelia Earhardt day beacon on Howland Island, March 1963.

(Photo A. B. Amerson, Jr.)

fair degree of anxiety. After quite a time had gone by nothing happened so we came down from the tower and went to sleep, never quite sure of what actually had been going on.

This would have been the end of it but when we went over to Baker Island, not far away, it happened again and once again we tied ourself to the top of a tower, this time the Baker Island Lighthouse, again with no results. We returned to Howland in November and it happened yet again. By this time it was getting to be a bit old hat and, as I was tired, after a short time I climbed down and went to sleep, telling the others not to wake me if I drowned. [Actually, by this time I was pretty sure that the meteorological experts on the ship were really not on their game.] I learned some years later that the water actually did rise during these events, some five feet or so, but because the islands were about 10 feet out of the water, we had been in no real danger.

Landings on the various islands were originally attempted with ships' boats, but the difficulty in landing on some of the small islands was so great that we later used 12-man rubber rafts. Some landings on the coral islands required considerable skill, because cross rips offshore, particularly at treacherous landings such as at Baker Island, often led to rafts capsizing with some hazard to the occupants and loss of valuable equipment. Doug Hackman, for example, once came up under a capsized raft and had a few tough moments before he was able to struggle free. As a consequence of this and other such adventures, one tended to look for the bosun who counted waves. I also made considerable attempts not to be on a raft run by an exmarine whose approach was "Banzai, go, full stream ahead", and who managed to flip boats on a regular basis.

Most of us surveying the islands had our primary backgrounds in ornithology but there were some who had been trained as mammalogists and a few as herpetologists. One of us, Bob Long, was our primary botanist and another, A. Binion Amerson, Jr., specialized in avian ectoparasites (cf., Amerson 1966, 1967, 1968; Amerson and Emerson 1971), but we had few with backgrounds in either botany or invertebrate zoology, surely to our loss. Few of us had Ph.D.s, primarily I suppose because the project was initially envisioned as lasting only 18 months with the result that few established ornithologists wanted to disrupt their employment elsewhere for short-term employment. As a result, many recent college graduates and some with masters' degrees ended up running and doing most of the work in the field, much to their benefit I am sure. On many trips our earlier five-man contingents consisted largely of ornithologists. One trip I remember vividly had three with primarily ornithological interests, one who was more interested in herpetology, and one who was a former jazz musician.

The supposed temporary nature of our engagement led to problems with obtaining results that could not have been foreseen. Because we thought every trip might be our last, we concentrated primarily on collecting, banding and censusing of birds as these endeavors would seem to produce the most information for the time spent. Little were we to know that the project would run for almost eight years with some areas receiving more than a dozen visits. Consequently, some marking studies that might have revealed considerable information on the various species were never conducted although much information was obtained from our island stations on Kure and Johnston Atolls (cf., Woodward 1972, Amerson and Shelton 1976).

The Pacific Ocean Biological Survey Project (POBSP) was the name under which we conducted our investigations. The older hands, however, remember that its original name, probably because it was essentially based out of the Bird Division of the Smithsonian, was originally the Pacific Ocean Ornithological Project. This name, after someone figured out the acronym, lasted but a short time and we formally buried POOP on Baker Island with appropriate honors.

Lawrence Nicholas Huber, who was my companion on a number of trips to the southern islands and to the Gilbert and Marshall Islands, was one of the most interesting persons I met on the Pacific Program. He had been raised largely by his mother, and, while by no means stupid, retained the naivete of a young child. This led him to engage in actions which led to what were known as the "Huber" stories in which Larry did something untoward and usually with unfortunate results.

I learned of the first of these from his own lips. While in school at the University of Arizona he had been collecting reptiles at which he was quite skilled. As he returned to school, he was pulled over by a state patrolman. I cannot remember why he did not simply tell the officer what he was doing, but I suspect he had animals in his possession that were illegal. To conceal his activities, he had hidden a recently caught, still live Gila Monster (*Heloderma suspectum*) under his arm. The warmth made the lizard more active, and not liking its captivity much, bit Huber under the arm. Now Gila Monsters are poisonous, if not lethal, and I gather Larry had a hard time of it for awhile.

He added to his reputation no little during the Pacific Program. During our trips to the southern islands onshore we largely ate C-Rations, a diet that led to digestive problems, probably because most had passed their expiration dates, sometimes by nearly a decade. Nonetheless a few items such as pound cake, which was almost edible, became great favorites. Larry managed to annoy the rest of us not inconsiderably by going immediately to the rations and picking out the pound cake before the rest of us had a chance. In retaliation one day, we emptied one of the pound-cake cans, put some stuff in it to get it to the proper weight, left a note inside and sealed it up not too well. Larry, of course, reacted predictably, and the expression on his face when he opened the can to read "Sorry, Booth Food Products" made our efforts well worthwhile.

Larry also had an inordinate fear of sharks, almost a phobia as it was so intense, and often had nightmares about trouble with sharks from which he awoke with considerable distress. It seems safe to say that he actually hated sharks. During the spring of 1964, as we conducted a survey of Palmyra Atoll, low tide allowed many

small 1-3 ft Black-tipped Sharks (*Carcharhinus melanopterus*) to cruise near our camp by the lagoon. Larry, seeing a chance to gain vengeance on his nemesis, made a small spear and proceeded to happily chase all the small sharks. We watched him with some bemusement as he splashed through the several foot-deep water, but then noticed a somewhat larger fin following him. Somehow, a ca. 8-ft shark was managing to follow Larry as he gave chase to the smaller sharks. We immediately began yelling and waving to Larry, who initially thought we were cheering him on and who continued chasing the smaller sharks. At one point he glanced over his shoulder, saw the larger shark, and virtually walked on water heading for shore.

During our trip to Marshall and Gilbert Islands in the fall of 1964, a trip ably led by A. Binion Amerson, Jr., we visited various islands, most of them inhabited. The hospitality of the natives was enormous; they usually insisted on roasting a pig on each of our visits. Indeed, one missionary told us our visit was the biggest event in the islands since the visit of Queen Elizabeth. Eventually, the partygoing was cutting significantly into our survey time so two of our members (truthfully, the least able collectors) were appointed the official party people so the rest of us could get on with the survey. This we did, collecting the few birds remaining after the natives had eaten most of the rest. It was clear to us that the natives knew their birds well because they were always pointing out Bristle-thighed Curlews (*Numenius tahitiensis*) for us to shoot, largely I believe because of their culinary properties. It was also most disconcerting to shoot a White Tern (*Gygis alba*) or Brown Noddy (*Anous stolidus*) from the top of the forest, and immediately have some small child come out of the undergrowth carrying it.

During this trip Larry (Fig. 8) was proudly carrying an enormous bowie knife (for sharks, perhaps?). On one of our first visits to an inhabited island, one of the natives took a sharp stick and with about three rapid strokes dehulled a coconut, opened the shell at one end and offered it to us to drink. Larry evidently envied our admiring comments of the resident's dexterity so he grabbed a coconut, held it against his chest, and proceeded to try to open the coconut with his knife. Predictably, the knife slipped off the coconut and stuck in his chest. Fortunately it managed to hit his breastbone and did little damage, leading once again for the rest of us to remark on his luck.

One of Huber's notions entailed constructing nesting sites for Red-tailed Tropicbirds (*Phaethon rubricauda*), which always attempt to nest under something, be it an old house as the 10 on Jarvis Island, or under coral rubble along the beach and, at worst, under a heavy tuft of beach grass. Huber made a considerable effort during one trip to implement his idea, actually a good one, by forming shelters in an inverted V with two slabs of coral rubble. These shelters were immediately and widely used by the tropicbirds and for many years later (Fig. 9).



Figure 8. Larry Huber preparing a frigatebird skin in 1964.



Figure 9. Red-tailed Tropicbird in Huber-built nest site on Jarvis Island in 1972.

guests. This trip included some of the most extraordinary collecting experiences of my life. Because Ely was unfamiliar with the islands and because he felt he should keep good relations with the crew, I was free to run off to the favored collecting spots, all of which I knew quite well from previous visits. At the overrun area of the airstrip on Sand Island, Midway I collected a Long-toed Stint (*Calidris subminuta*) on 25 August, on Southeast Island I collected a Ruff (*Philomachus pugnax*) on 28 August, and on Laysan Island I got two Baird's Sandpipers (*Calidris bairdii*) on 6 September.

The overrun area on Sand Island, when dry as in November 1980 (Fig. 10), usually holds nothing of interest. Following rain and the presence of small standing pools of water and during the peak of migration in the fall, it is one of the best places in Hawaii to find rare and vagrant shorebirds. Small wonder then that a Semipalmated Plover (*Charadrius semipalmatus*), previously unrecorded from the Northwestern Hawaiian Islands and the first specimen from Hawaii, and a Lesser Yellowlegs (*Tringa flavipes*), the second specimen from Hawaii, were also collected there by us in August 1967.

However, the most exciting night of collecting came on Lisianski on 4 September 1967. I had been going to bed somewhat earlier than the others as I had had



Figure 10. Overrun area at Sand Island, Midway Atoll, November 21, 1980.

My last Pacific Program survey of the Northwestern Hawaiian Islands came in August and September 1967 when I, accompanied by Charles A. Ely, then field director of the Program, and David I. Hoff visited several islands of the chain. It was unique in that our logistic support was three harbor tugs, which accompanied one another, presumably in case one ran into trouble. As a result, Ely and I were on one tug and David Hoff was on another, leaving the remaining one for any unexpected

my fill of banding Sooty Terns in preceding years, somewhat to their disgruntlement. After Chuck, my boss after all, mentioned making more of an effort, I spent most of one night banding birds around the island. As there was no moon I made a special effort to catch Bristle-thighed Curlews in the long grass on the southern portion of the island and was quite successful. Twenty-two years and some months later one of these birds was captured on Laysan Island to become the oldest known example of this species (Marks et al. 1990).

I continued north along the west shore to the northeastern coastline where I saw a small black petrel sitting among the Bonin Petrels (*Pterodroma hypoleuca*) in the vegetation along the beach crest. Not knowing what it was, I crept over, picked it up, and dispatched it, putting it in my bag until I got back to camp. But my night was not over. When I was about halfway down the east side of the island, where there is a rocky promontory stretching out somewhat from the island, I saw that the promontory was occupied by many roosting Ruddy Turnstones (*Arenaria interpres*) and Pacific Golden Plovers (*Pluvialis fulva*). As I walked down the beach towards them they exhibited their usual wariness, all fleeing to hide among the *Scaevola* at the top of the beach. However, a single shorebird remained with its head tucked under its wing. Struck by its anomalous behavior, I cautiously approached it, managed to get very close, and reached down and picked it up. It soon joined the petrel in my bag but when I got back to the camp neither Ely nor I knew what the birds were.

Once we got them back to the Smithsonian, it turned out that the petrel was Jouanin's Petrel (*Bulweria fallax*), a bird from the Arabian sea previously unrecorded in the Pacific (Clapp 1971), and the shorebird was a Mongolian Plover (*Charadrius mongolus*), the first recorded in the tropical Pacific east of the Marshall Islands (Clapp and Wirtz 1972). The records for Long-toed Stint (Clapp 1968) and Baird's Sandpiper (Woodward and Clapp 1969) were the first acceptable records for the Central Pacific but the Ruff, alas, was only the second specimen for Hawaii (I had collected one earlier on Kure Atoll in December 1963).

Most remarkably this is not the only instance of this petrel, from almost half a world away, occurring in the Northwestern Hawaiian Islands. Fifteen years and 11 months later (4 August 1993), U.S. Fish and Wildlife Service refuge personnel (Seto et al. 1996) found one calling from a burrow in a Bonin Petrel colony at Sand, Midway.

Most of our field work in the central Pacific was done from military vessels with much at-sea work done on the Navy's large YAG vessels. Many of the other surveys were conducted in the much smaller U.S. Army's navy, the ATF series of sea-going tugs. While the YAG's were large and even had staterooms for our use, the ATF's were small and cramped, and we shared bunks down below with the crew, not the most appetizing of places, particularly when one was near the oily stench of the engines. On other occasions we piggybacked on other organization's surveys, such as the U.S. Fish and Wildlife Service's surveys of the Northwestern Hawaiian Islands and the U.S. Coast Guard's annual inspections of the lighthouses and day beacons in the Phoenix Islands and American Samoa.

One of the problems with the ATFs was that we had no space in which to prepare our specimens. After considerable effort we ended up with a portable lab that sat on the fantail with the appearance of an enormous brown can of spam with a door at one end and a long work table along one side. I became acquainted with these labs while preparing specimens with Larry Huber during our cruise to the Gilbert and Marshall Islands. One warm night, as we were preparing skins and the others were off partying, a native arrived with a complimentary drink of fermented coconut milk (better not to ask how this is done). I had a little 6" tumbler from which I sipped as I worked on birds, thinking that the drink was surprisingly mild. What was more surprising was

my sudden fall from the stool to the lab floor as I sipped on my second tumbler. It was a tasty drink but considerably more potent than I originally thought.

My first experience of participating on another agency's voyage took place in March 1968 when I traveled up the Leeward Hawaiian Chain with Eugene Kridler, the first manager of the Northwestern Hawaiian Islands Refuge, Ernie Kosaka, John Sincock, and Karl W. Kenyon of Monk Seal fame (Fig 11). For some reason my brain was on hold prior to the trip for I simply forgot how unpleasant the trip had been in March 1965. When we got to Nihoa and saw that the Refuge sign way above the water and guyed with iron cables was gone, we were once again reminded of how non-Pacific these waters can sometimes be. Once again, I was freezing daily and were it not for the kindness of John Sincock, who lent me some additional clothes, the trip would have been most unpleasant. The others were primarily interested in surveys of the seals and turtles, but I spent much time once again surveying the seabirds, sometimes stopping to help with the census of endangered species such as the Laysan Teal (*Anas laysanensis*) and Laysan Finch (*Telespiza cantans*).

This trip featured the most exhausting collecting experience of my life. I was walking along the northwestern side of the runway at Tern Island, French Frigate Shoals on the warm and sunny afternoon of 14 March when I heard an odd "wicka-wicka-wicka" call which reminded me somewhat of a Northern Flicker (*Colaptes auratus*) call.



Figure 11. Clapp, thoroughly bundled up, Ernie Kosaka, John Sincock, and Eugene Kridler on the 1968 survey of the Northwestern Hawaiian Islands. (Photo Karl W. Kenyon)

As I approached the edge of the runway, I saw a large dark petrel sitting at the edge of a low clump of *Scaevola*. Unfortunately, it flushed. Knowing that Gene Kridler, as much biologist as preservationist, had brought a shotgun along, I ran most of the rest of the way back to our quarters to borrow the gun. It took me several minutes to catch my breath enough to ask for the gun. Once armed I tried to run back to the spot where I had seen the bird, but was breathing so hard I was reduced at last to a rather staggering walk. To my considerable disappointment, I saw no

trace of the bird and was about to leave when I heard the call once again and glanced up to see the bird headed right for me out of the sun. Now while I am a fairly skilled collector, I am a poor flight shot, but I shot anyway. To my surprise and delight the bird dropped almost at my feet. Later, I discovered that the bird was a Herald Petrel (*Pterodroma arminjoniana heraldica*), an uncommon visitor to North Pacific waters and, at the time, 950 miles north of the previous northernmost record (Amerson 1971).

In between our voyages or flights to various islands, we stayed at a hotel in Waikiki and amused ourselves by making surveys of offshore islands, counting and banding birds. It was on one of these visits I had met my once and onetime wife, Tina Abbott, who was then working as an illustrator at the Bernice P. Bishop Museum. On a

night survey to another islet, Moku Manu, we had to jump from our rafts, swim ashore, and grab a ledge. Although I am a fairly adequate swimmer, I missed the first pass and in the strong swell soon began to founder. Indeed, I was breathing in so much water that I could not even call for help. Fortunately for me, Warren B. King and Robert R. Fleet, both strong swimmers, noticed my plight and jumped back in to undoubtedly save my life. I was enormously grateful, once I finished upchucking a considerable volume of water. This was the third time in my life that I had nearly drowned, leading me to forever be a bit wary of water.

Moku Manu was also the island upon which we found a hybrid between the Blue-faced Booby and the Brown Booby (*Sula leucogaster*). Although I later caught, banded, and measured this bird, and another that I thought might be a back-cross hybrid, I never got around to formally writing up the record for publication, although the record was of a very rare hybrid, one that then had been only once recorded previously in the Philippines. As is often the case in hybrids between two forms that are not closely related, the hybridization occurred when there were few choices of breeding mates of the proper species. In this case, there was a single pair of Masked Boobies and many pairs of breeding Brown Boobies, while in the Philippines the reverse was true with many breeding Masked Boobies and very few Brown Boobies (Worcester 1911). This bird or another offspring of the mixed pair continued to be seen on Moku Manu for a number of years but I do not know if any hybrids still remain on this islet.

This visit also led me to being somewhat more craven than usual. While Huber and I had arrived and landed safely, when it was time to leave, the surf had risen considerably and I wasn't about to try to swim from the same islet where I had earlier nearly drowned. Consequently, we hid when our pickup boat came which eventually led to a Marine helicopter coming to pick us up. It landed in the middle of a Sooty Tern (*Sterna fuscata*) colony and the sound of rotors hitting birds sounded like a string of firecrackers popping. The Marines, if not quite panicked, knew this would not be a good situation and departed post haste. I received a few admonitions for this, and the Marines, one of whom said that the landing was scarier than anything he had faced in Vietnam, wanted nothing to do with us thereafter. On the other hand, I didn't drown.

One aspect of our work on the islands was the mass banding of enormous numbers of seabirds. Most of this was done at night with headlamps that provided an eerie sight as a line of bobbing lights proceeded through the darkness. Dark of the moon was the best time of course, and full moon the worst because many birds would flush if they could see to fly.

Most of the birds we banded were fairly small and relatively easy to handle, but the Blue-faced Booby (now known as Masked Booby), was another matter. The size of a small goose with a 5" bill (Fig. 12) with fish-knife serrations along the edge and a clamp like a vice grip, this booby was one that one grabbed with some care. The females, larger than the males and with a less yellow bill during the breeding season (Fig. 13), were more dangerous than the males but either could give you a nasty bite. Usually one would distract a bird with one hand, grab it by the neck with another, sling it between one's legs with the head to the rear, and pull up the leg to attach the band. The release was also done with some care because a mistake could leave an unpleasant

grip on the leg and perhaps elsewhere.

I am happy to say that in handling thousands of these boobies, I never made a really bad release, but Fred was less lucky. During one night of banding the significantly smaller Red-footed Boobies, which have particularly agile necks, he was grabbed in a sensitive area as we could all tell from the noise coming from his direction.



Figure 12. Dangerous end of large female Masked Booby, Christmas Island. April 15, 1988.



Figure 13. Pair of Masked Boobies on Christmas Island. April 21, 1988; the smaller male is on the right.

This is the first, and last, time I ever saw a Red-footed Booby dropkicked (and so far!), but Fred apparently recovered all right as he has a number of offspring.

The Masked Booby strike is very fast, however, and I learned how to capture them the hard way during my first visit to the Phoenix and Line Islands. By the time the two months of field work were completed, I had five major infections on one hand, four on the other, and my hands were swollen so badly that they flew me back from Canton, rather than let them fester during the six or seven days it would take to return to Honolulu. Other species caused little damage but could be seriously annoying, the worst of which were the Wedge-tailed Shearwaters (*Puffinus pacificus*). They continually battled the bander, trying to bite and claw so vigorously that one needed a glove with a gauntlet on one hand to keep one's wrist from turning red with scratches. This degree of aggressive behavior was characteristic of the Wedge-tailed Shearwater alone; other petrels such as the Christmas Shearwater (Fig. 14) were relatively docile and made little



Figure 14. Christmas Shearwater on Motu Upua Islet, Christmas Island. April 21, 1988.

fuss. Indeed, the White-throated Storm-Petrels (*Nesofregatta fuliginosa*) were so tame that they would sit unrestrained on your hand for a number of moments before deciding to take flight.

Because much of the banding activity was pretty much repetitive drudgery, stoop labor for the Pacific, we often made a contest out of who could band the most birds. One evening on Jarvis Island, I won the all-time [Masked] Booby grabbing competition

with a total of 566 to Fred's 560 with Larry trailing badly at 300 or so. I must admit, had I not run into this enormous club (roosting aggregation) where I banded more than 100 birds (some later found nearly 1,000 miles away on Howland Island), I would not have won as Fred was really more adept than I.

POST PROGRAM PACIFIC PEREGRINATIONS

In ensuing years I visited the Phoenix and Line Islands three more times, participated in two surveys of the Northwestern Hawaiian Islands, and joined surveys of pelagic birds in a variety of areas, but mostly north and east of Oahu and between Oahu and Johnston Atoll. During these years some of our touring was directly for the Smithsonian and other times in conjunction with other agencies. Some of these I remember more strongly. The trip to the Northwestern Hawaiian Islands in the spring of 1965 was memorable for the truly rotten weather as it rained off and on during most of the trip. It was raw and chilly and one could easily see why the young albatross, mostly birds of southern ocean islands, needed their dense plumage. We had to go out and reshore up our tents following many a windy and rainy night (Fig. 15), during one of which a tent finally collapsed drenching those within. During the day, much of our work as usual entailed banding and censusing birds (Fig. 16) and censusing Hawaiian Monk Seals (*Monachus schauinslandii*) and Green Turtles (*Chelonia japonica*) as well.



Figure 15. Setting up camp on Southeast Island, Pearl and Hermes Reef, March 1965. (Photo William O. Wirtz, II)



Figure 16. Clapp and Dennis Stadel banding Gray-backed Terns (*Sterna lunata*), on Lisianski Island, Northwestern Hawaiian Islands, March 1965. (Photo William O. Wirtz, II)

Some have criticized the Smithsonian Pacific Project because it was funded by the military and because we took blood samples from the birds (although such work was being done quite openly at the same time on the eastern shore of Maryland). Despite the occasional cloak of "secrecy", little that we did (and nothing that I know of) was certainly secret. Most of our work consisted of a wide-scale biological survey with attempts to obtain representative samples of vascular plants, fish, mammals, birds, reptiles, insects and other arthropods.

Following the Pacific Project, I accepted a job in the fall of 1970 with the U.S. Fish and Wildlife Service's Bird and Mammal Laboratory and spent much time curating the North American bird collection. I was also allowed to spend one day a week in completing a number of overview Atoll Research Bulletins on the fauna and flora of the Northwestern Hawaiian Islands (e.g. Ely and Clapp 1973, Amerson et al. 1974, Clapp and Wirtz 1975).

Field work, to my disappointment, became much more limited but I manage to revisit some of the southern islands in 1970 and 1971 as a consultant to the U.S. Air Force, which wanted to put some towers on these islands to track ballistic missile launches into Canton Atoll lagoon. My last visits to the southern islands were during the 1973 and 1975 joint Anglo/Smithsonian Expeditions.

Surveys in the 1970s were by helicopter from the base camp on Canton Atoll, a somewhat risky venture as some of the trips entailed a 500- mi round trip over the open ocean. In consequence the helicopters went in groups of three although only one was needed to carry passengers. I got in the habit of sitting near the door with a seat belt extending from the seat. This allowed me to lean out the door and take some decent aerial photographs of some of the islands (Figures 17, 18). During one of these surveys, the helicopter heeled over in a downdraft leaving me at a sort of 45-degree angle to the ground. I absentmindedly put one hand on the top of the door to stable myself while continuing to take photographs. When the helicopter leveled, I looked down and saw that I had completely stepped out of the seat belt meaning that only my fingertips had kept me from a sudden drop of ca. 500 feet to the ground. I quietly sat down, sweating considerably although it wasn't that hot, hoping that none had noticed. Unfortunately one of the crew had and they never let me near the door again.



Figure 17. Aerial view of Birnie Island. May 30, 1973.

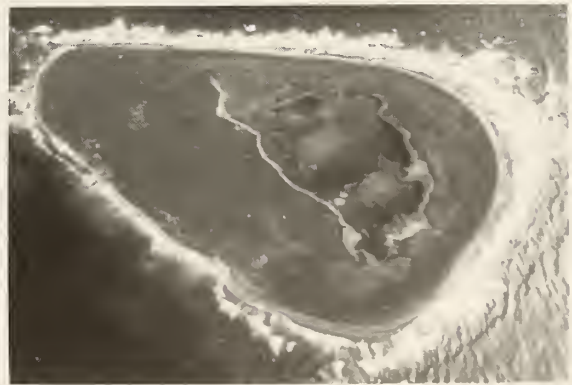


Figure 18. Aerial view of Phoenix Island. June 2, 1973.

My companions in 1975 were Ray Fosberg, whose encyclopedic knowledge of Pacific Islands was ever a source of inspiration, and David Stoddard, a British geographer who was both energetic and enterprising. I was considerably embarrassed when we visited Birnie Island which I had noted had but three species of vascular plants on its small surface (so small, in fact, that the Wilkes Expedition passed it up for nearby Enderbury Island seen in the distance.). Ray and David immediately walked almost

directly to a withered bush of *Sida fallax* which I had never seen on the island. I consoled myself that this was after the long drought that had occurred during the days of the Pacific Project and it could have easily been overlooked.

During the summer of 1978 I accompanied Elizabeth Cummings, then an assistant refuge manager, on another U. S. Fish and Wildlife Service survey of the Northwestern Hawaiian Islands — my only visit there at that season (Fig. 19). I ended up being a tourist guide for Liz (who is now my neighbor in Aldie, Virginia), but still managed a few observations of interest. During July, when the last of the Laysan (*Diomedea immutabilis*) and Black-footed (*Diomedea nigripes*) Albatrosses were fledging, the offshore waters filled with Tiger Sharks (*Galeocerdo cuvier*) that gathered to feed on uncautious young. One young Black-footed Albatross was particularly unfortunate. It apparently had a defect in its barbules making its feathers completely unable to support flight. Day-by-day it stood there, slowly starving to death, but I guess it eventually decided on suicide. I watched as it walked to the edge of the water, swam a few hundred yards out to sea, and was engulfed by a huge maw coming up from below, the tips of its wings being the last seen as the bird was dragged below.

Further field work in the Pacific occurred during the fall of 1979 when I organized an expedition to the northern Mariana Islands. The 1979 trip produced perhaps the greatest gaffe of my career. I had to spend many nights prior to the Marianas trip arranging for a yacht, the “Lion’s Den,” to take us up the chain and spent daylight hours doing my regular work. In consequence, when we arrived in the



Figure 19. Liz Cummings making the acquaintance of a nesting Red-footed Booby (*Sula sula*).

Marianas I had no time to familiarize myself with the specimens of the species that we might encounter. One day after arriving in Guam, Andy Ross and I went up to the reservoir to collect birds. To my amazement a large thrasher-like bird, which I did not know, flew up and began singing. Having recently read a paper by Ricklefs explaining why high altitudes were likely to produce new species, I thought that I had indeed found a new species and promptly collected the bird.

During the ensuing trip I mentioned it to the skipper who had previously told me he was happy to be one of the first (if not the first) to take a yacht into the Northern Marianas during the typhoon season (which was news to me). He included this “fact” in an article he later wrote for a local magazine. On our return to Guam, we once again went out collecting near the former Japanese airstrip at the northern end of the island. A bird with an unfamiliar silhouette popped up on a branch nearby, and with my reflexive collector’s instinct, it was immediately shot. When I walked over to pick it up, I realized it was the same species that I had shot near the reservoir about a month ago. Two and two suddenly came together and I instantly realized that I had not one, but two, examples of the endangered

Nightingale Reed Warbler (*Acrocephalus luscini*).

I immediately realized that I was in some trouble, particularly since I had already retrieved an endangered Micronesian Megapode (*Megapodius laperouse*), that had stumbled into a rattrap that Andy had set on Guguan. I did know this bird was endangered and dutifully released the chick from the trap whereupon it walked in a circle and fell on its side. I picked it up and set it upright and it did it once again. After several more repetitions of this behavior, I realized that it had become so badly damaged that it could not possibly survive and took it as a specimen considering that its value as a specimen was more important than the fact of my collecting it. On our return to Honolulu, I immediately called Ernie Kosaka who arranged for an agent to come over and pick up the specimens. Later, back in Washington, I received some hard-nosed interrogation by enforcement officials, but I managed to convince them that I was only an idiot and not a conscious malefactor.

Although we obtained a good series of small-bird specimens for the museum, relatively little was published based on this trip other than our discovery of a considerable range extension for the Gray-backed Tern (*Sterna lunata*) (Clapp and Hatch 1986) and the inclusion of some of our records in a paper reporting new distributional records for the Northern Mariana Islands (Glass et al. 1990).

The trip itself was not uneventful although the quarters where we recorded data and prepared specimens were quite cramped (Fig. 20). Most of the islands, volcanic peaks rising out of the sea, had poor or no anchorages leading to keeping a wary eye on weather conditions. At some stops, Danny the bosun or someone else actually dived to find some kind of fixed rock or ledge to which to attach the anchor, but often the boat just floated offshore while we conducted surveys on the islands. One day, as I sat on the



Figure 20. Andrew Ross and Clapp at work in the chartered yacht

fantail and Andy worked on herpetological specimens, an enormous green wave came rolling in and, although the yacht was well away from the island, we were being pulled up and driven towards the cliff. The captain called down to the bosun for “full astern” only to hear the reply “you know we don’t have any astern”. While sitting there wondering how long I had, and whether I would survive jumping off the back of the boat, the captain with considerable presence of mind called for “full ahead” which fortunately was

available. The captain pulled the boat in a circle as it slid toward the cliffs and we came close enough so that I could have hit the Black Noddies (*Anous minutus*) roosting there with a rock.

Although I was no longer primarily working on seabirds, I continued to do a fair amount of work on marine birds elsewhere. During the early 1970s, my skills as a rapid bander of seabirds were enlisted by John S. Weske to help him in banding creches of

Royal Terns (*Sterna maxima*) on the eastern shore of Virginia and North Carolina (Fig. 21).

My last voyage to the Northwestern Hawaiian Islands occurred in November 1980 under the auspices of the U.S. Fish and Wildlife Service. I was accompanied by Nancy Withers, who was interested in nudibranchs, her assistant Deborah Burns, and Eric (whose last name I have forgotten), whose job was to collect seal skat (Fig. 22). My companions were most pleasant and introduced me early in the trip to the “green



Figure 21. Banding Royal Terns on Fisherman’s Island, August 19, 1973.



Figure 22. Camp on Laysan Island. November 16, 1980, Nancy, Deb, and Eric, near entrance.

flash,” the point at which the setting sun turns an apple green just before it slips below the horizon. Like many others, I thought this was some strange legend, so I was nonplused when I saw it the very first night that my very much more experienced companions had predicted its occurrence. I was much pleased a number of years later to see a color photograph of this phenomenon on the cover of *Scientific American*.

On this trip I carried no armament but a camera. It was pretty late in the season though, so not many vagrants were present. Nonetheless, Tern Island had a pair of Mallards (*Anas platyrhynchos*) that had become relatively tame. Because there was virtually no standing fresh water, they took to using a child’s plastic wading pool as their choice for a swimming area (Fig. 23). Another uncommon bird present was a Cattle Egret (*Bubulcus ibis*), presumably from the population in the main Hawaiian Islands, not so tame as the Mallards, but still allowing a fairly close approach (Fig. 24).

Not surprisingly, once we got to Midway I headed for the overrun to find it dry – to my great disappointment (See Fig. 10 above). While we were on Sand Island, someone told us, though, that there were ducks in the island reservoirs. With Eric’s help we went to the reservoir and managed to obtain some vagrant ducks. (At that time the Navy was not as negative about shooting as was the U.S. Fish and Wildlife Service). Eric’s role was gradually to shoo the ducks towards a wall of the reservoir so we could use a long dip net to retrieve them. We ignored the various Northern Pintails (*Anas*



Figure 23. Female Mallard in wading pool on Tern Island, French Frigate Shoals. November 10, 1980.



Figure 24. Cattle Egret among the morning glories, Tern Island, French Frigate Shoals.

acuta), regular in the Northwestern Hawaiian Islands and by far the most common migrant duck there. We instead obtained on 22 November a fine adult male Bufflehead (*Bucephala clangula*, USNM 599951), a female and male Tufted Duck (*Aythya fuligula*, USNM 599952, 599953), perhaps the second most frequent migrant duck at Midway, and a male Black Scoter (*Melanitta nigra*, USNM 599954), the first record of that species from Hawaii and the tropical Pacific (Fig. 25).

The previous day I had made my first acquaintance with an adult Short-tailed Albatross (*Diomedea albatrus*), a species that usually lives on Torishima Island near Japan. Because it bore a band, I thought it worthwhile to catch the bird and send the band number to the banding office at Patuxent Wildlife Research Center. The Black-footed and Laysan Albatrosses have a relatively slow strike and I had previously



Figure 25. Room at the Bachelors' Officer Quarters (BOQ) with the catch of the day. November 22, 1980.

captured hundreds, if not thousands, of them with a slow grab to the neck. I tried the same on the Short-tailed Albatross, but forgot that it is much larger than the other two (Fig. 26). Although my grab was eminently successful, the Short-tail, because of its longer neck, had no difficulty reaching back and ripping the top of my arm with the hook of its bill. The resulting wound was spectacular, if superficial, and a sheet of blood poured off my arm. After reading the band, I released the bird and at the insistence of others went to the

nurse's office to get my really very minor wound treated.

During the latter part of the 1970's and early 1980's, I spent much time compiling three large treatises on marine birds of the southeastern United States and Gulf of Mexico (Clapp et al. 1982a, 1982b, 1983) and also aided in a compilation dealing with waterbirds of the New York Bight (Howe et al. 1979). I also participated



Figure 26. Short-tailed Albatross (left) and much smaller Laysan Albatross (right) on Sand Island, Midway Atoll, November 21, 1980.



Figure 27. Adult white morph Red-footed Booby on Little Cayman Island, January 26, 1986.



Figure 28. Adult dark morph Red-footed Booby on Little Cayman Island, January 23, 1986.



Figure 29. Adult variegated Red-footed Booby on Christmas Island, April 18, 1988.

in work on Sooty Terns with Bill Robertson during the springs of 1983 and 1984, in a Mike Irwin trip to study Common Terns (*Sterna hirundo*) in Trinidad in the winter of 1985 (c.f. Erwin et al. 1986) under the auspices of Patuxent National Wildlife Refuge, and in a winter 1986 survey of the Red-footed Booby colony on Little Cayman Island (Clapp 1987). The latter entailed mostly cutting transects through heavy vegetation to census the boobies. This was not without hazard as there were points of eroded limestone sticking up

like needles in depressions among the nesting trees, but I fortunately remained unpunctured despite a few close calls. The Red-footed Boobies here come in color phases like those in the tropical Pacific and the Northwestern Hawaiian Islands, but none have the variegated plumage of those found in the Phoenix Islands (Figs. 27-29).

My last trip to the Pacific came in 1988 when David W. Johnston and I went to Christmas Atoll in the Line Islands to survey populations of Bristle-thighed Curlews and to Kwajalein Atoll to make a survey of the ornithological resources there. The trip to Christmas Atoll was pretty much of a bust as only a few shorebirds remained there, most having already migrated north. I did enjoy my week there, though, obtaining a considerable number of pictures of the breeding seabirds as well as doing some surveys and banding (Fig. 30) in areas

then under study by Betty Anne Schreiber, Ralph Schreiber's widow.

The trip to Kwajalein was considerably more successful (Clapp 1990), although whether the military paid any attention to my comments on the ornithological importance of the various islets is unknown to me. Because the atoll has long been inhabited, the bird life is fairly sparse but occasional colonies remain on some of the more remote islands (Fig 31).



Figure 30. Banding a Masked Booby on Christmas Atoll. (Photo David W. Johnston)



Figure 31. Black Noddy (*Anous minutus*) colony on Enewetak Islet, Kwajalein Atoll. March 16, 1988.

RECENT YEARS

Over the years I have continued to work for the same organization at the Smithsonian through a number of title changes and affiliations (e.g. National Fish and Wildlife Laboratory, as part of the Biological Survey, and now as part of the Biological Resources Division of the U.S. Geological Survey). Nonetheless, my desk stands less than 10 feet from where it did nearly 40 years ago, although it now has two computers rather than a somewhat beat-up typewriter.

Beginning in the early 1990's, I began working as senior editor of a book on "The Breeding Birds of Virginia" which is to include maps of bird distribution based in part on the work of the Virginia Bird Atlas Project 1984-1989. Consequently, much of my work has been focused on passerines since then. During this period I have gradually assembled a string of over 800 birdhouses located in five northern Virginia counties and have amassed considerable information on the breeding biology of various species, much of which is being used in species accounts being prepared by myself and others. Despite my interest in this project, I still remember my years in the Pacific as the high point of my career, and only wish I could visit them once again.

While Central Pacific ornithology has produced much good material since the heydays of the Pacific Project, it seems unfortunate that the great mass of anecdotal and banding data from those years still, and perhaps forever, will remain unanalyzed. Some species-specific information has been summarized for the albatross (Sanger 1974a, 1974b), Wedge-tailed Shearwater (*Puffinus pacificus*) (King 1974), storm-petrels (Crossin 1974), Red-tailed Tropicbird (Gould et al. 1974), Sooty Tern (Gould 1974),

Blue-gray Noddy (*Procelsterna cerulea* (Rauzon et al. 1986) and the Gray-backed Tern (Mostello, et al., 2000) that incorporates data from the various islands studied, but most of these studies deal more with pelagic observations than those made on land. There still remains a great deal of information on the natural history, populations, distribution and movements of other species that remains unsynthesized. Four of the southern islands, Howland, Baker, and Jarvis Islands, and Palmyra Atoll, are now National Wildlife Refuges, the last designated quite recently. The Fish and Wildlife Service and I made post- Project visits to these islands and the data on these islands should be summarized for the benefit of future generations.

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Frederick M. Bayer, 1950. (Photo R. Tucker Abbott)

OCTOCORAL RESEARCH — PAST, PRESENT AND FUTURE

BY

FREDERICK M. BAYER

When I was a small boy of five or so in New Jersey, my mother chanced upon the cocoon of a moth one autumn day and brought it home to keep it until it emerged, so I could see its miraculous transformation from an inert bundle of tawny silk threads to a gorgeous cecropia moth. I now think it was that event that sparked a lifelong interest in natural history. Later, growing up as I did in Florida, I was surrounded by all sorts of natural wonders and as a teenager I began collecting seashells, native orchids, and butterflies.

Before long I concentrated on shells because they were beautiful, many kinds were plentiful around southern Florida, and several well known conchologists lived in the area. Among them was Maxwell Smith, who had a large private collection and was author of several books on native and worldwide seashells for collectors. The famous malacologist, Henry A. Pilsbry of the Academy of Natural Sciences of Philadelphia, often visited Maxwell and his friends the McGinty brothers during his winter vacations. They were extremely helpful to me and several other budding conchologists including Gil Voss, whom I later came to know on a professional basis. With their help I became familiar with the common marine species, and I constantly barraged Harald Rehder, one of the curators at the Smithsonian Institution, with requests for identification of specimens that the local experts couldn't identify. The director of the Florida State Museum at the time, who was an avid shell collector and in need of cheap help cataloguing his specimens, even hired me during summer vacations to curate his collection at the museum in Gainesville.

While collecting seashells I became aware of the sea fans and sea plumes that dominate reefs in the Florida Keys. In the summer of 1941 I took a course in marine biology taught by F. G. Walton Smith at the University of Miami, which provided a more comprehensive view of marine life (Fig 1a), and in the fall I enrolled as a junior at the university. I had seen a little of the wide variety of gorgonians that abound on the reefs of Florida, but I was just a beginning student of marine biology at the university and they were the farthest things from my mind to consider for a research project or a thesis subject. At that stage in my education I easily could have been guided into entomology or tropical botany had my professors been sufficiently inspiring. But the only dynamic and enthusiastic teacher I had was Walton Smith, assistant professor of zoology and instructor in all aspects of marine biology, himself a student of the eminent English embryologist E. W. MacBride, so my interests began to focus on marine mollusks. I had learned the rudiments of invertebrate zoology in my first two years of college in Palm Beach, and during my junior year, 1941-42, Walton Smith opened up new vistas in marine zoology.



Figure 1. (a) Summer course in marine biology, University of Miami, July 1941 (photo by J.W. Mayer, University of Miami). Seated around table, clockwise from lower left: Bill Sutcliffe, May Smith, Christine Stenstrom, Helen Tierney, F.G. Walton Smith; standing l. to r., Herman Doochin, F.M. Bayer, Naomi Fork; (b) Drawing a Moorish Idol fish at Biak Island, April 13, 1945; (c) Elisabeth Deichmann at U.S. National Museum, 1954.

December 7 of that year changed many plans, mine included. I knew that military service inevitably would interrupt or terminate my college education. When the director of the Florida State Museum in Gainesville invited me to join the staff until I had to go into the military, I accepted my first full-time job. In less than a year I was drafted into the Army Air Corps and trained as a photographic technician, followed by an overseas assignment that took me to New Guinea, the Philippines and Okinawa. This provided an opportunity to collect mollusks and butterflies in the tropical Pacific. Personal acquaintance with exotic fauna and flora intensified my interest in natural history as a profession, although I really wasn't fully aware of it. My comrades-in-arms regarded me as something of a freak because I spent much of my free time on the reef collecting shells or in the nearby jungle catching butterflies. The only method of preservation that was practical under the circumstances was drying, so I made drawings of some of the items I couldn't preserve as dried specimens (Fig. 1b). The mollusks that I collected are now incorporated in the collections of the Department of Systematic Biology in the National Museum of Natural History.

Unlike many of my classmates I survived the war and was able to resume studies at the University of Miami. By then, Walton Smith had succeeded in establishing the Marine Laboratory of the University of Miami. When my interest in mollusks showed signs of persisting after the war, Walton advised me to look for a different subject to pursue. He had little time for shell collectors. In his opinion, shell collecting was for spinsters in the change of life and little old ladies in tennis shoes, not for the serious student of marine biology. About that time Walton was cultivating John Wentworth and his wife, a rich couple on Miami Beach who owned a powerful motor-yacht. They used it to dredge for shells and other marine creatures in the Straits of Florida and around the Bahama Islands. Among the specimens they collected were several resembling sea fans but quite unlike anything that grew on the reefs. Recognizing Walton's influence on my continuing education, I prudently decided to embark on an investigation of Alcyonaria. The only available comprehensive reference on the western Atlantic fauna was Deichmann's *Alcyonaria of the Western Part of the Atlantic Ocean* published in 1936 (Fig. 1c), which was based upon a huge unpublished monograph begun many years before by A. E. Verrill (Fig. 2a), the American pioneer in coral research. I quickly discovered that the Alcyonaria, now generally called Octocorallia because of the invariable octoradial symmetry of their polyps, are a taxonomic challenge even greater than the mollusks.

I didn't have time to get very far in my study of the gorgonians dredged up by the Wentworths, because I was invited by Waldo Schmitt, head curator of biology at the Smithsonian Institution, to apply for a position as assistant curator on the staff of the U.S. National Museum. As there were few candidates even moderately qualified so soon after the war, I was awarded the position. Dr. Schmitt, who was a specialist on Crustacea, had hoped to develop a new crustacean specialist for the zoological staff, but I persisted in working with alcyonarians. I imagine I was a major disappointment to him, although he never said so. As understanding and tolerant as he was, Dr. Schmitt did not pressure me to abandon my investigations on octocorals in favor of crustaceans. In my favor was the fact that the museum had a large collection of octocorals and other coelenterates but never had had a specialist to study them. Specimens always had been identified by collaborating specialists in other institutions, including A. E. Verrill, C. C. Nutting and Elisabeth Deichmann.



Figure 2. (a) Addison E. Verrill; (b) Elisabeth Deichmann and Frederick M. Bayer, Washington D.C., photo by L.B. Holthuis; (c) Kumao Kinoshita, Tokyo, about 1910; (d) Charles Cleveland Nutting, State University of Iowa.

Not long after I arrived at the U.S. National Museum in 1947, the Division of Marine Invertebrates was visited by Elisabeth Deichmann, curator of invertebrates at the Museum of Comparative Zoology of Harvard University, and the author of that vast monograph I had struggled with earlier in my efforts to identify gorgonians in Miami (Fig. 2b). Thus began a professional relationship and personal friendship that lasted the rest of Liska Deichmann's life. Eventually she became the external adviser and examiner of my doctoral dissertation, although officially Waldo Schmitt was my dissertation supervisor and advocate at my oral defense.

My first field assignment at the museum was to join an expedition to Bikini Atoll to survey the results of the atomic bomb tests that had taken place in 1946. My colleagues on the Scientific Resurvey of Bikini included Leonard Schultz, curator of fishes; J. P. E. Morrison, associate curator of mollusks; John Wells, professor of geology at Cornell University; and Harry Ladd and Joshua Tracey of the U. S. Geological Survey.

My immediate supervisor at the museum, Fenner Chace, was studying two genera of crabs that live in commensal association with stony corals, so I spent most of my time sloshing around on the reef flat with John Wells, collecting reef corals and documenting the crabs associated with them. It had been observed that the various species of crabs in the genus *Trapezia* differed in their color patterns. Having no camera but modest skills as a water-colorist, I took the opportunity to make sketches in life colors of the various color patterns of the specimens that I collected.

As I was not a skilled skin diver, I had no opportunity to collect alcyonarians other than a number of massive soft corals from the reef flat and back-reef trough. A Navy landing craft fitted for dredging, LCI 615, under the direction of R. Dana Russell, conducted several stations that obtained a number of interesting gorgonians. These formed the basis for my first published effort in the field of alcyonarian taxonomy.

Knowledge of the gorgonians of the Indo-west Pacific is scattered through the scientific reports of various expeditions beginning with the French Voyage of the *Astrolabe* 1826-29 under Dumont-Durville (Quoy & Gaimard 1833) and the *Vénus* 1836-39 under Dupetit-Thouars (Valenciennes 1846), which obtained only a few octocorals. The U.S. Exploring Expedition 1838-42 under Charles Wilkes obtained enough material for J. D. Dana (1846) to produce a comprehensive account of the Alcyonaria collected during the expedition along with species previously known. Dana reported the first specimen of precious coral, genus *Corallium*, collected in the Pacific, giving it the appropriate name *Corallium secundum*. Dana referred a problematic specimen to the fossil genus *Aulopora*, naming it *Aulopora tenuis* and classifying it in the family Tubiporidae because of its bright red skeleton.

Milne Edwards and Haime (1857) significantly advanced taxonomic knowledge of coelenterates in their *Histoire Naturelle des Coralliaires*, a comprehensive account of all anthozoans together with hydrocorals such as *Millepora* and *Stylaster*. This three-volume work, illustrated by an atlas of 31 plates, substantially extended Dana's treatment of zoophytes by including many species unknown to Dana as well as fossil species, which were not included in Dana's report for the U.S. Exploring Expedition. Volume 1 of this landmark work, devoted to the alcyonarians, antipatharians and zoantharians, incorporated for the first time Valenciennes' discovery of sclerites in the classification of alcyonarians. Volumes 2 and 3 were devoted to scleractinians and hydrocorals.

Several other authors reported on collections of more or less limited scope. Those geographically most pertinent to the Marshall Islands were by Kinoshita (Fig. 2c) (1907,

1908, 1909, 1910, 1913) on Japanese material, Nutting on Hawaiian and Japanese specimens (1908, 1912), and Versluys (1902, 1906, 1907) and Nutting (1910, 1911) (Fig. 2d) on the collections assembled by the Dutch *Siboga* Expedition in Indonesia. More cosmopolitan collections obtained during expeditions aboard H.M.S. *Challenger* (Wright & Studer 1889) and *Valdivia* (Kükenthal 1906, 1919; Kükenthal & Broch 1911) also could not be ignored in the context of that time.

With the help of this and other pertinent literature, I distinguished 12 species collected at Bikini, 4 of them new and one of them with a new “forma” (Bayer 1949). It was presumptuous for an inexperienced would-be taxonomist familiar only with gorgonians of the western Atlantic to describe and report on specimens from a vastly different faunal region. How many of the new species and new records reported in that paper will stand the test of time remains to be seen, but it now is clear that one of those new species probably is identical with a Japanese species described as new in 1906 by Versluys and again in 1908 by Kinoshita (Bayer 1982). The literature consulted in preparing this paper clearly demonstrated the superficiality and inadequacy of illustrations of taxonomic characters, and I resolved not to perpetuate that mistake. The only advance in octocoral taxonomy to which I can lay legitimate claim is the quality of illustrations that accompany descriptions of species even if not established as new.

About that time, Ray Moore of the University of Kansas invited me to contribute a chapter for Part F of the newly established series *Treatise on Invertebrate Paleontology*. For that volume Ray Moore retained the name “Coelenterata” for the phylum, contrary to Libbie Hyman’s preference for “Cnidaria.” In it I defined and illustrated all the genera recognized at the time and replaced Dana’s name “Alcyonaria” with Haeckel’s term “Octocorallia” (Bayer 1956).

Beginning in 1950, the Office of Naval Research sponsored a program of Scientific Investigations of Micronesia (SIM) administered through the Pacific Science Board of The National Academy of Sciences. SIM conducted a series of expeditions to Pacific atolls, including Arno in the Marshall Islands, Onotoa in the Gilbert and Ellice Islands, Raroia in the Tuamotus, Ifaluk in the Caroline Islands, and Kapingamarangi, one of the southernmost Carolines. The Pacific Science Board established the Atoll Research Bulletin as a vehicle to publish the results of these expeditions and information about atolls and coral reefs in general. Beginning in 1950 under the editorship of F. R. Fosberg, the Bulletin has now reached its golden anniversary under the editorship of Ian G. Macintyre.

Relatively few octocorals were collected during the SIM expeditions, but as a member of the team that surveyed Ifaluk in 1953 (Fig. 3a) I obtained several specimens of a very unusual gorgonian that I later described as a new species, genus, and family of Holaxonia (now placed in Calcaxonia) called *Ifalukella yanii*, family Ifalukellidae. As I was already halfway around the world, I returned to Washington via Japan, where I hoped to find the type specimens described by Kinoshita. A young Japanese foreign service officer who had translated some of Kinoshita’s papers in Japanese while a student at Georgetown University, now back in Tokyo, was of immeasurable assistance in my search. I was partly successful in that goal but, more important, two of my Japanese colleagues, Dr. Itiro Tomiyama and Dr. Tokiharu Abe, who were official research collaborators for Emperor Showa, arranged for us to visit the Emperor’s biological laboratory, which contained a great many specimens for the most part personally collected. Although interested in biology in general, the Emperor’s serious research



Figure 3. (a) SIM Expedition to Ifaluk Atoll, Caroline Islands, 1953. Standing (l. to r.) Ted Arnow, U.S. Geological Survey; Donald P. Abbott, Stanford University; Joshua I. Tracey, U.S. Geological Survey; seated (l. to r.) Robert R. Harry (now Rofen), Stanford University; Frederick M. Bayer, Smithsonian Institution; (b) Octocoral (*Sarcophyton*) photographed underwater at Palau, 1955; (c) Photographing reef fauna underwater at Palau Islands, 1955.

interest was on hydroids. After touring the laboratory we were invited to a private audience with the Emperor. He was aware of the large collection of corals in the Smithsonian and wished to know more about it. It was an extraordinary and unforgettable meeting for an obscure American biologist and a young Japanese foreign service officer. However, in Washington after the fact it was considered a breach of diplomatic protocol: it was not proper for an employee of the U.S. Government to meet a foreign head of state without first notifying the State Department. An unexpected result of that event was several subsequent meetings with the Emperor during later visits to Japan (also without prior approval of the State Department), and he visited my department and my lab during his official state visit in 1975 (Fig. 4).



Figure 4. Emperor Showa examining specimens in Department of Invertebrate Zoology, Smithsonian Institution, October 2, 1975. With F.M. Bayer, Joseph Rosewater, and Prof. H. Sato, interpreter.

Also with support from the Office of Naval Research, the George Vanderbilt Foundation at Stanford University sent an expedition to the Palau Islands in 1955 to collect and study the marine fauna (Fig. 3b,c) (Bayer & Harry-Rofen 1957). I was able to obtain a substantial collection of octocorals, including three specimens of Dana's *Aulopora tenuis* (now *Cyathopodium tenue*), an inconspicuous but highly interesting species that had not been collected since Dana described it in 1846 (Bayer 1981).

P. Wagenaar Hummelinck, a Dutch zoologist who collected extensively in the Caribbean islands, sent me his large collection of gorgonians from the Netherlands Antilles for identification. To determine the species in the Hummelinck collection it was essential to resolve the many taxonomic problems in the existing literature on that fauna.

Because travel to foreign museums to examine type specimens was not feasible in those days, it was necessary to evaluate the numerous species from their cursory and often unillustrated descriptions. Verrill had obtained slide preparations of a few type specimens in European museums, and Liska Deichmann herself had seen and made notes on other specimens during visits to Europe. Working with this shaky foundation, often in consultation with Liska during her many visits to Washington, I produced a report on the Hummelinck collection supplemented by previous and new collections of the U.S. National Museum and Museum of Comparative Zoology that satisfied both Dr. Hummelinck and my dissertation committee at The George Washington University. After three years in press, it was published (Bayer 1961/62) both as a trade book by Martinus Nijhoff in The Hague, and as volume 12 in the series *Studies on the Fauna of Curaçao and Other Caribbean Islands*, identical except for the preliminary leaves and page 1. In the interests of bibliographic and nomenclatural accuracy, I should point out that the official copyright date of the trade book is 1961. Because the journal version published by Natuurwetenschappelijke Studiekring voor Suriname en de Nederlandse Antillen was not distributed until January 1962, new taxa and nomenclatural acts date from the trade book issued in 1961.

That work covers the shallow-water fauna of the same geographical area considered in Deichmann's report on the deep-water fauna. Although subsequent collections have revealed all too clearly its inadequacies, it has remained for four decades the only taxonomic work on octocorals of warm western Atlantic waters. Many specimens later obtained by scuba diving on deep reef slopes, as well as collections from previously unsampled localities, are difficult to identify with any certainty and suggest a degree of individual, ecological, bathymetric and geographic variation that I could not take into account.

Although I divided the huge number of specimens examined during preparation of my paper ("monograph" is too pretentious) into 101 species, they now clearly reveal that not all specimens of any given species are exactly alike. Too many are "just a little different" from the diagnoses of the species; they don't fit neatly into the preconceived notion of the species. This suggests that morphological variation might be induced by environmental factors. An opportunity arose to join the faculty of my old alma mater and I thought it would provide opportunities to study living populations of gorgonians in different habitats to determine how species vary. Early in 1962 I left my position at the museum to become a professor of marine science at the University of Miami with plans to tackle the problem of ecological variation through experimentation in the field. I embarked upon an ambitious and, as it turned out, unrealistic program of transplanting parts of colonies from one habitat to another. This involved cementing them on concrete tiles and installing them in areas of the reef where water currents differed from those of the reef where the colonies grew originally. I had not reckoned with the inquisitive nature of skin divers, and as rapidly as I established a series of tiles for transplant experiments they were disturbed or removed by persons unknown.

Under the direction of Gilbert Voss, the University of Miami began a research project in deep-sea biology involving a program of trawling in the Straits of Florida and Caribbean Sea using a North Sea-type fishing trawler, *Gerda*, outfitted for scientific dredging and trawling. This program expanded substantially with the acquisition of oceangoing research vessels, first the *John Elliott Pillsbury* (Fig. 5), later the *James M. Gilliss* and then the *Columbus Iselin*. Operations from these vessels obtained vast



Figure 5. R/V *John Elliott Pillsbury* about 1965.

numbers of marine animals from the tropical western Atlantic, the Bay of Panama, and the coast of West Africa, including a rich collection of octocorals as well as other invertebrates and fishes. Operations aboard R/V *Gerda* in 1963 obtained the first records of the precious coral genus *Corallium* in the tropical western Atlantic, as the new species *Corallium vanderbilti* reported by Boone (1933) was based upon a specimen of *Dodogorgia nodulifera* (Hargitt), a species with gross aspect deceptively similar to *Corallium* but lacking the stony axis characteristic of that genus (Bayer 1964).

The variety of gorgonians and other octocorals we collected in the western Atlantic was far greater than Deichmann had included in her monograph. With new collections arriving after every cruise it was impossible to keep up with new discoveries, so I confined my taxonomic work on octocorals to investigation of material from Antarctic waters obtained by naval operations in the 1960s and transmitted by overseeing agencies including Stanford University and the Smithsonian Oceanographic Sorting Center.

I had not bargained for the amount of time required to meet teaching commitments in an academic environment, and to apply for grants from funding agencies to support research. Neither was I prepared for the complications involved in doing taxonomic research away from comparative collections. Over a period of more than 10 years I was able to publish only two short papers on gorgonians, an unacceptable level of productivity. Knowing that taxonomic work depends upon comprehensive research collections not present in Miami, I had maintained an association with the museum through an appointment as Smithsonian research associate. During 1971 and 1972 I took

a year's leave of absence from the University to occupy a visiting curatorship at the Smithsonian Institution that allowed me to make some inroads on western Atlantic octocorals from the Miami collections as well as some Antarctic material, and to lay the groundwork for a permanent return to the Smithsonian. In 1975 I rejoined the museum staff in a temporary curatorship later made permanent by the museum director at the time, Porter Kier, himself an accomplished taxonomist who worked on living and fossil echinoderms.

The discovery of biologically important compounds in octocorals, such as the prostaglandins reported by chemists at the University of Oklahoma (Weinheimer & Spraggins 1969), intensified biochemical investigation of these widespread marine animals and dramatically increased the demand for identification of species under study. This increased demand has far outstripped the capacity of the few active taxonomists to carry the load. In 1950, the *Zoological Record* reported 7 taxonomic papers by 3 authors, none dealing with biochemistry. In 1970 the *Zoological Record* reported 6 papers by 6 authors or author-teams on the biochemistry of octocorals, but only 7 taxonomic papers by 5 authors. In the twelve-month period spanning 1997-98, 30 authors and author-teams published 45 papers on biochemistry; but only 10 authors published 11 taxonomic papers during the same period.

In 1980 I was awarded a Senior Queen's Fellowship to Australia, where I was a guest at the Roche Research Institute of Marine Pharmacology at Dee Why, near Sydney. The Roche Research Institute was interested in the identification of octocorals for possible exploitation of their bioactive compounds. While at Roche, Phil Alderslade and I studied hundreds of specimens of gorgonians of the families Melithaeidae and Paramuriceidae, both major components of the octocoral fauna of the Great Barrier Reef. Six genera of Melithaeidae and about 22 of Paramuriceidae are accepted as valid, most with numerous nominal species common around Australia. Although we found groups of "species" roughly equivalent to the known genera in both families, it was virtually impossible to detect significant morphological discontinuities for separating genera, and the traditional characters used in the delimitation of species seemed to merge imperceptibly. We concluded that specimens in those two families formed what amounted to morphological continua without tangible discontinuities that could be used to delimit "species" consistently. Neither geographic nor bathymetric data for the specimens yielded information that would help in recognizing species. Some specimens corresponded nicely with species described in the literature, but in most cases they intergraded with adjacent species to such an extent that it was impossible to draw a definite line of demarcation.

An important outcome of my stay at Roche was the decision to hold an international workshop on Octocorallia in 1981 at the Australian Institute of Marine Science at Townsville (Bayer 1982). One tangible result of that workshop, attended by Jaap Verseveldt (The Netherlands), Manfred Grasshoff (Germany), Steven Weinberg (The Netherlands), Philip N. Alderslade and John Coll (Australia), Katherine M. Muzik (USA and Japan), Shohei Shirai (Japan) and others (Fig. 6), was the first draft of a trilingual glossary of anatomical terms used in the description of octocorals, which eventually was published in the Netherlands (Bayer, Grasshoff & Verseveldt, eds. 1983).

Recognizing the need for increased expertise in identifying octocorals, UNESCO sponsored the First Octocoral Research Workshop and Advanced Training Course at Phuket, Thailand, November 30 to December 13, 1987. Under the guidance of Philip



Figure 6. International Workshop on Octocorallia, September 1981, Australian Institute of Marine Science, Townsville, Queensland. Standing, l. to r., M. Mahendran (Sri Lanka), Stephane LaBarre (Australia), P.N. Alderslade (Australia), Manfred Grasshoff (Germany), Shohei Shirai (Japan). Seated, l. to r.: J. Verseveldt (The Netherlands), Katherine Muzik (U.S.A and Japan), F.M. Bayer (U.S.A.), Zena Dinesen (Australia), Steven Weinberg (The Netherlands).

Alderslade, Y. Benayahu, and Katherine Muzik, participants from Indonesia, Malaysia, Papua New Guinea, the Philippines, Sri Lanka and Thailand learned methods of collecting, preserving, and curating octocorals, as well as the basic techniques of taxonomic identification. One of the conclusions of that workshop was the often-repeated observation that Southeast Asia, like the rest of the world, is lacking in trained personnel and basic research in the field of octocoral taxonomy.

In the absence of a comprehensive monograph of the world octocoral fauna, biochemists and taxonomists in many parts of the world continue to experience great difficulty in determining species of octocorals. The number of species recorded in the literature is too great to be confirmed, defined and illustrated in a comprehensive, reliable treatment consistent with modern taxonomic procedures.

The first monograph of worldwide “zoophytes,” the category that included octocorals, was published more than two centuries ago by Peter Simon Pallas (1766), who reported a total of 31 species of octocorals along with the sponges, hydroids, stony corals, actinians, bryozoans, and “ambiguous genera” that included parasitic worms, *Volvox*, and coralline algae. Lamarck (1816) increased the number to 124 species, and a treatise by Lamouroux (1816), confined to the “polypiers coralligènes flexibles,” included 163 nominal species of octocorals.

In a work on the corals of the Red Sea, Ehrenberg (1834) proposed a dual classification distributing the zoophytes between two orders, Zoocorallia for soft-bodied or unattached forms divided into three Tribes, and Phytocorallia for attached forms

divided into seven Tribes. In this ingenious but unnatural system, octocorals were split between Zoocorallia (the Pennatulacea and Alcyonacea) and Phytocorallia (all of the Gorgonacea, with *Heliopora* grouped with *Millepora*).

The most significant advance in octocoral classification was made by Valenciennes (1855) in the abstract of a larger, more detailed monograph that never was published. Although John Ellis (1755) published the first--and remarkably accurate--drawings of the "Figures of hollow Crosses" in the cortex of the scleraxonian *Corallium rubrum* (Fig. 7), Valenciennes was the first to observe that these calcareous structures, which he called "sclerites," in the soft tissue of various gorgonians differed in shape from species to species. He established several genera of octocorals on the basis of such differences, many of which are still recognized today.

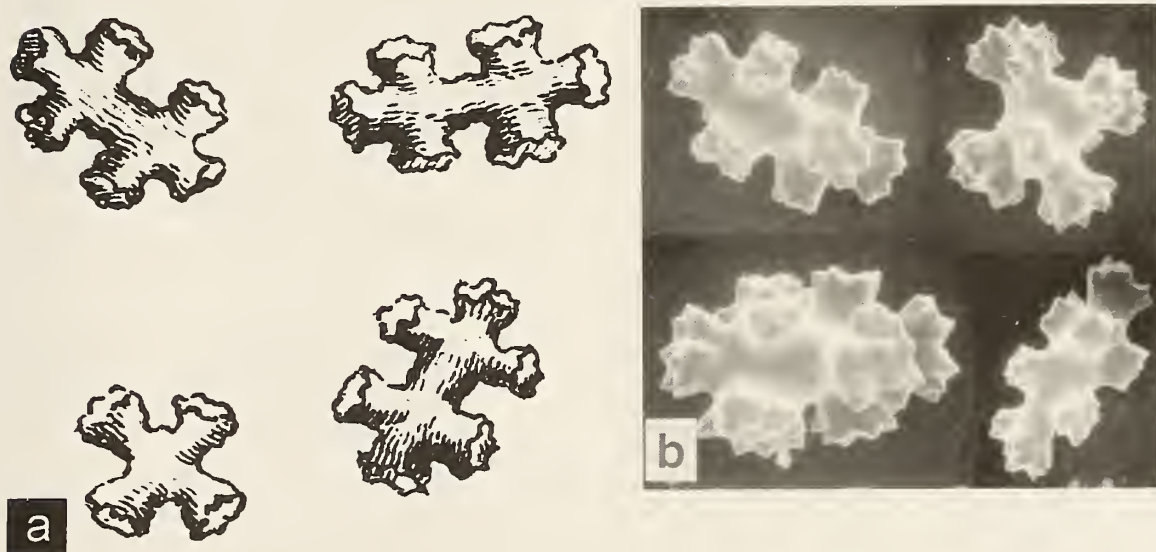


Figure 7. (a) Sclerites of *Corallium rubrum* as drawn by John Ellis, 1755; (b) same as photographed under scanning electron microscope at Smithsonian Institution, 1994.

Although Milne Edwards and Haime (1857) incorporated Valenciennes' observations on sclerites in their classification of octocorals in which they recognized 197 species based for the first time upon objective criteria and beginning the "modern" era of octocoral taxonomy, it remained for Kölliker (1865) to publish illustrations of many of the kinds of sclerites described by Valenciennes. Since that time, sclerites have become a principal character for the description, identification and classification of octocorals.

Recognition of sclerites as an important feature for defining and identifying octocorals by no means minimized the need for accurate depiction of colonial form. Today we can look back with admiration at illustrations of octocoral colonies produced by naturalists before the taxonomic value of sclerites was discovered. The magnificent encyclopedia of "Pflanzenziere" (Esper 1788-1830), containing hundreds of hand-colored engravings, illustrated for the first time colonies of many key species of gorgonians. The exquisite engravings of the few gorgonians collected by the French vessel *Vénus* (Valenciennes 1846) are meticulously accurate and are unequaled even by present standards.

By the end of the nineteenth century the number of octocorals had become so unmanageable that monographs on a worldwide scale were impractical. Major contributions to knowledge of octocorals were contained in the reports of major scientific expeditions such as that of H.M.S. *Challenger* (Wright & Studer 1889), R.I.M.S. *Investigator* (Thomson & Henderson 1906; Thomson & Simpson 1909), the Dutch research ship *Siboga* (Versluys 1902, 1906, 1907; Nutting 1910, 1911; Stiasny 1935, 1937) (Fig. 8a), and several smaller expeditions in various parts of the world.

Recognizing a need for an overall revision of the octocorals known so far, Kükenthal began a series “Versuch einer Revision der Alcyonaceen” (1902, 1903, 1905, 1907), which he never completed, although he published a number of shorter papers giving his ideas of the “System und Stammesgeschichte” of individual families, and a new classification of octocorals (1921). Nine years later, Hickson (1930) published his own classification of the Alcyonaria.

Kükenthal (1906, 1919), together with a few of his students and his Norwegian colleague Hjalmar Broch (Kükenthal & Broch 1911), published a comprehensive account of the octocorals collected by the steamer *Valdivia* during the German Deep-sea Expedition 1898-99. His summaries of the known genera and species of Pennatulacea (Kükenthal 1915) and Gorgonacea (Kükenthal 1924) are the nearest things to monographs of major octocoral taxa available today. Kükenthal recognized a world total of 141 certain and 134 doubtful species of Pennatulacea, and 805 certain and 255 doubtful species of Gorgonacea. A substantial number of species have been described in subsequent years, but 1,500 species would be a conservative total for those two orders of Octocorallia. Assuming a similar number of species for the soft corals and stoloniferans, which have not been summarized as Kükenthal did for the gorgonians and sea-pens, the number of valid species of octocorals could reasonably be estimated at about 3,000.

Regional monographs have been published recently for some geographical areas for which collections are reasonably complete (New Caledonia: Grasshoff 1999; Red Sea: Grasshoff 2000; Mediterranean: Carpine & Grasshoff 1975, and Weinberg 1976, 1977, 1978; eastern Atlantic: Grasshoff 1992). However, such regional works have an inherent limitation that can be underestimated if they are used to identify specimens from other geographical regions. Years ago, one novice complained to me that Deichmann’s monograph of western Atlantic Alcyonaria resulted in incorrect identifications of specimens from the Pacific coast of the Americas.

Current literature is adequate for the reliable identification of most octocoral species of shallow and moderate depths in the Mediterranean and eastern Atlantic, but specimens from deep water still may be difficult to determine. To a lesser extent the same is true of the western Atlantic, although the only account of the shallow-water fauna is seriously out of date. Octocorals of the eastern Pacific, the Indo-west Pacific, and Southern Ocean present a greater problem. The only comprehensive treatment of the shallow-water octocorals of the Pacific coast of the Americas is in serious need of revision (Verrill 1868-69), and the deep-water fauna has been investigated only along the coast of California and that in a very superficial and inadequate way (Nutting 1909). Shallow-water species from this entire region can be identified, but with doubtful accuracy, while deep-water specimens remain problematic.

Octocorals are poorly represented in the shallow waters of the Hawaiian Islands, and the deep-water species have been reported incompletely (Nutting 1908; Bayer 1952, 1956). Some species of this fauna can be identified with reasonable reliability, but others



Figure 8. (a) Gustav Stiasny at Rijksmuseum van Natuurlijke Historie, Leiden; (b) Jakob ("Jaap") Verseveldt at Zwolle, The Netherlands, 1984; (c) Philip Alderslade at Townsville, Australia, 1981; (d) Manfred Grasshoff at Townsville, Australia, 1981.

are inadequately known.

The vast expanse of the western Pacific is a veritable *terra incognita* of octocorals except for the principal soft coral genera that have been revised by Verseveldt (1980, 1982, 1983, 1988) (Fig. 8b) and the gorgonacean family Isididae (Alderslade 1998) (Fig. 8c). Otherwise, isolated specimens can be identified only by reference to widely scattered reports that leave many questions unanswered. Much Japanese material now can be identified with some degree of certainty, as the octocoral fauna has been investigated by several authors (Kükenthal 1908, 1909, 1910), Kinoshita 1908, 1909, 1910, 1913; Nutting 1912; Balss 1910; Aurivillius 1931; Utinomi 1952, 1954, 1957, 1960, 1961, 1966, 1976, 1977, 1979). Information on the octocorals of the Philippines, the islands of the tropical North and South Pacific, and most of the Indian Ocean and Red Sea is scattered among expedition reports, independent papers in scientific journals, and monographs of families or genera.

The octocoral fauna around the continental landmass of Africa that extends toward the Southern Ocean was studied by Hickson (1900, 1904), J. Stuart Thomson (1910, 1911, 1915, 1917, 1923) and, most recently, by Williams (1989, 1992a, 1992b, 1993). Little is known about the octocorals of the southern tip of South America. Studer (1879) reported on a few species of octocorals obtained in the vicinity of the Straits of Magellan by a German expedition aboard S.M.S. *Gazelle*, and specimens collected by H.M.S. *Challenger* at a number of stations in the area were described by Wright & Studer (1889), but the octocoral fauna as a whole is inadequately known.

Octocorals of the Southern Ocean have been described in numerous independent papers covering a few species or limited taxonomic groups, as well as in reports of Antarctic expeditions (e.g., Gray 1872: *Erebus and Terror*; Hickson 1907: National Antarctic Expedition; Kükenthal 1912: Deutsche Südpolar Expedition; Thomson & Rennet 1931: Australasian Antarctic Expedition), but so far no attempt has been made to revise and monograph the entire fauna with detailed synonymies that correlate information scattered through an extensive literature.

In retrospect, I can now look back on the 100 or more papers I have published on the octocoral fauna worldwide and ask: "What did I really accomplish toward alleviating the deficiencies of taxonomic information?" I am compelled to answer: "Not much." My article on Octocorallia in *The Treatise on Invertebrate Paleontology*, the handbook of shallow-water octocorals of the West Indian region (1961/62), and the key to genera of Octocorals excluding the Pennatulacea (1981) may have clarified some problematic aspects of octocoral taxonomy, but for the most part I have only added bricks to a structure that has yet to be designed and built. About all I can say with any satisfaction is that I have set new standards for illustrating taxonomic descriptions that should make them much more accurate, and identifications much more reliable in the future. The development of scanning electron microscopy and its adaptation to research on octocoral taxonomy would have resulted in new standards in any case, but I believe that I was the first author to publish a description of a new species illustrated with scanning electron micrographs (Bayer 1973). Since that time, most authors have followed suit. Until the year 2000, it was accepted that the mineral of all gorgonians having mineralized axes was calcium carbonate, either calcite or aragonite, depending upon suborder or family, and in some families the supporting axes were described as "purely horny." Through a very rewarding collaboration with geologist Ian Macintyre, some peculiar structures found in the organic axis of several genera in the family Gorgoniidae were found to be neither

calcite nor aragonite, but carbonate hydroxylapatite (Macintyre et al. 2000). Further study revealed that the mineral of all genera and species of the family Gorgoniidae having mineralized axes was carbonate hydroxylapatite (Bayer and Macintyre 2001), a unique taxonomic characteristic.

New techniques of molecular analysis are opening up exciting possibilities for arriving at solutions to the problem of fitting evolving concepts of species and genera that vary with geographical and ecological conditions over great distances into a taxonomic framework based upon the idea of morphologically distinct taxa. Although a completely revised monograph of the western Atlantic fauna still does not exist, comprehensive collections of both shallow-water and deep-water specimens are more than adequate for such a monograph are now on hand. Collections also are now available to support a thorough description of the deep-water fauna of the Pacific coast of the Americas and a review of the shallow-water Panamic fauna. Similarly, enough material exists in several museums around the world to attempt a complete analysis and revision of the octocoral fauna of the Southern Ocean. Although I did produce a few papers that clarified the status of a few Antarctic forms (Bayer 1990, 1996a, 1998), others merely added to the taxonomic overburden and remain to be incorporated in an overall synthesis (Bayer 1950, 1980, 1988, 1993a,b, 1996b, 1998; Bayer & Stefani 1987).

From a purely practical standpoint, one of the most pressing needs today is for a comprehensive study to discriminate species and determine such points where lines can be drawn to discriminate genera, families and, where necessary, suborders and orders. Grasshoff's (Fig. 8d) exemplary work on the shallow-water fauna of New Caledonia, the Red Sea, and the eastern Atlantic are fundamental building blocks in this direction. The literature is now so large and type material scattered among so many collections worldwide that a monograph of world Octocorallia is still an impossible dream. Smaller regional revisions, with their inherent inconsistencies and disagreements, may be the most that can be achieved for the time being.

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Charlie Veron with Christie, 1994

REFLECTIONS

BY

CHARLIE (J.E.N.)¹ VERON

In retrospect, I believe I had a good education for a career in coral biology: none. My Honors thesis was on the behavior of gliding possums, my Masters was on temperature regulation in lizards, and my Doctorate was on the neurophysiology of dragonflies. I have never attended a single lecture on marine biology. Even for those days, few people can have had so much space to develop their own thoughts, unfettered or unmoulded by those of others. And still my good fortune holds, for I now spend a lot of time delving into evolutionary issues. Yet until that subject made a forced entry into my own research territory, I had never actually read a book on evolution since my high school years.² I have always had (or given myself) the freedom, and the time, to ponder — a luxury according to many, a necessity for me. Many young scientists hasten down the path set by their Ph.D. primarily because of security: employment where the next steps are foreknown, and where the competition, the literature, and even the methods are familiar. But the price is often high. Straight-ahead career paths encourage straight-ahead thinking, the all-too-often outcome of which is the not-so-young scientist wishing he or she “just had the time to read and think.” Many of my colleagues imagine they have been caught, like Alice’s Red Queen, in a job where they have to run flat-out to stay in the same place. At times this happens to most of us, but in the longer term the reality is that it can become a habit, where so-called immediate imperatives are allowed to be all-dominating.

I came to work on corals because of two fortunate events. The first was that Terry Done (a colleague at the Australian Institute of Marine Science (AIMS) who also works on corals) started a scuba club at our small rural university, and we found corals where none had been recorded before – at the Solitary Islands off the New South Wales coast, not far north of Sydney. The second was that my Ph.D. turned out to be on an experimental subject where results came thick and fast. I gave a synopsis of my thesis at an International Congress of Entomology in Canberra in 1972 and won a prize, was offered three overseas postdocs (how times have changed), and was

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¹ Why Charlie and why J.E.N.? Well, when I was 6, my teacher nicknamed me “Little Darwin” because I was obsessed with collecting insects and spiders and the like. That soon became the less flattering diminutive “Charlie,” which I’ve been called ever since. But when I went to publish my first paper I discovered that editors don’t like nicknames. So I settled for “J.E.N.” rather than risk a revival of my real name (John) that has always been foreign to me. In retrospect I think this was a bad move because “J.E.N.” sounds formal these days, and rather out of character.

² Curiously for someone nicknamed after Darwin, I did not actually know who he was, nor anything about the subject of evolution, until I was 13. For as long as I can remember I have immersed myself in all things to do with nature, and not unreasonably became very religious, believing that God created it all. The teaching of evolution was banned in Australian schools, and I went to a church school. Then, at that tender age, I read something about “the missing link.” Two traumatic weeks later I emerged from what I can only describe as an emotional collapse, with a hatred of all things to do with my church, school, teachers – the lot. To the end of my secondary schooling I failed most exams, and to this day my first response to new information of any sort is to question it.

advised by the editor of the *Journal of Insect Physiology* to send him my work for publication immediately; otherwise my results would be “stolen.” I accepted the prize and did as the editor bid me do, but I turned down the post-docs. Turning down opportunities of a lifetime – in fact three of them – was not something that students did, even in those days. The harsh words of once-supportive colleagues were still ringing in my ears when I left my old university to take up a post-doc at James Cook University in Townsville (which is about central to the Great Barrier Reef) to work on corals. James Cook University had advertised three times for someone to work on corals who had a Ph.D. and was a scuba diver. I was their only applicant (times have certainly changed) and last hope. I knew next to nothing about corals.

This article is not a biography; I reflect on my own experiences in order to make some points that I think are of general interest. I had dumped an apparently promising career in insect physiology, at that time a field about as big as all the rest of zoology combined, for “a scuba diving holiday,” as my professor described it. What actually happened was that I had listened to myself, and that self knew that I was not an experimentalist who would enjoy a regimented workplace.

What coral research does one do on thousands of kilometers of reef when one knows next to nothing about corals? My plan was to do on reefs what botanists do in forests: describe the communities, work out where they occur, and what the dominant species are – that sort of thing. Enter the word “species,” the word that became central to virtually all my future work. Although James Cook University had a reasonably good reference library for coral taxonomy, I could seldom confidently apply the names I found in these imposing volumes to what could be seen underwater. (And as it turned out, it was many years before I realized the nature, and extent of, the gap between museum-specimen-based coral taxonomy and the realities of the reef.) The essential issue was that as soon as I swam from one environment to another, the species appeared to change, at least a little. I had some knowledge of species in dragonflies, where a minor change in wing venation delineated a different species, or so conventional wisdom then decreed. If such notions were applied to corals, the logical conclusion would be that there are many thousands of species of corals on the Great Barrier Reef, each growing in one specific type of environment, such as a lagoon edge or an outer reef slope (Fig. 1). Nevertheless, the species (if that was the word) that occurred together on the same patch of reef usually appeared more or less distinct. This, and other similar observations, suggested that there was some sort of order, or natural reality, behind the apparent chaos of coral variability as seen underwater.

At this stage I was tempted to continue my work by applying the nearest name from the literature to what seemed to be the species on the reef. Had I had some training in coral taxonomy (or taxonomy of any sort), this is probably what I would have done. As it was, I decided to abandon my research plan, although that seemed a likely path to the ranks of the unemployed – and get to the bottom of this continual (now called intraspecific) variation. Why was it that this variation appeared obvious underwater but was usually ignored by taxonomists? Why did no coral taxonomist ever state how one species could be distinguished from another? Was this taxonomy an end in itself, absolved from responsibility to support other disciplines?

I decided to try to work out precisely how two very well-known corals (I hesitated then to call what I saw “species”), *Pocillopora damicornis* and *Stylophora pistillata*, varied with environment. It was a mixture of laboratory and field work, and the results were, broadly speaking, unbelievable by the standards of any conventional taxonomy (except in plants, but I didn’t think of that at the time). Both species



Figure 1. Bringing taxonomy to the reality of the reef. Most useful taxonomy has been done since the advent of scuba diving, which has allowed coral taxonomists to make careful observations underwater. Great Barrier Reef, Australia (Photo Terry Done).

occurred in a wide range of environments, from the roots of mangroves to wave-hammered reef crests to the deepest depths of outer slopes. When a colony collected from any one of these environmental extremes was compared with a colony from a very different environment, they usually had little or nothing in common. Not only was the growth form different but skeletal details were different also. Yet these details were usually the basis of taxonomic descriptions. It was the lack of gaps in this variation, readily seen underwater (by swimming gradually from one place to another) but also seen under the microscope, that demonstrated links among colonies and indicated the existence (or not) of single species units. OK so far, but this was in stark contrast to what was usually described in taxonomic publications. I decided I didn't like coral taxonomy as a subject and spent most of the following year swimming around reefs, trying to work out what sort of order there was in the apparent chaos of natural variation. Certainly I did not think of *this* as being "taxonomy": I wasn't a "taxonomist." These were people who knew about names and usually (so I then thought) had an awesome knowledge of the detail of skeletal structures. I was just "observing."

That probably would have been the end of this story, had I not had the good fortune to meet two people who reset my stage. The first was "Red" Gilmartin, the first Director of the newly formed AIMS. Apart from offering me a job, Red saw the issues: he said my work *was* taxonomy and that it had to be done before meaningful ecological work on the Great Barrier Reef could be accomplished. AIMS gave me the opportunity to get on with it, and that became the start of the monograph series

Scleractinia of Eastern Australia, undertaken collaboratively with other like-minded field-oriented coral specialists. I reflect now on how much that little bit of insight on Red's part changed the course of my work.

The other person I was fortunate to meet was John Wells, by far the world's most respected coral taxonomist. John well knew that there were problems with the then conventional concept of species in corals, but as he was not a diver he didn't appreciate what the problems were. This came to a crunch at a coral taxonomy workshop held at the Marshall Islands (Fig. 2). We had previously talked about corals at length when I visited John at Cornell University, but at the Marshall Islands we had the opportunity to do more than just talk. I had brought the manuscript of the second volume of *Scleractinia of Eastern Australia* with me in order to get John's comments. He thought the amount of variation my coauthors and I had attributed to several species was "over the top" and singled out *Favites russelli*, one of his own species (and a common one at the Marshall Islands), as a case in point. Armed with hammer, chisel, and plastic laundry basket (the basic tools of the trade), I dived down a reef slope near the laboratory and returned an hour later with about 30 specimens collected at regular depths from the lower slope to the wave-hammered intertidal crest. John and I cleaned them and laid them out in a row on a bench. They made a perfect series, clearly correlated with environment. They convinced John that our observations had, in fact, a sound basis.



Figure 2. Participants Coral Taxonomy Workshop, Marshall Islands, 1976

Top row: Charlie Veron, John Stimson, Paul Jokiel, Gerard Faure, Dennis Devaney, Brian Rosen, Richard Randall

Middle row: Bob Kinzie, Maya Best, Michel Pichon, Jim Maragos, Carden Wallace

Bottom row: Lynton Land, Phil Lamberson, Janet Lamberson, Judy Lang, John Wells, Austin Lamberts

John commented in passing that most coral paleontologists would not hesitate to divide such a series into several genera, and we discussed likely paleontological interpretations had this series of *Favites russelli* been found as a stratigraphically arranged fossil sequence. In all probability, such a sequence would be interpreted as evolutionary change; certainly it would not be attributed to environment: an interesting observation, as valid today as it was then. Such is the continuing gap between coral taxonomy and paleontology. Such comments can be invaluable coming from someone who has the experience to make them. John made another interesting comment a year later, while we were taking a lunch break on a huge Devonian reef about two hours' drive west of where I live. He thought it unlikely that there would ever be a single internationally applicable taxonomic framework for corals. I wish he was still with us – I miss these chats.

This article is about the significant highlights in my work and about what I consider to be turning points. We all have these sort of highlights, and the entry of evolutionary issues (described shortly) is certainly one of them for me. But apart from collecting the insights of people like John Wells and reading a good deal, two things have been especially important to me. The first is the hundreds of conversations I have had with all sorts of people who are willing to share their thoughts with me, irrespective of the subject matter. I am one of those people who store thoughts away, most of which fade, but some pop up in the most unlikely context. It seems to me that humans are good at subconsciously synthesizing information, and that many ideas simply come of their own volition rather than as the intended outcome of planned research. We should always listen to ourselves: intuition, after all, is the outcome of very powerful (cerebral) computers using unimaginably sophisticated programs. The second thing that is important to me is having time to think, even (or especially) if it is, like Winnie the Pooh, thinking about nothing in particular. Powers of deductive logic are probably critical to the work of most scientists, but alas not to me. Give me the soporific combination of a hot sun and a dinghy anchored in calm water on a reef patch, and maybe every so often I'll have a thought that matters.

Fossils and DNA have little in common except that they are linked through our concept of species and of evolutionary change. Or they are thought to be. I have made some personal discoveries here as, no doubt, have dozens of others. I note that palaeontologists love to make pronouncements about genetics, and *vice versa*, yet the jargon is so heavy on both sides that the intended point seems almost never clear to the author, let alone the reader. I note also that if, as rarely happens, a point from one camp *does* manage to infiltrate the other, it does so because of skillful writing, not because of the intrinsic merit of the point. These are unhappy reflections, all the more so because, sandwiched between fossils and DNA, come taxonomy and biogeography, making the issues worse. In general, the dissemination of ideas across distant disciplines is a hazardous undertaking. It usually takes a lot of words; hence "big picture" debates tend to be in books rather than in articles. It also usually invokes the "Scientific American Principle" (as it was once described to me), which states that when an author crosses several discipline boundaries, most readers will give a thumb's-down to the treatment of their own specialty but will probably think the rest is OK.

Being critical is all very well, but many, if not most "big picture" debates are dominated by misunderstanding, or misinterpretation rather than the real issues. At least that was the conclusion I came to while writing *Corals in Space and Time: the Biogeography and Evolution of the Scleractinia*. I read virtually all the relevant

literature of the time (1992-95), not because I wanted to but because I had to because I found good reviews were nonexistent for one subject after another. What I wanted to do was to make a cohesive summary of all research relevant to coral biogeography and evolution and use this as the basis for presenting my own work. This work was multidisciplinary, with the different subjects forming an interlinked network. As it turned out, the task was not the salutary “journey” I had thought it might be. Instead, I found that individual disciplines – paleontology, taxonomy, biological oceanography, ecology, systematics and molecular science – tended to have boundaries, forged by tradition and terminology, which are seldom crossed in meaningful ways. Yet the common subject was “nature” in general and “corals” and “species” in particular; each discipline represented no more than just a different view of the one and same subject.

If I had the space to recall just one incident in retrospect, it would be the following, selected partly because of the subject but also because of the way it happened. The subject, reticulate evolution, I consider to be the most important bit of original work (for want of a better description) I have done. Yet at no stage did this work have an “aim,” and there were never any “materials,” let alone “methods.” To explain, I need to go back about a decade, to a time when I felt confident in my knowledge of the corals of the Central Indo-Pacific (from Australia in the south to Japan in the north). There was some justification for this confidence because, after 20 non-stop years of field and laboratory work, I had studied the corals in most parts of the region and had worked in detail in many. At that time, another field trip to a new location might result in some new (at least new to me) species, but most of what I would see would be all too familiar. I could, more or less, grapple with changes in the appearance of species from one country to the next. That feeling of confident familiarity, however, did not extend to other regions of the Indo-Pacific. If I travelled further afield, east or west, my confidence faltered, not because the corals were different, but because most were neither different nor the same. This became a major issue for me personally: I make mistakes and make “best-guesses,” but I don’t pretend to know what I don’t know. Now, if I ventured into the Central Indian Ocean or Central Pacific, I found myself doubting, with clear justification, different aspects of my own work of so many years. This was serious; I had set out to do my part in what John Wells thought would never be done – the creation of a globally functional taxonomic framework for corals. I thought the taxonomy of corals could be worked out eventually; that species could be described in detail, separated from one another, and mapped. A unified taxonomy, one that took environmental variation into account and one that would support all manner of field and laboratory work, was attainable. Most important of all, this taxonomy would provide the scientific basis for conservation. But, the more I studied the corals of the Central Indo-Pacific (the centre of diversity), the more I doubted the applicability of this work to other parts of the world except in the Caribbean where all species were different. That, at least, was fun. And so, about 10 years ago, I made the decision to bypass the issues rather than confront them. I would not do any more coral taxonomy outside the Central Indo-Pacific.

That was until early one morning (I’m a morning worker) when I got out of bed and went to make a wake-up cup of coffee (as I always do). By the time the jug had boiled, the notion entered my head of its own accord that species were not what I had long assumed them to be. Most were not “natural units” at all; they intergraded geographically, forming patterns of geographic continua. This was a simple, indeed obvious, explanation for what had caused me so much trauma. It made sense of

geographic variation, including problems with taxonomy, synonymies and “fuzzy” distributions. Yet it argued against all current biogeographic theory, in fact the whole neo-Darwinian concept of species, which treated species as units. Then came (while I was still drinking my coffee) the thought that what was observable in geographic space must also apply to evolutionary time. That morning I consulted John Benzie, at that time another colleague at AIMS, who introduced me to what geneticists called “reticulate evolution” and recommended I read Verne Grant’s *Plant Speciation*. I did just that, and found many of my thoughts of the morning clearly set out – for plants.

Reticulate evolution is a paradigm, fundamentally distinct from that of neo-Darwinism (the “neo” meaning the follow-on from Darwin). It offers an alternative view of the nature of species and how species change in space and time. It clearly applies to most corals and (I now believe) to most other “species”, for most species do not form genetically cohesive units. I outline the main points, as applied to corals, below,³ but do so reservedly as I am aware that brief explanations such as this one read like scientific heresy. But, given an hour to present the theory at a conference (as I have done many times), or over a glass of beer (many more times), I get no such reaction – in fact, the very opposite. There are no mysteries about it. The basis is obvious to the point of being undeniable, yet reticulate evolution is either a falsehood, or much of what is generally believed about the nature of most invertebrate and plant species (at least), including their evolution and biogeography, stands on a false premise.

I have described reticulate evolution, as applied to corals, in *Corals in Space and Time* and again, perhaps more clearly, in the third volume of *Corals of the World*. As a concept, it has a lot of development ahead, especially concerning how it interfaces with Darwinian natural selection and the many biogeographic and evolutionary theories that depend on species being units. The bottom line appears to be that natural selection is basically the icing on the cake, icing which comes into existence when, and only when, a species forms a genetically cohesive unit over its whole distribution range. Only then are species what they are generally thought to be: units.

Humans cannot communicate easily in terms of continua. They need to have units of some kind, units with names (such as species names) to which other sorts of information can be attached, e.g., descriptions, maps, ecological attributes, and experimental results. The concept of reticulation makes this difficult. Worse, the concept is destructive, not only of generally accepted neo-Darwinian principles

³ For most marine organisms, ocean currents are the vehicles of larval dispersal and are therefore the pathways of genetic connectivity. These paths repeatedly and continuously change over time, creating changes to the distribution ranges and genetic compositions of species. Geographic space and evolutionary time interact: species diverge, then re-form into different units. For corals, this creates “reticulate” patterns in both geographic space and evolutionary time. In geographic space, species are typically distinct in any single region but lose their identity as taxonomic units over very great distances. When these patterns are envisaged in evolutionary time, species have no time or place of origin and there are no distinctions between sympatric and non-sympatric concepts of origination. Differences between species and subspecies taxonomic levels and between species and “hybrids” are arbitrary and/or unrecognizable. Reticulate evolution is driven by environmental parameters, not biological competition. Rates of evolution and extinction (which occur through fusions as well as terminations of lineages) are similar over long geological intervals. Taxonomic, systematic, and biogeographic concepts of neo-Darwinian and reticulate evolution are mutually exclusive, except where single species form genetically cohesive, reproductively isolated, units. In the latter case, natural selection as the driving force of evolutionary change becomes dominant over environment-driven reticulate change.

embodied in taxonomy, systematics, biogeography, and evolutionary theory, but also of the life-long work of many scientists. So why (I am often asked) do I persist with it? My answer is partly that I would have gone about my own work on corals differently had I known about it. More fundamentally, the concept of reticulation suggests an alternative way of interpreting practically any biological result or observation. It argues that young biologists should think again if they believe they are dealing with any sort of biological unit, for these mostly depend on poorly supported assumptions. I come back to the main point of this article: time to think laterally may be a necessity, not a luxury, if the aim of the work is to get at the truth.

I am also often asked: if reticulation is as widespread as I claim it to be, why is it not generally known or accepted? The answer may be that reticulation is not visible to taxonomists unless the taxonomist works over very large geographic areas as well as in depth. More likely, however, the literature embodying neo-Darwinian thinking (the books alone occupy about three meters of shelf space in my study) offers such a smorgasbord of ideas that none who had received any education in the subject would be likely to question it.

If I may reflect just a little on the foreseeable future, I do not think that reticulate evolution is likely to gain widespread acceptance for the simple reason that natural selection has so much inertia in so many areas and is clearly responsible for the exquisite adaptations we see in nature and presented on TV nature programs. A corollary of this is that cladistics (a supremely logical computer method of phylogenetic analysis and a useful tool in knowing hands) is likely to remain *the* central means of phylogenetic analysis. I find that alarming. It has long been known that cladistics will not work on reticulate systems, but it appears to have been commonly assumed that these systems are unusual or restricted to phylogenies which hybridize. Cladistics packages seldom fail to produce superficially impressive (editor-pleasing) results, yet these results can all too easily displace intuitive thinking. These are, admittedly, idle conjectures. What is not so idle is that we all are guilty, to some degree, of deluding ourselves that we are understanding nature. We should remind ourselves that all we can ever hope to do is make the best of it. The natural world is, and probably always will be, complicated far beyond human understanding.

My concluding “reflections” predictably focus on conservation. When I first worked on the Great Barrier Reef, I always felt a moment of anxiety after rolling backwards off the side of a boat to go for a dive. We all felt that. We waited for the bubbles to clear just to make sure that there wasn’t a big tiger among the sharks that always gathered around. Now, anywhere in the Asian region, I swim long distances over deep water without the slightest concern, for there are virtually no sharks left, big or small. I haven’t even seen big fish in any numbers around an Asian reef in years. The plight of sharks is symptomatic of what is happening to reefs. Destruction through explosive and poison fishing, accompanied by the smashing of the corals in which the fish hide, is now going on at an awesome pace. And now coral reefs are bearing the brunt of global climate change. Having worked in all the major reef regions of the world, my job has become depressing – the last thing I would have once expected.

It was this feeling that prompted Mary Stafford-Smith and me to produce *Corals of the World* (Fig. 3). We hope it can win some hearts as well as minds. We hope it will encourage people of all descriptions to do their part in conserving what is now left. This, more than anything else that I have mentioned in this article, has become what matters.

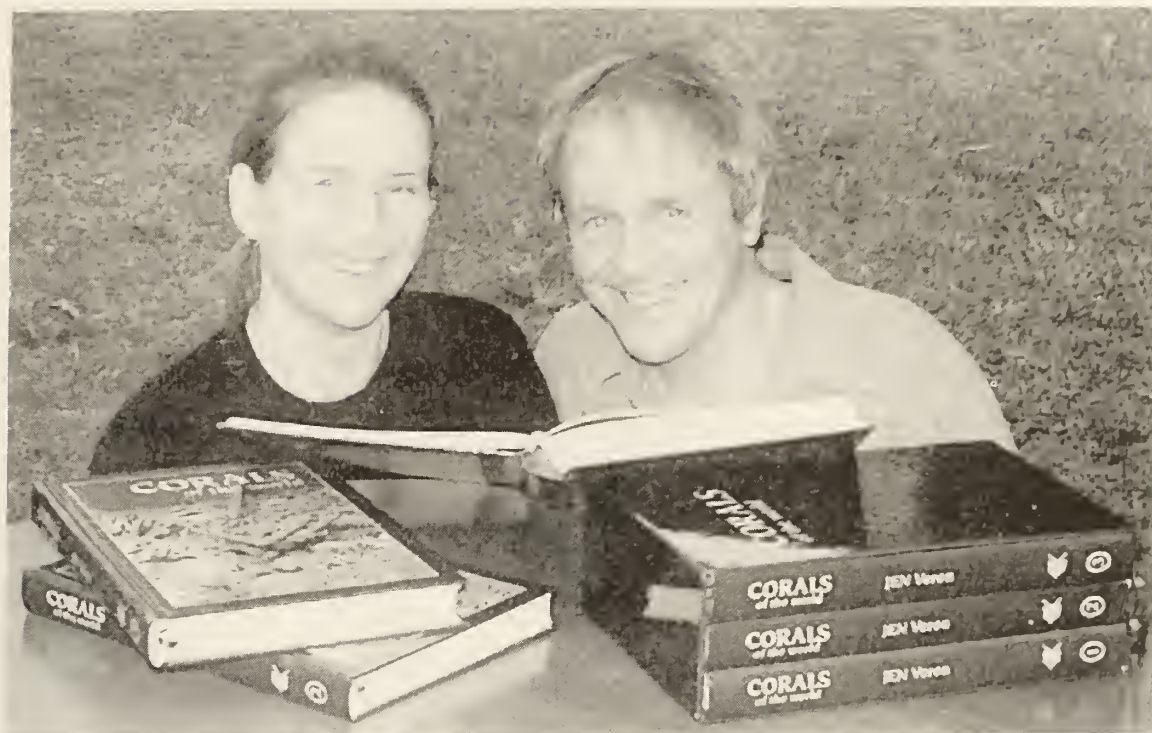


Figure 3. Charlie Veron (author) and Mary Stafford Smith (scientific editor and producer) after the publication of *Corals of the World* in October, 2000. The book was written for the general public and was produced at their home in Townsville, Australia.



Peter W. Glynn with a new record of Indo-Pacific hydrocoral *Millepora exaesa* Forskål, discovered at Clipperton Atoll in April 1994 (Photo Gerald M. Wellington)

EASTERN PACIFIC CORAL REEFS: NEW REVELATIONS IN THE TWENTIETH CENTURY

BY

PETER W. GLYNN

INTRODUCTION

Ever since I entered the field of coral reef ecology, in the early 1960s, I have been intrigued by Darwin's (1842) and Dana's (1843, 1890) contention that, for reasons of cold water currents and upwellings, coral reefs should not (and do not) exist off western American shores. This idea was upheld by Vaughan (1919) and Crossland (1927), among others, almost 100 years later. However, Joubin's (1912) map of the coral reefs of the world, reprinted with additions by Wells (1957), shows coral reefs present on the Pacific coast of southern México, and along the coasts of El Salvador, Costa Rica, Panamá, Colombia, Ecuador, and northern Peru (Fig. 1). A coral reef is also shown at Cocos Island, but none is indicated in the Galápagos Islands. To my mind, this was clearly an interesting contradiction violating the dictum that coral reefs are confined to clear tropical waters of low organic productivity. This paradox stimulated one of my first aims upon reaching Panamá in 1967, that is, to determine if coral reefs — wave-resistant, geomorphologic structures that build the substratum on which they continue to grow — do in fact exist in the highly productive marine environment of Pacific Panamá. If coral reefs are found in eastern Pacific waters, then how are they distributed, what are their sizes, and when did they begin to accrete? Further, posing some ecological questions, what is their species composition, and how do various interactions, such as competition, grazing, predation, and symbiosis, regulate their community structure? This essay will explore the historical development of coral-reef studies in the eastern tropical Pacific, beginning in the late 1960s, from the perspective of someone who first observed reef-building corals in the Gulf of California in the early 1950s.

One of the strongest motivating factors in any scientific discipline is the excitement of discovery. The discovery of eastern Pacific coral reefs, their associated biota and diverse species interactions, has truly been an exhilarating experience for me. Many of the broad-ranging questions posed above have required extensive travel to remote areas. This has involved transporting field equipment on back roads, some mapped and some not, by foot and by a variety of watercraft that would never meet the safety requirements of the University National Oceanographic Laboratory Systems

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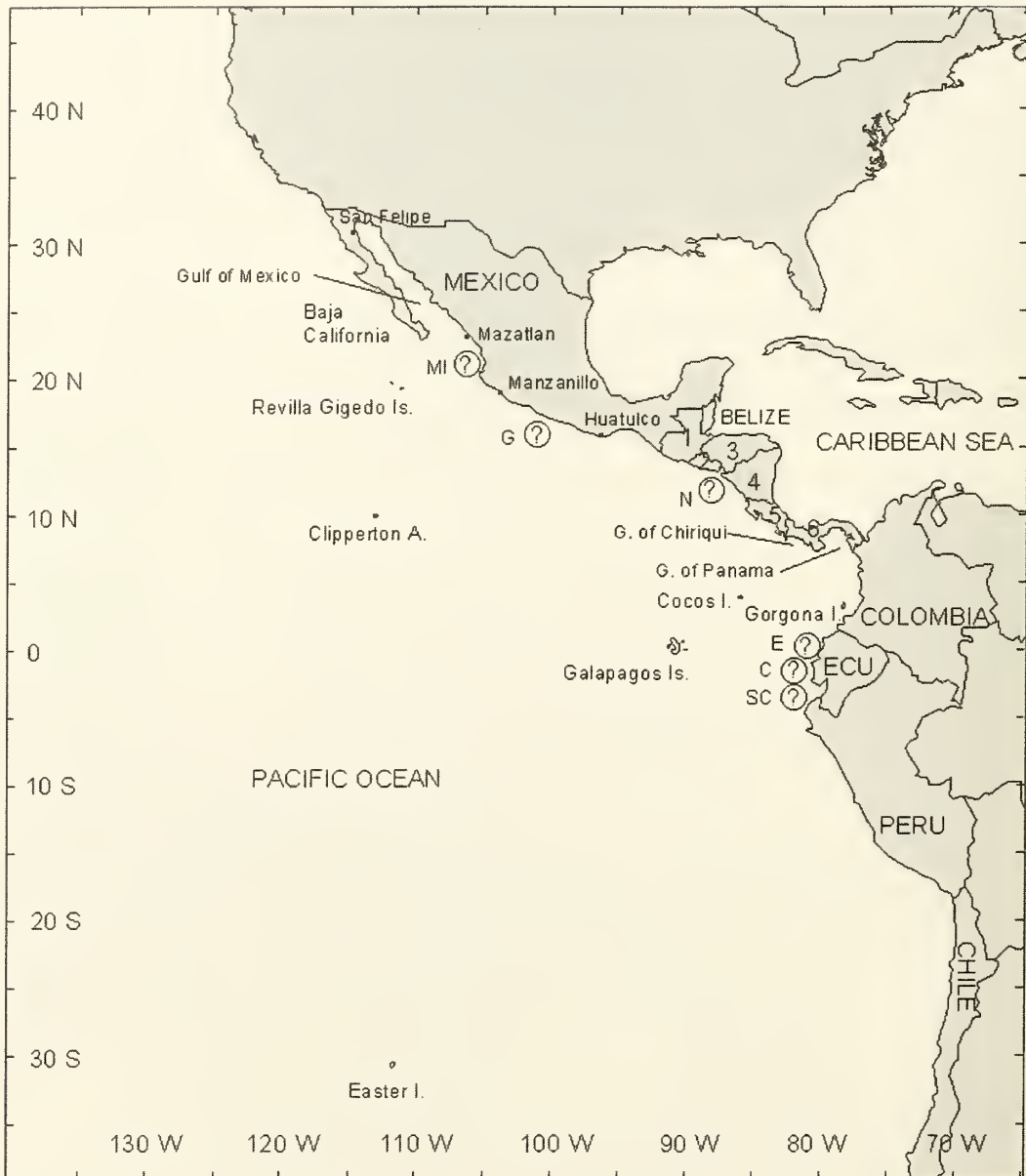


Figure 1. Locator map of the eastern Pacific. 1, Guatemala; 2, El Salvador; 3, Honduras; 4, Nicaragua; 5, Costa Rica; 6, Panamá. Question marks denote unexplored or relatively little studied areas: MI, Marias Islands; G, Guerrero state; N, Nicaragua; E, Las Esmeraldas; C, El Copé bank; SC, Santa Clara Island.

(UNOLS) National Science Foundation program. These excursions sometimes resulted in false starts, and unexpected, hazardous and amusing events that inevitably occur during field studies at remote sites. A few of these, which have contributed to the adventure of my work, are interlaced in the following narrative.

How did it all begin? My most vivid and cherished memories of first exploring and learning about marine life were with my great aunt, Dora Conrad, an eccentric but

lovable schoolmarm who gloried in explaining the ways of the natural world. During my preadolescent years, we tramped the hills, parks and beaches of southern California, where she extolled on the animal, vegetable and mineral worlds that unfolded before us. Since we both lived on Coronado Island, our walks became more focused on the shoreline. She was in great measure responsible for sparking my interest in marine life, which first led to making a shell collection, then to the collection and study of various plants, invertebrates and fishes. By the time I entered high school, my friends — John C. Elwell, Stephen E. Flynn, Marvin A. Nottingham, Charles Quinn, among others — and I began to make diving trips into México. As time went on my interests shifted more from spearfishing to exploring the panoply and interrelationships of marine life. I first observed living corals in the Gulf of California, in subtropical areas south of San Felipe, Baja California, and around San Carlos Bay and Tiburón Island. Our excursions eventually extended farther south into México, and to tropical areas around Mazatlán and Manzanillo. Stimulated by F. G. Walton Smith's book, *Atlantic Reef Corals* (1948), I traveled to Belize (formerly British Honduras) in 1959 to observe firsthand the largest coral-reef system in the Caribbean Sea. There I offered my diving services to a Mexican fishing crew in exchange for passage to the barrier reef. The three weeks of living and diving along the Belizean Barrier Reef left me with a lasting impression of the magnificence of the marine tropical world.

Two of my science teachers at Coronado High School — Curtis J. Yeagar in biology and Marvin Nottingham in chemistry — who recognized my interests encouraged me to continue studying in the natural sciences. In graduate school, at the Hopkins Marine Station of Stanford University, I was especially influenced by the high standards of scholarship and investigative pursuits of Arthur Giese, Cornelius van Niel, Lawrence Blinks, and Donald P. Abbott. Don Abbott was my doctoral adviser and mentor, greatly admired by me for his love and dedication to teaching and research (Fig. 2).



Figure 2. Three vertebrates contemplating the ways of intertidal invertebrates off the Hopkins Marine Station, Pacific Grove, California. Left to right: Donald P. Abbott, Peter W. Glynn, and Charles (Chuck) Baxter. Pete's (P. W. Glynn) Island is visible behind Chuck. July 20, 1977. (Photo C. Kitting)

Some experiences related to finding lodging form a vivid part of my memory of graduate school. During my field studies in Pacific Grove I inquired about renting the late Edward F. Rickett's Pacific Biological Laboratories. Steinbeck's colorful character "Doc," in his classic novel, *Cannery Row*, was in large measure fashioned from Ricketts, the marine biologist whom I greatly admired. I was shown through the living and laboratory spaces, still cluttered with papers and preserved specimens. I was allowed to keep one of Ed Rickett's desiccated tortoises, which I plucked from a large holding tank. Unfortunately, when I called the

following week to finalize a contract, I was told that another party had already offered a higher rent that I could not match. I did the next best thing, however, and let a room for \$25.00 per month above the Bear Flag bar and restaurant down the street. The clacking and whistling of the predawn freight train assured early morning rises for the summer low tides. While completing course requirements at the Stanford campus in Palo Alto I again began searching for convenient and low-cost accommodations. One afternoon after a seminar in Jordan Hall, the enormous labyrinthine biological sciences building, I wandered up a flight of stairs and discovered a little-known greenhouse loft. Off to one side was a vacant, low-ceiling room with a long desk, book shelves, sink, and electrical outlets. At first I tentatively occupied the desk, moved my books in, then an electric heating plate and coffee pot, sleeping bag and clothes, and presto, I had new accommodations. Early morning trips to the showers in the basement and my constant companions, the cooing pigeons nesting in the eaves, made this a most comfortable arrangement. After a few months my living situation was discovered by one of my distinguished committee members, shall we say Professor "Goodheart." Fortunately, he was sympathetic with my situation and allowed me to occupy this niche for the remainder of my tenure on the Stanford campus.

Upon nearing completion of my dissertation research, a study of the community structure and trophic interrelationships in a high intertidal assemblage, Don Abbott invited me into his office to discuss where I might like to teach. He laid out several letters from prominent, stateside universities, inquiring of the availability of recent graduates who might be interested in joining their faculties. Don was not surprised when I announced that I preferred to work in a tropical setting, and to take up an available teaching post at the University of Puerto Rico (Mayagüez) where I could investigate coral reefs. My wife, Carmen S. Glynn (Quiñones), who was born in southwestern Puerto Rico (Lajas), played no minor role in this decision. Don understood my desire to move to the tropics and offered his full support, for he too had an abiding interest in coral reefs, having lived for nearly five months in 1953 on Ifaluk Atoll in the Caroline Islands. He was part of a research team, including such other members as Marston Bates, Frederick (Ted) M. Bayer, and Joshua I. Tracey, charged with documenting the way of life of the Ifaluk people (Bates and Abbott, 1958).

The years spent in Puerto Rico (1960-1967), at the Institute of Marine Biology in Mayagüez and the Magueyes Island (La Parguera) field station, were pleasant and rewarding. There I began to develop an appreciation for the physical controls of reef development and the often subtle and complex biotic interactions regulating coral-community structure. Memories of 24- and 48-hour field exercises on offshore reefs with the Coral Reef Ecology class are still vivid and evoke a feeling of pedagogical accomplishment. At that time I also began reading the *Atoll Research Bulletin*, which supplied a diverse literature that helped to broaden an understanding of coral reef science in Puerto Rico, e.g., reef studies by J. W. Wells (1951) in the Marshall Islands, P. E. Cloud (1952) in the Gilbert Islands, and D. R. Stoddart (1962) in Belize. One of the star students in this course, Alina M. Szmant, has become a distinguished coral reef biologist. Juan A. Rivero and John E. Randall, successive directors of the Institute, offered invaluable support and encouragement during my employment at the University

of Puerto Rico. In 1967, I accepted a position with the Smithsonian Tropical Research Institute (STRI) in the Republic of Panamá. Upon leaving Puerto Rico, it was with sadness on the one hand, but with excitement on the other because I would be able to revisit tropical Pacific shores. A major research goal at STRI, strongly supported by successive directors Martin Moynihan and Ira Rubinoff, was to conduct comparative studies of the ecology, behavior, and evolution of marine species and communities on both Caribbean and Pacific shores.

EXPLORATION AND DISCOVERY

Equatorial Eastern Pacific

It seemed to me that information on the distribution of coral assemblages, their species composition, zonation, and extent of buildup under different environmental conditions was needed to help understand why reef development was not widespread in the eastern tropical Pacific. With my good friend and colleague, Robert H. Stewart, who had already begun surveying coral reefs in the Pearl Islands, I began a reconnaissance of coral reefs in the Gulf of Panamá. Coral specimens were collected, many of which were identified by J. Wyatt Durham. I was struck by the abundance of coral assemblages and coral reefs present on islands throughout the gulf. This was in stark contrast to Crossland's (1927) conclusion that coral reefs were absent from the upwelling Gulf of Panamá. To locate coral assemblages and reefs, I often flew with pilot friends along coastal areas during midday low-tidal exposures. Ira Rubinoff, Richard Rosenblatt, and others pursuing ichthyological studies suspected the presence of a rich fish fauna associated with corals in the nonupwelling Gulf of Chiriquí, western Panamá (Fig. 1). Ira arranged with the U.S. Navy an expedition to this remote and poorly known gulf in April 1970 aboard the LST *Traverse County*. In addition to the discovery of several new fish records and species, an interesting coral fauna and diverse assemblages of associated reef species were found. Several relatively well-developed coral reefs also were discovered, with distinct depth zones and thick framework structures. One of these, at the south end of Bahía Damas, Coiba Island, covers about 160 ha and may be the largest coral reef on the continental margin of the eastern Pacific (Glynn and Maté, 1997). Several coral-reef accumulations also were found along continental shorelines and, surprisingly, at the mouths of some large estuaries. Three species of hydrocorals, a taxon previously unknown in the eastern Pacific, were discovered on the Uva Island coral reef. Two of the hydrocorals belonged to central/western Pacific species, but one proved to be a new species endemic to the Gulf of Chiriquí (Weerdt and Glynn, 1991). Moderately large populations of the Crown-of-Thorns sea star, *Acanthaster planci* (Linnaeus), were found foraging on a variety of coral prey. Because of the prominent development and relatively high diversity of coral reefs in Chiriquí, a number of workers have been attracted to this region, including Charles Birkeland, Thomas F. Dana, Raymond C. Highsmith, Ian G. Macintyre, William A. Newman, John C. Ogden, James W. Porter, Robert H. Richmond, Ernesto Weil, and

Gerard (Jerry) M. Wellington. One of Tom Dana's (1975) studies, now considered a classic, proposed a model of long-distance dispersal from the central Pacific to explain the origin of eastern Pacific corals and coral reefs following Pleistocene extinctions. This hypothesis provoked caustic criticism from a few workers who held to a vicariance explanation for eastern Pacific coral origins (McCoy and Heck, 1976; Heck and McCoy, 1978). Some of my students have studied (and continue to study) coral reefs in Chiriquí as well, on one occasion establishing a temporary field laboratory with running seawater and various other amenities (Fig. 3).



Figure 3. Uva Island Beach Club, a temporary field laboratory, Gulf of Chiriquí, Panamá). Juan L. Maté and C. Mark Eakin (standing, left to right), David B. Smith, Peter W. Glynn, and José Manuel Gil Lasso (sitting, left to right). February 21, 1989. (Photo C. M. Eakin)

With information crossing my desk in the early 1970s of the existence of coral reefs in the Galápagos Islands, this seemed like the next logical place to investigate. The author of these findings, Jerry Wellington, located several coral-reef formations during an assessment of the marine resources of the Galápagos coastal environments. Thanks to grants from the Smithsonian Institution and the generosity of

Thomas J. Watson, Jr., we were able to embark on two research cruises in the Galápagos in 1975 and 1976. The research team in 1975 included John W. Wells, Charles (Chuck) Birkeland, Jerry, and myself (Figs. 4, 5, and 6). Working from the *M/Y Beagle III*, we surveyed several coral-reef sites from the southern sector of the archipelago to the northernmost islands of Darwin and Wolf. Upon completion of a second survey in 1976, aboard the *M/Y Palawan*, we had gained sufficient knowledge to publish a book on the corals and coral reefs of the Galápagos Islands (Glynn and Wellington, 1983). No fewer than six new species of azooxanthellate corals were named as a result of this work (Wells, 1982).

On our return trip to Panamá in 1975, John, Chuck, and I rented a car and drove up the Ecuadorean coast to examine a coral formation spotted earlier during an aerial overflight. It was high tide when we arrived at the beach near Machalilla and the object of our visit was located a few hundred meters offshore at Sucre Island. I spotted some fishermen down the beach with canoes and the stage was set for bartering. After considerable haggling, the best I could negotiate was a round trip to the island for the equivalent of about \$40. Chuck and I decided this was too high a price, so we donned our diving gear and started swimming toward Sucre Island. We were soon joined by a



Figure 4. En route to the M/Y *Beagle III* with a skiff full of gear, Academy Bay, Santa Cruz Island, Galápagos Islands. Fore to aft: John W. Wells, Peter W. Glynn, and Gerard M. Wellington. January 9, 1975. (Photo C. Birkeland)



Figure 5. Examining corals at the Urvina Bay uplift, Isabela Island, Galápagos Islands. Left to right: Peter W. Glynn, Gerard M. Wellington, and John W. Wells. January 15, 1975. (Photo C. Birkeland)



Figure 6. Discussing the day's activities in the salon of the M/Y *Beagle III*. Left to right: Charles Birkeland, John W. Wells, and Peter W. Glynn. January 22, 1975. (Photo G. M. Wellington)

fisherman in his canoe who paddled alongside us. About halfway to the island, the price dropped to \$30, then to \$20, and finally to \$10 upon nearing the island and its fringing coral assemblages. By this time the fisherman realized we were serious and proceeded to help us collect corals during the remainder of the dive. By the end of the survey we were on friendly terms, and he told us of other coral formations in the area that he had seen while fishing. It had not escaped me that he was admiring my swim fins,

so these remained with him as part of the final payoff. The Sucre Island coral formation turned out to be the first coral reef reported on the Ecuadorean coast and, at that time, the southernmost coral reef in the eastern Pacific.

Flying into and out of the Galápagos Islands in the 1970s was sometimes problematical and amusing in retrospect. On one occasion, Jerry and I were bumped from our flight when attempting to leave the islands after a research cruise. This

involved retracing our journey back to the Charles Darwin Research Station via overcrowded buses and a boat trip across Baltra Canal — a roundtrip journey that took up most of the day. We were again promised seats by the airline manager the next morning and I slipped him a \$20 bill to make certain we wouldn't be left behind. To our consternation, and to that of about 20 other souls standing on the tarmac, the plane was loaded, the doors shut, and it began to taxi down the runway. With engines whining, suddenly a door opened and the manager pointed to us, giving the sign that two more passengers were welcome aboard. With that, everyone started to run toward the plane juggling baggage the best they could. Jerry was quite athletic then (and is still), and was able to jump up to, and grasp, the opened doorway with a boost from my cupped hands. I then threw our bags to him, and grasped his down-stretched arm for the final lift into the plane — or so I thought. The other would-be passengers saw what we were doing and decided to take advantage of our human chain. One man grabbed my legs and another began to climb up my torso to reach the open door. I beckoned to Jerry on how to extricate myself from this predicament. Without hesitation, he instructed me to pummel and kick at the intruders until they fell away. This I did, Jerry quickly hoisted me up, the manager slammed the door shut, and off for the mainland we headed. On later visits to the Galápagos, I wondered if I might be spotted and “paid back” for this bravado.

One of my intrepid volunteer pilots, Dennis (Capt.) Cismowski, flew helicopters in the U.S. Army, helping me with reconnaissance flights in his spare time. Not only did Dennis assist me with aerial surveys in Panamá, Costa Rica and Colombia, but he was adroit at a variety of underwater tasks and also helped with in situ reef studies. Our automobile trip to Machalilla in 1975 allowed us to survey the central Ecuadorean coast, but there were rumors of coral reefs in the northern part of Ecuador, at Las Esmeraldas, near the Colombian border. I mentioned this to Dennis and he proposed that we fly to Ecuador from Panamá, cruising the coastline on the way. After obtaining permission to cross the Panamá/Colombia and Colombia/Ecuador international borders, with the assistance of the Smithsonian Institution and the U.S. State Department, we loaded our Cessna 172 with four scuba tanks and five jerricans of aviation fuel. We flew through some harrowing electric storms along the Choco coast and landed safely in Cali after a brief refueling stop in Buenaventura, Colombia. The administrator at the Cali airport claimed that our papers were not in order and demanded that we return to Panamá. I tried convincing him that we did have the necessary clearance, but he would not budge from his position. So we again refueled and took off, heading north to Panamá. When we reached a coastal mountain range, Dennis winked at me and said: “If you really want to go to Ecuador I can drop to just above tree level and no radar will be able to detect our progress.” I winked back and we were quickly heading south again. By the time we reached Tumaco, not far from the international border, we were running low on fuel. We buzzed the airfield, faking engine problems, were given a green light from the tower, and then landed. Our plane was immediately surrounded by armed Colombian soldiers. We were taken into custody and held at an army base for two days. The Cali airport administrator had radioed ahead that we would probably be heading south. We struck up a friendship with the Colombian lieutenant in charge of the garrison, who

allowed us to “escape” early one morning. As of this writing, Las Esmeraldas is still in need of study.

Before the 1980s, virtually nothing was known of the extent of coral-reef development along the Colombian Pacific coast. On our ill-fated flight to Tumaco, Dennis and I skirted Gorgona Island and spotted coral reefs on the east side of the island. A few years later (1979), I was invited by Henry von Prael to participate in the Sula III Expedition to Gorgona Island, organized to conduct inventories and studies of the marine and terrestrial biota of the island. Joining me was Peter Castro, one of my first students in Puerto Rico, who was on sabbatical leave (from California State Polytechnic University, Pomona) and working in my laboratory in Panamá. Between Henry, Felipe Guhl, and myself, we were able to characterize the coral fauna, assess corallivore activities, and complete reasonably detailed surveys of the distribution and geomorphology of the Gorgona Island coral reefs (Glynn et al., 1982). Peter Castro collected and described the decapod crustacean fauna living symbiotically among the branches of pocilloporid corals (Castro, 1982).

During our stay at Gorgona we were comfortably housed in the penal colony’s guest quarters and nourished by the prison’s finest cuisine. Admittedly, the green-colored needle fish was not my favorite breakfast fare, but the freshly baked bread was a delight. To obtain a bread roll required hard bartering with the inmates, some of whom were assigned to look after our needs. One evening I engaged “El Diablo,” one of our inmate helpers who was named for his Mephistophelean features, in a conversation to learn of the misdeed that landed him in prison. Many years ago, so the story went, he was an innocent lad fishing with a group of men in an estuary. They were dynamite-fishing, which was unlawful at the time. Suddenly a government patrol boat rounded the point. Everyone, save El Diablo, quickly jumped into the water and escaped by swimming into mangrove thickets. El Diablo didn’t know whether to flee or surrender; he panicked, compulsively lit a stick of dynamite, and threw it toward the approaching boat. He never revealed the full extent of the bodily harm done, but did say that he was resigned to remain in prison for the rest of his life.

In the late 1970s we conducted surveys along the Costa Rican coast, first by performing reconnaissance flights at midday low-tidal exposures to locate suspected coral assemblages, and then by traveling to sites by automobile and boat. These in situ inspections revealed the presence of numerous coral reefs from the southwestern sector of Costa Rica near Panamá to northwestern Costa Rica close to the Nicaraguan border. All of our survey work in Costa Rica was pleasant and productive except for one incident on the Santa Elena Peninsula near Nicaragua. Anastasio Somosa, the former president (dictator) of Nicaragua owned a ranch on this peninsula and at that time he still tried to lay claim to this territory. Further complicating events were the bands of Sandanista insurgents who were organizing raids in Nicaragua from sanctuaries in Costa Rica. On this occasion, in 1978, we were surveying areas in Santa Elena Bay by diving from a black inflatable. We were unaware that we were being watched by the crew of a Nicaraguan gunboat. When the gunboat suddenly accelerated and started heading directly toward us, we pulled anchor and made haste for the shallow end of the bay where our truck was parked. Fearing that we might be pursued by an armed landing

party, we quickly loaded the truck with our gear and departed the area in haste. My ventures into Costa Rica were not only beneficial on the research side, but also allowed me to meet several resident marine biologists, among whom Manuel M. Murillo, Jorge Cortés, and Hector Guzmán have figured importantly. Jorge was to become my first doctoral student at the Rosenstiel School of Marine and Atmospheric Science, University of Miami.

Other Eastern Tropical Pacific Areas

More recently I have participated in field studies at Clipperton Atoll (1994), at Huatulco on the south coast of Mexico (1996, 1997), and at Easter Island (1999, 2000). For many years, the coral fauna and extent of reef development at Clipperton were largely unknown. Only tantalizing brief accounts and unpublished field notes were previously available (Allison, 1959; Hertlein and Emerson, 1957) on what is probably the largest coral-reef formation in the eastern Pacific region. Thanks to a grant from the National Geographic Society, J. E. N. (Charlie) Veron, Jerry Wellington, and I, among several other marine scientists, were able to join the Clipperton Expedition organized by John D. Jackson. Our 13-day study revealed that Clipperton contains a meager nine zooxanthellate coral species, of which only three or four have contributed importantly toward reef building. One of these, exhibiting an attractive plating morphology, was recently described by one of my current students and a colleague of his: *Porites arnaudi* Reyes Bonilla and Carricart Ganivet (2000). Since total live coral cover extends over 370 ha of bottom, to a depth of at least 80 m, this is the largest known coral reef in the eastern Pacific (Glynn et al., 1996). The next largest studied coral reef occurs at Coiba Island (Panamá), and covers 160 ha (Glynn and Maté, 1997).

En route to Clipperton, about 450 km SSW of the tip of Baja California, we visited three islands in the Revillagigedo Archipelago: San Benedicto, Clarion, and Roca Partida. Due to the sudden appearance of Barcena Volcano on San Benedicto Island, which received considerable local press in my home town (Coronado) when it was "born," during an explosive eruption on 1 August 1952, the extent of coral development on this island was uncertain. Barcena reached 300 m in elevation in only 12 days. Amazingly, six coral species were found at shallow depth along the basalt shoreline during our survey in 1994, and some of these corals formed abundant populations. One-meter-thick living pocilloporid fringing reefs were present at the north end of the island, suggesting rapid colonization and growth since the eruptive event. Growth rates of pocilloporid branches from 3 to 6 cm per year have been measured in the eastern Pacific (Glynn, 1977; Guzmán and Cortés, 1989), so a spurt in reef-framework accumulation of this magnitude is possible. Current studies in the Revillagigedo Islands, including Socorro Island, are being actively pursued by Mexican researchers (e.g., Reyes Bonilla and Carriquiry, 1994; Ketchum and Reyes Bonilla, 1997).

Palmer's (1928) brief and provocative account of the coral reefs of Huatulco was largely responsible for turning my attention to the southern coast of México. Like the gulfs of Panamá and Papagayo (Costa Rica), Huatulco lay within the upwelling Gulf of

Tehuantepec. Therefore, Palmer's mention of coral reefs was not surprising in light of their occurrence in other eastern Pacific upwelling centers. Initial surveys revealed the presence of 12 zooxanthellate coral species and 17 pocilloporid coral reefs, mostly in bays, ranging in depths from 2 to 14 m, and with framework buildups of 1 to 5 m (Glynn and Leyte Morales, 1997). Additional coral species records and new reefs are being discovered in this area as investigations proceed by workers at the Universidad del Mar, Oaxaca (Leyte Morales, 2001). A heightened interest in Pacific corals by Mexican workers in the 1990s has resulted in much new information regarding the reef-building scleractinian fauna and the distribution and environmental controls of reef development (e.g., Reyes Bonilla, 1993; Carriquiry and Reyes Bonilla, 1997; Ketchum and Reyes Bonilla, 1997; Reyes Bonilla and López Pérez, 1998). An area still in need of study, however, is the Marías Islands, which lie northwest of Banderas Bay (Fig. 1, MI). A preliminary inspection of one of these islands, the northeast side of María Cleofas, has revealed the presence of a circular pocilloporid reef several km in circumference, possibly one of the largest coral reefs in the eastern Pacific (H. W. Chaney, pers. comm.). Since a penal colony is located in the Marías Islands, it is difficult to obtain permission to work in this area. Finally, the Guerrero state coastline, a 300 km stretch from Acapulco to Zihuatanejo, is another relatively unstudied Mexican area.

Remaining Areas in Need of Study

Other eastern Pacific areas that are still in need of exploration are El Salvador, Nicaragua, and parts of the Ecuadorean coast. A recent survey of El Salvador by Héctor Guzmán, Jorge Cortés, and Juan Maté (29 March to 3 April 2001) failed to produce any corals. It is likely that turbulent seas and reduced visibility during this survey prevented a thorough search of favorable coral habitats. Another attempt should be made to investigate this area since local divers have observed corals here, notably offshore of Los Cóbanos. The Farallones Islands in the Gulf of Fonseca (within Nicaraguan territory) may also be of interest because these islands are located in a more oceanic setting, of probably higher water quality, near the gulf entrance. The subtidal marine assemblages of the Esmeraldas coast of northern Ecuador are still largely unexplored since my failed attempt to reach this area in the late 1970s. The extensive shallow rocky platforms in this region may support coral communities. Ecuadorean fishermen have reported abundant massive corals further south on offshore banks, e.g., at El Copé off Libertad, and a coral reef at Santa Clara Island in the Gulf of Guayaquil.

Exploratory studies were recently (1999, 2000) initiated at Easter Island, an isolated coral outpost in the southeastern Pacific. This area is of interest because of its hypothesized biogeographic link with the far eastern Pacific. It is possible that Easter Island has served as a stepping stone during the migration of corals and reef-associated species from the easternmost Polynesian Islands into the equatorial eastern Pacific. Of the 11 currently recognized zooxanthellate corals, two species are predominant: *Pocillopora verrucosa* (Ellis and Solander) and *Porites lobata* Dana. Incipient reef frameworks are 2 to 7 m in vertical relief in some areas, e.g. on the northeastern insular

shelf that is sheltered from high wave assault (Hubbard, pers. comm.; Glynn et al., in press).

A central Pacific area that may share a link with the eastern Pacific is the Line Islands, an island chain located immediately north of the equator and centered about 1,600 km south of the Hawaiian Islands. These islands lie astride the North Equatorial Counter Current and may represent a source area for propagules traveling toward the east as proposed by Tom Dana (1975). Jerry Wellington and I are now planning a research trip to the Line Islands to study the ecology, morphology, and genetic structure of suspected scleractinian coral migrants.

CORAL BIOLOGY AND ECOLOGY

From the exploratory work outlined above and more focused studies, our understanding of eastern Pacific coral-reef biology and ecology has been greatly broadened in recent years. I offer here a thumbnail sketch of some of these recent advances, emphasizing findings that serve to characterize eastern Pacific reef-coral communities. In the following, I comment briefly on: (a) the nature of eastern Pacific coral reefs and their general community structure, (b) the dynamics of coral population abundances, (c) coral growth, (d) feeding relationships, (e) bioerosion, (f) coral reproduction, (g) disturbances, (h) zooxanthella symbiont diversity, and (i) coral population modeling.

Eastern Pacific Coral Reefs, a Profile

It quickly became evident from the surveys conducted in Central and South America that structural coral reefs were abundant in many areas of the eastern Pacific, albeit small in size, patchily distributed, and generally present at shallow depths. The established prevalence of coral reefs supports Durham (1947, 1966) and Squires (1959), who maintained that coral reefs were present along the Pacific coast of the Americas. Even though most of these reefs are developed at shallow depths, they seldom break the surface and are visible for only short periods during extreme low tidal exposures. Unlike coral reefs in other provinces, eastern Pacific reefs are best developed in protected bays or along coastal areas not subject to intense wave assault. The most prevalent taxa contributing to reef frameworks belong to species of *Pocillopora*, which form vertically elongate, interlocking, and highly porous structures. Since these reefs contain sparse amounts of binding crustose coralline algae and exhibit minimal submarine cementation, this may in large part explain why they are not developed on exposed coastlines. In upwelling areas, the best reef development also tends to occur on the sheltered sides of islands, oriented away from the strongest effects of upwelling (Glynn and Stewart, 1973). An additional interesting feature of most eastern Pacific reefs is that they are monogeneric in composition, constructed dominantly of one or only a few species. In addition to *Pocillopora* spp., some reef assemblages contain massive corals such as *Porites lobata*, *Pavona* spp., and *Gardineroseris planulata*

(Dana). See Guzmán and Cortés (1993), Cortés (1997), and Glynn (2001) for succinct overviews of the nature of eastern Pacific coral reefs.

Like having a favorite book, coral reef biologists often have a favorite reef. My favorite reef is the Uva Island reef in the nonupwelling Gulf of Chiriquí, which has been under constant study since 1970 (Figs. 7 and 8). Why? Because it has offered up so many answers to a long list of research questions. It also supports a high diversity of



Figure 7. Bird's eye view of the Uva Island study reef, Gulf of Chiriquí, Panamá during a midday low tidal exposure. February 8, 1989. (Photo C. M. Eakin)



Figure 8. Peter W. Glynn and Ian G. Macintyre preparing for a dive at the Uva Island study reef. February 22, 1989.

reef-associated organisms and is located in a beautiful embayment of a heavily forested, uninhabited island with a waterfall spilling onto a pebble beach. Adding to the excitement of our studies, elasmobranch sightings were common at the Uva Island reef in the 1970s. These included mostly white-tip sharks, bull sharks, and numerous manta rays. As many as a half dozen manta rays could be seen wheeling along the reef front where they were grazing on zooplankton.

Occasionally they became an annoyance by swimming into our float lines that marked study sites and dragging them seaward beyond the reef. These graceful animals, and the sharks, are now rarely seen. I must relate a bizarre encounter with one of these mantas, a behavioral maneuver I have not heard repeated.

One morning I was deeply engaged in a task that required close attention to the bottom. Although it was a sunny day, the light from above would momentarily dim as if from a passing cloud. After two or three such incidents, I looked toward Anibal, my diving partner. He motioned for me to look overhead. I had attracted a large manta, with a four-meter wing span, that was hovering above. I disengaged from my work and began

to watch the manta that was moving ever closer. It moved close enough for me to touch its mouth. It then began to press against me (I was wearing a black wet suit) and to envelop me with its wings, curling them around my body. When I observed a pair of claspers and realized that this was a male, perhaps in a precopulatory mode, I gently tapped it on the head with my dive knife. After a few additional taps, the manta moved back and slowly swam away.

Coral Population Abundances

With the discovery of new zooxanthellate coral records and new species in the eastern Pacific during the past few decades, generic and species diversity have increased from 5 to 10, and from 10 to 40+, respectively (compare Figs. 48 and 50 in Veron, 1995, with Table 1 in Glynn and Ault, 2000; Reyes Bonilla, in press). Some new records are instantly recognizable, such as the occurrence of hydrocorals or fire corals (*Millepora* spp.) in the Gulf of Chiriquí, Panamá. When I first observed these corals at the Uva Island reef in 1970 — and this was not difficult because they were everywhere — it was immediately obvious that they represented new species records because fire corals had not been previously reported from the eastern Pacific region. Upon further study, it was found that three species of *Millepora* were present on the Uva reef and elsewhere in the Gulf of Chiriquí. Two of these, *Millepora intricata* Milne Edwards and *Millepora platyphylla* Hemprich and Ehrenberg, are well known throughout the Indo-Pacific region. One of the fire corals proved to be a new species, first recognized by the late Prof. Dr. H. Boschma from specimens that I sent to him. All known colonies of this unnamed species bleached and died during the 1982-83 El Niño event. Realizing the importance of this mortality event, and not wanting the species to slip into oblivion — to become an anonymous or “centinelan extinction” (Wilson, 1992) — I invited Walentina (Wallie) H. de Weerd, one of Prof. Boschma’s disciples, to accompany me to Chiriquí to observe and collect additional dead colonies to describe the species. This we did (Weerd and Glynn, 1991), and the then presumed extinct new species became known as *Millepora boschmai* Weerd and Glynn. Since this disappearance of a reef-building coral species was the first ever — no other coral was known to have become extinct in recent times — it was reported in the journal *Science* (Glynn and Weerd, 1991). Five live colonies were rediscovered about a year later, on the north side of Uva Island (Glynn and Feingold, 1992). This site was subsequently christened Lazarus Cove by Mark Eakin. I was both elated to learn that *M. boschmai* was still alive and chagrined in having to retract the high profile claim of an extinction. The lesson learned: one must exercise extreme caution in announcing a species extinction in the marine environment, especially for a subtidal species capable of larval dispersal. Postscript: the five live colonies of *M. boschmai* again bleached and died during the 1997-98 El Niño-Southern Oscillation (ENSO) event. Thorough searches in Lazarus Cove and other likely habitats (as of March 2001) have failed to disclose any living colonies.

Extreme fluctuations in population abundances have been documented for several eastern Pacific zooxanthellate corals. The vulnerability of a high proportion of

the coral fauna can be appreciated from the following: (a) 15 of 41 species are known from only 1 or 2 localities; (b) many consist of small populations (< 100 colonies per site); and (c) 9 species have experienced recent local-to-regional scale extinctions (Glynn, 1997; Glynn and Ault, 2000). Some of my colleagues have grouched that it is difficult to follow the population status of particular species from my publications. This is true, and it is no less difficult for me in light of the rapidly changing abundances that are observed between censuses. Coral population structure at many localities is indeed dynamic.

Another enigmatic occurrence has involved the appearance, and sudden disappearance, of *Acropora valida* Dana at Gorgona Island, Colombia (Prahl and Mejía, 1985). The “curse of *Acropora*,” as it has become known, is a haunting malediction not soon to be forgotten. It started as a remarkable discovery and ended in terrible tragedies. Sightings of *Acropora* off western American shores had been rumored for many years, but never substantiated. Then on 8 September 1983, three colonies of *Acropora valida* were discovered by Angela Mejía, one of Henry von Prahl’s students, at Gorgona Island, Colombia. The publication of this discovery created quite a stir among the cadre of eastern Pacific cora-reef workers. Many questions concerning these corals were being asked and follow-up studies planned. Sadly, Angela suffered a severe motorcycle accident in 1988, which totally beclouded her memory of this remarkable find. Then in 1989, Henry von Prahl, the remaining link to the discovery, fatefully boarded an Avianca flight from Bogotá to Cali on which he and all others perished in a midair explosion, the pusillanimous act of a terrorist. *Acropora* colonies have not been found subsequently at Gorgona Island or anywhere else in the eastern Pacific. To this day, some superstitious-leaning workers are both hopeful and fearful of making such a discovery.

A final example of an extremely rare species is *Siderastrea glynni*, named in my honor by Ann (Nancy) Budd and Héctor Guzmán (Fig. 9). This species was found at Urabá Island in Panamá Bay, and the only known population consisted of just five colonies (Budd and Guzmán, 1994).



Figure 9. Live colony of *Siderastrea glynni*, about 11 cm in diameter, in an aquarium. March 16, 1993. (Photo C. M. Eakin)

Since the condition of these colonies began to deteriorate during the 1997-98 ENSO event, displaying bleaching and tissue loss, Héctor transported four of the remaining colonies to Naos Island. Here they are being cared for, and are showing signs of recovery, in aquaria of the Smithsonian Tropical Research Institute. Following the discovery of this species, extensive surveys in presumably suitable habitats have failed to reveal additional populations. What is the origin of this species? Are there

source populations elsewhere (locally or distant), perhaps in unexpected habitats? Are the five colonies survivors of a large population, now destined to extinction? This is a sample of the questions raised by this puzzling discovery.

Coral Growth

Once it was established that coral reefs were widespread in the eastern Pacific, the next task was to measure the growth rates of a variety of reef-building species to determine if these were comparable with coral growth elsewhere in the central and western Pacific where reefs are better developed. Pocilloporid corals, the chief reef builders, were found to have vertical skeletal extension rates of 3 to 6 cm yr⁻¹ (Glynn and Stewart, 1973; Glynn, 1977). During periods of moderate upwelling, coral growth was considerably less, around 1 to 2 cm yr⁻¹, and growth ceased altogether at temperatures of 18°C or lower. Massive corals typically grow more slowly than branching species, and the mean outward-growing skeletons of different massive species range from about 0.8 to 1.7 cm yr⁻¹ (Glynn and Wellington, 1983; Guzmán and Cortés, 1989). The general conclusion emerging from these studies is that eastern Pacific corals are capable of rapid growth and that their growth rates are approximately equivalent to those of similar or identical species in the central and western Pacific.

Feeding Relationships

As Birkeland (1977) noted in his study of coral recruitment in relation to competition with other benthic organisms — such as algae, sponges, bryozoans, barnacles, and tunicates — slow-growing corals do not compete well with other fast-growing taxa in nutrient-rich environments. Selective grazing by fishes and invertebrates, if sufficient, can tip this balance in favor of corals by removing potentially superior competitors that would otherwise overgrow the corals. Beyond Birkeland's early study in the Gulf of Panamá, no additional work has been carried out in the eastern Pacific on the quantitative relations between grazing pressure and coral recruitment.

Another aspect of feeding ecology relating to animals that feed on the living tissues of corals (corallivores) has received considerable attention, perhaps because of the unusual numbers of these consumers on eastern Pacific reefs. These range from micropredators, roughly equivalent to parasites, to macroconsumers that are capable of digesting all tissues from sizable coral colonies. The variety of corallivores that have been studied are species of gastropods, crustaceans, echinoderms, and fishes (Glynn, 1982a; Guzmán, 1988).

Acanthaster planci (Linnaeus), a large sea star that feeds almost exclusively on corals, occurs from the Gulf of California to Colombia (Malpelo Island) and Ecuador (northern Galápagos Islands). Usually, only single individuals are occasionally seen at the latter two localities, suggesting a transient existence in its southernmost range. Moderately large numbers (20 to 30 inds ha⁻¹) are sometimes observed on reefs in the Gulf of Chiriquí, but population outbreaks — on the scale observed at several central and western Pacific reefs (100s to 1000s inds ha⁻¹) — have not been reported in the eastern Pacific. A clue to how the numbers of this sea star might be controlled came from frequent sightings of particular shrimp and worms with *Acanthaster*. Careful observations revealed that the harlequin shrimp *Hymenocera picta* Dana and the amphinomid polychaete worm *Pherecardia striata* (Kinberg) were actually attacking



Figure 10. The harlequin shrimp, *Hymenocera picta*, attacking the crown-of-thorns sea star *Acanthaster planci* on the Uva Island reef, 3 m depth. Shown is one of a ♂/♀ male pair that was following the sea star for several days and removing its organs (hepatic caeca and gonads). December 19, 1982

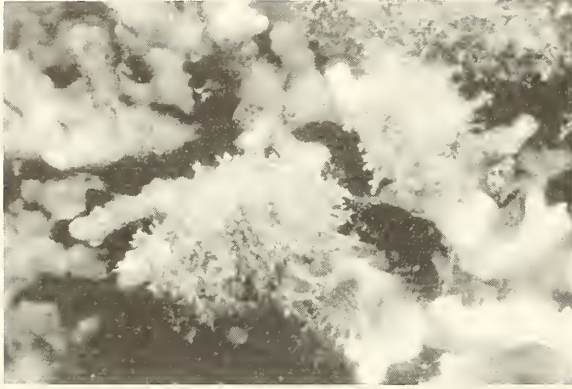


Figure 11. A scavenging polychaete worm (*Pherecardia striata*) feeding on the dismembered arm of a sea star, Uva Island reef, Panamá, 2 m depth. December 19, 1982

that repel corallivores and grazing fishes — have offered a rich source of material for documenting the intricacies of some of the biotic interactions affecting coral-community structure. The gastropod corallivore *Jenneria pustulata* (Lightfoot) can sometimes attain high local abundances where it can consume all of the tissues of large colonies of *Pocillopora* (Glynn, 1984b). *Jenneria* is truly a beautiful animal, and one cannot help but imagine that its polyp-like mantle extensions and black-encircled, orange shell pustules are not somehow involved in mimicking its coral prey (Fig. 12). A variety of fishes feed on *Jenneria*, at least when it can be detected. The tenacity with which crustacean guards (*Trapezia* spp. and *Alpheus lottini* Guérin) defend their coral hosts from *Acanthaster* attacks, and their repertoire of defensive strategies (Fig. 13), are nothing short of amazing (Glynn, 1983). The guards can even detect an approaching

Acanthaster, in lilliputian style (Glynn, 1982b). The ~3-cm-long shrimp, usually a female-male pair bond, typically rides on the upper surface of ~30-cm-diameter sea stars (Fig. 10). I have spent hours to days following individual *Acanthaster* under attack by these shrimp. By means of their sharp chelae, the shrimp pick and tear at the sea star's body until breaking through to the organs within. They then remove and feed on hepatic caeca, gonads, and various other soft parts. Then enter the 5- to 10-cm-long worms (Fig. 11), which come streaming out of the porous reef — upwards of 200 to 400 inds m⁻² (Glynn, 1984a). The worms crawl into the sea star through the incisions made by the shrimp. The worms also consume the sea star's soft parts, reaching virtually all internal recesses. These attacks cause slow death, from a few to several days, but occur so frequently they would seem to exercise an important control on adult sea star numbers.

Other intriguing feeding relationships — involving ovulid gastropod and pufferfish corallivores, and crustacean guards and damselfishes



Acanthaster by means of waterborne chemical cues and ready themselves for a defensive attack by scurrying along the outermost colony branches (Glynn, 1980). A variety of field observations

Figure 12. *Jenneria pustulata*, an ovulid gastropod corallivore stripping the tissues from *Pocillopora damicornis* at night on a coral reef in the Secas Islands, Gulf of Chiriquí, Panamá, 5-m depth. July 19, 1981.

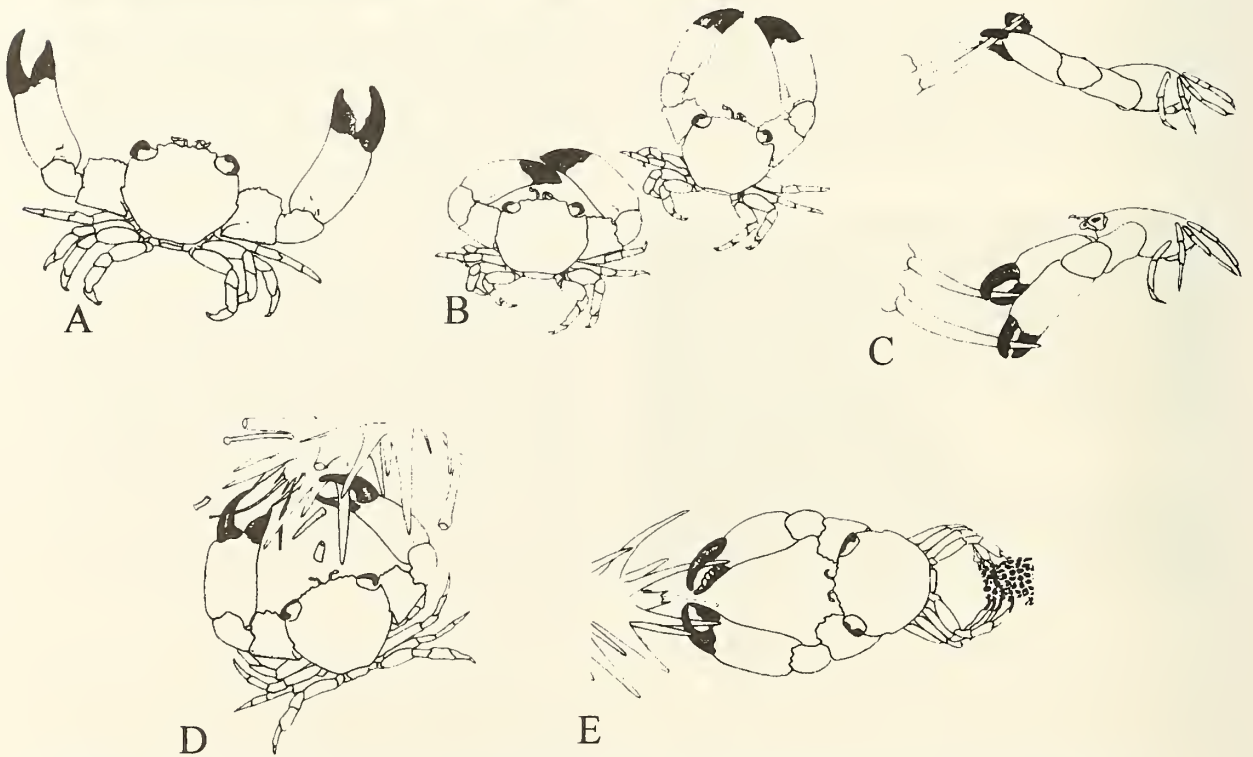


Figure 13. Defensive behaviors of a *Trapezia* crab guard directed toward the crown-of-thorns sea star. (A) startle display; (B) pushing; (C) up-down jerking of sea star; (D) pinching and clipping of sea star's spines and tube feet; (E) resisting retreat of sea star (modified after Glynn, 1983).



and experiments have been performed to document these behaviors (Fig. 14).

Figure 14. Peter W. Glynn investigating the responses of crustacean guards to a model Crown-of-Thorns sea star and the 'juices' of a living sea star, Uva Island study reef. January 14, 1980.

Bioerosion

In 1980, Ray Highsmith published an insightful paper relating bioerosion to areas of high nutrients and plankton primary productivity. This relationship was based primarily on a correlation of sites of varying productivity with the numbers of boring bivalves in coral skeletons. He ranked the eastern Pacific as the coral-reef region most severely affected by bioerosion. Basically, because most bioeroders prosper in high nutrient environments with high concentrations of plankton, their growth and abundance overwhelm corals that generally do well in less productive settings. Several studies have substantiated this pattern and have quantified the high rates of reef erosion, not only by internal bioeroders (e.g., cyanobacteria, sponges, worms, and bivalves), but also by external bioeroders (e.g., crustaceans, mollusks, echinoderms, and fishes). Bioerosion accelerates in dead corals, rapidly weakening and reducing the size and integrity of limestone skeletons. This sort of damage often leads to frustration when attempting to secure a complete core from an old coral colony (see below, Coral and Reef Growth History). The bases of such colonies, with the earliest (and oldest) growth records, are often riddled with bioeroders or are completely missing.

Galápagos Islands coral reefs that suffered 95 to 99% mortality during the 1982-83 El Niño warming event were attacked continually by large numbers of the blunt-spined sea urchin *Eucidaris galapagensis* Döderlein. This sea urchin feeds on algae that colonize the dead coral skeletons and, in the process, its sharp-cutting teeth erode the dead coral surfaces. It was disheartening to witness the relentless breakdown and disappearance of the Galápagos reefs in less than 20 years following the El Niño disturbance (Glynn, 1994; Reaka-Kudla et al., 1996). Erosion of the Uva Island reef in Panamá following coral mortality in 1982-83 was not so dramatic as in the Galápagos, but nonetheless substantial, particularly in certain reef zones (Eakin, 1996). In Panamá, the most influential surface bioeroder is *Diadema mexicanum* A. Agassiz, the black-spined sea urchin. Reef-base zones with low abundances of damselfish are especially susceptible to sea urchin erosion, whose numbers commonly exceeded 50 inds m⁻². Damselfishes cultivate algal lawns and remove sea urchin competitors from their territories, thereby protecting portions of the reef frame (Eakin, 1987, 1988; Glynn, 1988). Eakin's (1996) CaCO₃ budget for the 2.5 ha Uva reef revealed an average net erosion rate of 4,800 kg yr⁻¹ in the mid-1990s, compared with a net deposition of 8,600 kg yr⁻¹ prior to the 1982-83 El Niño event. An updated model suggests the Uva reef is still in an erosional mode, but that extreme La Niña-related low-tidal coral mortalities in 1989 and 1993 had a greater influence on reef erosion than coral mortalities resulting from the 1997-98 ENSO bleaching event (Eakin, 2001).

Thus, Highsmith's (1980) contention that rates of bioerosion are elevated in high nutrient/primary productivity environments is still valid, but these rates are accelerated during periods of high coral mortality resulting from a variety of disturbances such as El Niño warming and low sea-level stands.

Coral Reproduction

Many hundreds, if not thousands, of human hours have been devoted to the study of coral reproduction by my research team. The first in-depth study by Bob Richmond (1985) failed to detect planulation in colonies of *Pocillopora damicornis* monitored weekly in Panamá for nearly a year. Central and western Pacific populations of this species are highly fecund, releasing larvae monthly throughout the year. Because of the apparent absence of spawning (planulae or gametes) in eastern Pacific *P. damicornis*, Bob concluded that these populations were essentially asexual and had evolved life-history characteristics favoring clonality. Bob further argued that these isolated “sterile” populations would occasionally receive sexual larval recruits by long-distance dispersal, especially during El Niño years when west-to-east flowing currents increase in volume and rate (Richmond, 1990).

Continuing studies in Costa Rica, Panamá, and the Galápagos Islands have revealed high levels of gamete production in *P. damicornis* and all other reef-building coral species studied to date (Glynn et al., 1991, 1994, 1996, 2000). Only *Porites panamensis* releases planula larvae, with most, and perhaps all, of the remaining zooxanthellate species spawning gametes. While *P. damicornis* is highly fecund, producing large numbers of mature eggs and sperm at these locations, we have never observed this species spawning. As of this writing, only three eastern Pacific species have been observed spawning gametes, namely, *Pavona gigantea*, *Pavona varians*, and *Pavona* sp. a. The latter species, closely related to *P. varians*, is presently being named (Glynn et al., 2001). Histological studies of several other species have revealed mature gametes in abundance, but no sign of planula larvae. Late one sunny afternoon in the Galápagos Islands, near full moon during a peak high tide, several colonies of *P. gigantea* were observed shedding clouds of eggs and sperm. Upon revisiting this population in the following and subsequent years, under virtually identical conditions, no additional spawning was observed.

Over the years, we have scheduled diving teams to watch for spawning on the Uva Island coral reef at all times of the day and night, performing late-night and early-morning observations. Meal times have been partly to blame for not detecting spawning sooner in two coral species. After a full day of diving-related work, none of us was interested in coaxing the ship’s cook to schedule dinner later than about 1800, at sunset or a little later. Also, if a night dive was planned, it was better to eat early to allow time for one’s stomach to settle. As it turned out, we continually missed the spawning of *Pavona* sp. a, which occurred shortly after sunset, at peak high tide a few days following the full moon. Then we were chagrined to learn that breakfast had interfered with observing spawning in *Pavona varians*, again near peak high tide a few days following full moon, but just before sunrise. It turns out that this 12-hour spawning separation prevents these sibling species from hybridizing. This difference in the timing of spawning serves as a clear-cut trait to help distinguish between these closely related species.

While these studies are contributing to our base of knowledge, there is still a plethora of unanswered questions associated with the reproductive biology of corals.

For example, more information is needed concerning spawning behavior and its relationship with water-column microscale mixing processes, lunar periodicity, the seasonality of spawning, fecundity, and the length of larval lives and their dispersal capabilities. Virtually nothing is known of the reproductive biology of eastern Pacific hydrocorals (*Millepora* spp.), the most sensitive of all reef-building species to ENSO warming disturbances. A big mystery is the discrepancy between the high level of sexual activity in many species and their typically low recruitment success. What is occurring between the time of mature gamete production and recruitment? Our histological studies show no gamete resorption. It is therefore reasonable to assume that spawning most likely occurs? Is mortality high in the water column before settlement, or are early benthic stages being killed before recruitment? The answers to these questions are crucial for an understanding of coral-community development and the capacity for recovery following various sorts of disturbances.

Disturbances

Cool currents and local upwellings, long believed to be the leading limiting factors affecting coral growth, are now recognized to be but two of numerous conditions that can influence eastern Pacific reef development. The two major temperate current systems that limit the northern and southern migration of reef-building corals are the California Current off the west coast of Baja California and the Peru Coastal Current near the Peru-Ecuadorean border, respectively. As has been amply demonstrated, however, the three major upwelling centers — the Gulfs of Tehuantepec, Papagayo, and Panamá — do support abundant coral populations and reef development.

Since ENSO disturbances, including primary and secondary effects, were not recognized until the dislocations documented during and following the 1982-83 ENSO, the realization of the multifarious effects of this natural perturbation could be regarded as the most important revelation of the twentieth century concerning impacts to coral reefs. Mechanically induced mortalities of corals and reef associates by storms and increased wave assault are more likely during ENSO activity (Robinson, 1985; Lirman et al., 2001). The immediate effects of El Niño warming (i.e., the loss of zooxanthellae leading to coral bleaching and mortality) may be followed by several longer-term secondary effects such as: (a) increased emigration and mortality of obligate coral crustacean symbionts (Glynn et al., 1985); (b) the disruption of live-coral barriers that prevent *Acanthaster* from entering and feeding on patches of preferred prey (Glynn, 1985); (c) predator concentration on surviving corals (Glynn, 1990; Guzmán and Cortés, 1992); (d) interference with coral reproduction and reduced recruitment (Glynn et al., 2000); and (e) the invasion of dead coral patches by echinoids and damselfishes that interfere with coral regeneration and perhaps larval settlement (Wellington, 1982; Glynn, 1990). Other effects that can interfere with corals are related to La Niña or the ENSO cool phase, such as (a) dinoflagellate blooms (Guzmán et al., 1990); (b) extreme low tidal exposures (Eakin et al., 1989; 2001); (c) stressful upwelling events (Glynn and D’Croz, 1990); and (d) the proliferation of macroalgae that compete with, and sometimes overgrow, corals (Glynn and Maté, 1997).

Another type of seriously degrading impact to coral reefs worldwide — i.e., sedimentation and eutrophication — is increasingly observed in the eastern Pacific. This is especially true at low-latitude coastal areas with high rainfall. Rampant clear-cutting, with little or no concern for land management, has transformed verdant forests and mangrove shores to croplands, pasturage, aquaculture ponds, and urban sprawl. Two of my former students, Jorge Cortés and Bernardo Vargas Ángel, have documented this kind of damage to coral reefs in Costa Rica and Colombia, respectively. Several reef scientists attending the Eighth International Coral Reef Symposium in Panamá in 1996 were reminded of how a damaged watershed can have far-reaching effects on coral reefs. Rainfall was excessive a few weeks before a post-symposium field trip to coral reefs in the Gulf of Chiriquí, an outing I helped arrange to showcase our study reefs to a cadre of international reef researchers. Unfortunately, a period of heavy rainfall before the excursion caused extensive soil erosion and river runoff with silt-laden plumes extending over most of the continental shelf. Our field trip was literally a washout with water visibility often ranging between only 30 to 100 cm.

Sadly, poor land use in coastal areas has greatly diminished three high-diversity ecosystems: lowland rainforests, mangrove shores, and coral reefs. It is becoming increasingly difficult to swim over pristine coral reefs and to hear howler monkeys and squawking parrots in the adjacent forest. Some additional disturbances to coral reefs and environmental concerns, mainly related to human activities, are addressed below (see, *The Future of Eastern Pacific Coral Reefs*).

Zooxanthellae Diversity

Molecular genetic studies have revealed that scleractinian corals harbor a diversity of algal symbiont taxa (Rowan and Powers, 1991; Rowan and Knowlton, 1995; Baker and Rowan, 1997; Rowan, 1998; Baker, 1999). Some intriguing patterns of bleaching resistance in relation to symbiont distribution in scleractinian corals have been recently documented. For example, in Caribbean *Montastraea*, much of the intraspecific variation in response to a natural bleaching event in 1995 was explained by the distribution of symbiont taxa (Rowan et al., 1997). Additionally, one of four symbiont taxa identified in Panamanian *Pocillopora* spp., a member of *Symbiodinium* clade D, was especially resistant to bleaching caused by elevated temperature and high irradiance stress during the 1997-98 El Niño warming event (Baker, 1999; Glynn et al., 2001). In contrast, the hydrocoral *Millepora intricata* is often the first to bleach during periods of high-temperature stress, and suffers high rates of mortality. In Panamá, *Millepora* hosts unusual symbionts (*Symbiodinium* clade A), which have not to date been documented in scleractinian corals from the far eastern Pacific (*Symbiodinium* clade A), an observation that is intriguing in the context of its bleaching susceptibility (Baker, 1999).

Some workers have suggested that bleaching may provide corals with a mechanism for the removal of sensitive symbionts and replacement by more resistant alternatives (Rowan and Powers, 1991; Buddemeier and Fautin, 1993). This stress response — termed the “adaptive bleaching hypothesis” — remains controversial, partly

for want of direct empirical investigation (but see Baker, 2001). Certainly, some colonies with resistant symbionts fared better during the 1997-98 El Niño than colonies without. However, it is not clear whether bleached corals that contained susceptible symbionts, but still managed to survive, have shown any change in their symbiont communities since recovering from the bleaching event.

Coral Population Modeling

With the availability of long-term data on various aspects of a coral population (namely, growth, recruitment, predation, El Niño-related mortality, and recovery), Peggy Fong and I have developed a dynamic simulation model of the abundance and size structure of *Gardineroseris planulata* on the Uva Island coral reef (Fong and Glynn, 1998). Results of this initial simulation effort demonstrated that predation by *Acanthaster planci* was of overwhelming importance to coral population structure on the Uva reef due to both stronger effects of predation (larger transitions) and the frequency of predation (yearly) compared with ENSO (episodically) disturbances. This model was modified for use at the regional scale to incorporate diverse environmental settings of temperature and predation in nonupwelling and upwelling areas of Panamá and the Galápagos Islands (Fong and Glynn, 2000). Results of the regional model suggest that ENSO impacts can be accurately predicted by the rate of temperature change in an area during an ENSO event. Validation of model predictions in upwelling and nonupwelling environments during the 1997-98 El Niño event is encouraging and signals an understanding of the critical processes that regulate coral population structure (Fong and Glynn, 2001). As we continue to gain confidence in this effort, we plan to expand the modeling to include multiple coral species populations and other aspects, such as nutrient availability, algal/coral competition, and herbivore grazing effects.

One problem inherent in studies of coral-reef ecology is that most reefs have been studied over relatively short ecological time scales (Jackson, 1997), while many processes controlling coral population and community dynamics occur over much longer periods. Simulation modeling is one tool that can be used to synthesize knowledge of processes investigated on ecological time scales in order to develop predictions on a longer time scale (Ebenhöh, 1994; Jørgensen, 1994). The effects of ENSO events in the tropical eastern Pacific have been studied over a relatively long time (~30 years) compared with ecological studies in many other reef systems (Connell, 1997). These data provide a unique opportunity to develop simulation models that can be used to predict the condition of coral populations, communities, and ecosystems in the future.

CORAL AND REEF GROWTH HISTORY

Since many of the newly found eastern Pacific reefs were well developed, some exhibiting vertical thicknesses of several meters, we began to wonder how old they might be. We started to investigate this question by core drilling, using the same

underwater hydraulic drill, assembled by Ian Macintyre, that worked so effectively on Caribbean coral reefs. However, due to the large number of cavities and fragile structure of pocilloporid reefs, which are the main reef types present in Panamá, we found that the drill usually fell rapidly through the coral framework without retaining sufficient material for study. Occasionally we encountered a massive coral species near the reef basement, which could be dated to reveal the age of one small section of the reef. These preliminary efforts disclosed maximum reef ages of 4,500 to 5,600 years BP and vertical framework thicknesses of 10 to 13 m (Glynn and Macintyre, 1977). Jorge Cortés, my first doctoral student, core-drilled a poritid reef in Costa Rica (Cortés et al., 1994), which also revealed a comparable age (4,000-5,500 years) and vertical buildup (4-10 m). These measurements compared reasonably well with the ages, vertical buildups and rates of CaCO_3 production in other coral-reef regions (see Table 24 in Glynn and Wellington, 1983).

In order to develop a sense of the extent of pocilloporid reef development under different environmental settings and over larger areas, we began to probe these reefs with iron pipes, an idea suggested by Ian Macintyre. We utilized sections of black iron pipe of high carbon content, fitted with stainless steel bits. These were driven into reefs with a sledge hammer, and by rotating the drill string with clamps outfitted with handles. This probing was generally timed to take advantage of low tides, so that some of the work could be completed on drying reef flats or in waist-deep water. Aníbal Velarde, my steadfast assistant at STRI, did much to advance this work. On one occasion, by brandishing a pipe and sledge hammer, he averted an imminent mugging while we were probing a reef on Pedro Gonzalez Island, Pearl Islands. Our pipe sections and probing tools thus served not only the drilling work, but protection from hooligans as well.

After acquiring a sense of the time over which reef growth has occurred, and the rate of limestone accretion, it became of interest to gain some knowledge of the relationship between reef-growth history and the environmental setting of particular eastern tropical Pacific sites. An approach that would allow the investigation of this subject was the discipline of sclerochronology, which had its beginnings in a pioneering study by John Wells (1963). Work on this topic in the eastern Pacific was initiated mainly by my colleagues and me in Panamá, Costa Rica, and the Galápagos Islands. We began by core-drilling and dissecting the aragonitic skeletons of massive corals, many ranging in age from about 100 to 365+ years (Fig. 15). By determining the chronology of interannual growth records and skeletal hiatuses, it has been possible to identify periods of accelerated, retarded, and interrupted coral growth. Armed with the necessary analytical tools to measure certain chemical tracers incorporated into the lattice structure of aragonitic skeletons (e.g., O and C isotopes, trace metals such as Mn and Cd, and fulvic and humic acids), my co-workers have been able to relate periods of coral growth with environmental change. For example, analysis of oxygen isotopic fractionation has allowed the identification of past ENSO events (Carriquiry et al., 1988; Druffel et al., 1990; Wellington and Dunbar, 1995). Additionally, stable oxygen isotopic



Figure 15. Core drilling a large colony of *Porites lobata*, Secas Islands, Gulf of Chiriquí, Panamá. Left to right: Peter W. Glynn and Aníbal Velarde. July 23, 1984.

convergence zone (Linsley et al., 1994). While such paleoclimate studies help advance our understanding of the environmental bounds of reef growth, the extent of these investigations in the eastern Pacific is confined by a generally poor fossil record and high rates of bioerosion following coral death.

Another approach that can reveal information on the developmental history of coral communities involves a detailed examination of reef sediments. We have obtained sediments from the Uva reef by air lifting and from push cores (Fig. 16). One must



Figure 16. Air-lifting sediments at the Uva Island study reef, Gulf of Chiriquí, Panamá. Left to right: Peter W. Glynn, Aníbal Velarde, and Aaron Yedid. July 18, 1984.

exercise caution that the reef sites selected for study have not been subject to violent storms or burrowing organisms, both factors that could disrupt the sequence of sedimentary strata. The C-14 dating of carbonate sediments from the Uva reef have not shown any time reversals, suggesting minimal mixing of shallow and deep sediment layers. Sediments obtained from 2.5 m depth ranged from $1,645 \pm 300$ to $3,830 \pm 300$ years in age. The *Acanthaster* skeletal remains throughout the sedimentary strata are equal in abundance to the sea star remains found in surface sediments. This suggests the presence of low-to-moderate *Acanthaster* abundances (15-30 inds) on the Uva reef during the past ~4,000 years.

signatures in a 365+-year-old Galápagos coral have permitted detection of the Little Ice Age, revealing low sea temperatures during the early 1600s and early 1800s (Dunbar et al., 1994). Further, specific geochemical indicators can also provide information on variations in salinity, rainfall, river outflow (Dunbar et al., 1994; Linsley et al., 1994), upwelling and nutrient availability (Shen and Sanford, 1990; Shen et al., 1992), the timing of volcanic eruptions (Shen et al., 1991), and shifts in the position of the intertropical

ENTER EL NIÑO

After accepting a position at the Rosenstiel School of Marine and Atmospheric Science (University of Miami), I arranged a research cruise to revisit study sites in the Gulf of Chiriquí in March 1983. In anticipation of moving to Florida, I began making plans to return to coral reef studies in the western Atlantic. Therefore, before leaving Panamá I thought it would be appropriate to sample, perhaps for the last time, some Pacific reef sites that had been under continuous study since the early 1970s. Much to my astonishment, the Uva Island coral reef appeared to have been brushed by a snowfall. All corals were bleached bone white. Surveys on other coral reefs in Panamá, including both nonupwelling and upwelling environments, revealed the same severe bleaching effects. Corals began dying and by the end of the disturbance event, just a few weeks later, overall mortality in Panamá amounted to 75 to 85% of the total live cover. Several of my colleagues in Costa Rica, Colombia, and the Galápagos Islands were notified of this mass bleaching event and asked if anything similar had been observed in their study areas. It was soon realized that this coral bleaching and mortality disturbance was regional in scale, occurring throughout the eastern equatorial Pacific. Coral mortality was greatest in the Galápagos Islands, with 95 to 97% overall mortality.

Initially I was uncertain about the cause(s) of this unprecedented bleaching event. Before learning of its widespread extent, I thought it might have been a result of human pollution, possibly by pesticides. In the summer of 1983, while coral bleaching and mortality were still in progress, I was scheduled to participate in the Coral Reef Population Biology course offered by the Hawaii Institute of Marine Biology. Armed with underwater photographs of the bleaching event and observations of the immediate responses of corals and other reef associates, I presented this information to the summer class. During the lively discussion that followed, Paul L. Jokiel asked if we had considered elevated sea temperatures as a possible causative agent of bleaching. Since the disturbance was taking place during the 1982-83 El Niño event, which was accompanied by high sea-temperature anomalies, this seemed like a reasonable lead to investigate. An analysis of the timing, spatial extent, and rate of increase of sea-surface temperature anomalies showed a remarkably close correspondence with the patterns and severity of coral mortality (Glynn, 1984c; Glynn et al., 1988). These findings prompted an experimental study by Luis D'Croz and me, designed to assess the effects of slightly elevated water temperature on coral vitality and survivorship. It was found that controlled temperatures, mimicking the warming and duration that occurred during the El Niño event, also promoted coral bleaching and death, thus validating the field observations (Glynn and D'Croz, 1990). For this publication, Luis and I received the Best Paper Award for volume 8 (1990) of the journal *Coral Reefs*.

Large-scale bleaching and mortality can have important effects at the ecosystem level. Since coral reefs are built dominantly by zooxanthellate corals (calcareous algae play a minor role on most eastern Pacific reefs), and the high biotic diversity of reefs is largely a result of the shelter and trophic resources they offer, the reduction in abundance of these foundation species can have notable cascading effects. Of the

several secondary disturbances that followed the high coral mortality in 1982-83, the continued bioerosion of dead corals was perhaps the most serious. Sea urchins and fishes grazing on the algal cover of dead corals and internal bioeroders, such as sponges and bivalves, converted dead reef surfaces to sediment. This led to the complete elimination of reef structures in the Galápagos Islands (Glynn, 1994; Reaka-Kudla et al., 1996) and to the destruction of large sections of reefs along coastal Ecuador (Glynn, in press), in Panamá (Eakin, 1996), and at Cocos Island, Costa Rica (Guzmán and Cortés, 1992). Long-term secondary effects such as bioerosion, loss of firm surfaces for coral settlement, corallivores, and phase shifts to algal-dominated communities were considered earlier in more detail (see Disturbances).

Since the first documented coral-bleaching event of 1982-83, several others associated with ENSO warming have been reported with varying effects at other localities in the eastern Pacific in 1986-87, 1991-92, 1995, and, most recently, in 1997-98. It is plausible that repeated El Niño disturbances during the recent geologic past have limited coral community diversity and the development and persistence of significant reef structures in the eastern Pacific (Glynn and Colgan, 1992). Of greater concern is the possibility that eastern Pacific warming disturbances are a harbinger of change toward a greenhouse world that could affect coral reefs globally.

While nominees are never privy to the discussions that decide awards, I suspect that my research into the causes and consequences of widespread El Niño-induced coral disturbances played a pivotal role in receiving the Charles Darwin Medal in 1992, the most prestigious honor bestowed by the International Society for Reef Studies.

THE FUTURE OF EASTERN PACIFIC CORAL REEFS

Until the ENSO impacts of the last two decades, it was generally held that eastern Pacific coral reefs were threatened mostly by anthropogenic activities against a background of localized natural disturbances. Diverse human-related disturbances have been associated with declines in coral abundance and the degradation of whole reefs. As noted earlier, deforestation and soil erosion were perhaps responsible for the greatest damage to reefs in Costa Rica, Colombia, and Panamá. Other types of damage in México, mainland Ecuador, the Galápagos Islands, and the aforementioned countries have resulted from coastal construction projects, destructive fishing, the collection of corals for sale as curios, boat groundings, anchor damage, and the release of contaminants (such as oil, detergents, and pesticides) in reef areas. Adverse natural disturbances, as noted earlier, include ENSO warming events, low tidal exposures, tectonic events (resulting in coastal uplift and landslides), hurricanes, and a variety of biotic effects such as bioerosion and predation. I have personally observed the negative effects of the majority of these various sources of disturbance, with increasing frequency in recent years.

Coral bleaching and mortality resulting from ENSO-related elevated temperatures appear to be greater on offshore than on nearshore coral reefs in the equatorial eastern Pacific (Macintyre et al., 1992; Glynn et al., 2001). If this pattern is

substantiated by continuing studies, it will be even more urgent that nearshore coral assemblages be protected from human disturbances, which are more likely near mainland population centers. Such inshore areas could serve as refugia for some species that might suffer higher mortalities in more offshore settings. With 22 existing and 6 proposed marine-protected areas with coral assemblages and/or coral reefs in the eastern tropical Pacific (the majority of these are located in nearshore environments), several Latin American governments now recognize the importance of conserving natural resources and safeguarding high-diversity coastal ecosystems (Glynn, 2001).

Some incidences of human-induced damage have resulted from ill-advised good intentions. One such example occurred on a popular snorkeling reef at Huatulco, México. Since dive boats frequently visited this reef and indiscriminantly dropped anchors on live corals, an effort was made to position mooring lines along the reef edge to prevent anchor damage. Unfortunately, the placement of concrete blocks for fastening the mooring lines was not properly supervised and these were dropped directly onto the living coral framework, causing extensive localized damage.

Every now and again during my tenure at STRI (Panamá), the monstrous proposal of constructing a sea-level canal would raise its ugly head. This usually occurred during drought years when freshwater supplies became low, thus threatening the operation of the Panama Canal locks. Unless properly barriered, a sea-level canal would allow the movement of marine organisms, including predators, toxic species, parasites, and pathogens, from one ocean to the other. Since the two tropical seas have been separated for over 3 to 3.5 million years, allowing for substantial evolutionary divergence, the sudden rejoining of biotas would likely result in unpredictable biotic impacts. In light of the many environmental and economic problems that followed in the wake of previous large-scale ecological changes, such as construction of the Aswan Dam and the Suez and Welland Canals, the U.S. National Academy of Sciences Committee on the Ecology of the Interoceanic Canal considered an unrestricted breaching of the Panamic Isthmus by a sea-level canal totally unacceptable (Newman, 1972). It will be necessary to revive this important recommendation in the face of future threats.

Even if effective biotic barriers are in place, there are other ways that accidental or intentional introductions can occur, and all of these should be guarded against with vigilance. For example, exotic species can be introduced accidentally via ship's ballast water or from fouling communities, and in association with species intended for aquaculture or stock enhancement. Unfortunately, deliberate introductions have been made by some tropical aquarium enthusiasts who have released live, nonnative species into both Caribbean and Pacific reef waters. And even the scientific community is not exempt from such poor judgment. I won't mention names, but one of my colleagues once purposely introduced Indo-Pacific coral and molluscan reef species onto a Caribbean reef and another colleague suggested that it would be interesting to release the predatory sea star *Acanthaster* onto Caribbean reefs to observe its feeding behavior. In the first instance, all (hopefully all) of the introduced species were later found and removed by concerned reef workers. In the second example, my forceful objections prevailed and the crown-of-thorns sea star still remains an Indo-Pacific species, and a significant pest problem.

In recent years, examples of successful coral-reef management programs have been realized. Often a critical ingredient of this success is the involvement of local public support, which implies an understanding of conservation principles. An educational program designed to convey the benefits of coral reefs and how best to protect them is an important initial step in this effort. It is my hope that, by instructing and mentoring undergraduate and graduate students from various countries bordering the eastern tropical Pacific (Colombia, Costa Rica, Ecuador, México, Nicaragua, Panamá), I have made some contribution toward this end. In actuality, I am proud to announce that some of my former students are now engaged in coral-reef conservation and management in their home countries. In the pedagogical arena, there is no greater feeling of accomplishment than to know that one has influenced students to enter one's own discipline and, in my case, to see those students excel and become internationally recognized in coral-reef biological studies (Fig. 17).

How to end this essay? Let me propose one of my favorite quotations on the purpose of life, Preston Cloud's salubrious ethic, "to live it with as much grace and integrity as possible, to enjoy and improve it while you have it, and to leave the world no worse for your having been there." (Cloud, 1988). I believe that Preston Cloud left the world a little better off, and if each of us could make some contribution toward preserving coral reefs, then our brief stay on the Blue Planet would have been worthwhile.



Figure 17. Peter W. Glynn surrounded by a cadre of coral-reef colleagues, including former and present-day RSMAS, MBF (Rosenstiel School of Marine and Atmospheric Science, Division of Marine Biology and Fisheries, University of Miami) students, who attended the Ninth International Coral Reef Symposium in Bali, Indonesia (23-27 October 2000). Top row (left to right): Iliana Baums (MBF Ph.D. student), Joshua Feingold (Ph.D., 1995, MBF, present address: Nova Southeastern University Oceanographic Center), Patrick Gibson (marine science undergraduate student, University of Miami), Nohra Galvis (M.A., 1992, MBF; Corporación Propuesta Ambiental, Bogotá, Colombia), P. W. G. (MBF faculty), Susan Colley (MBF Research Associate), Juan L. Maté (Ph.D., 2001, MBF, present address: Smithsonian Tropical Research Institute, Panamá), Jorge Cortés (Ph.D., 1990, MBF; Centro de Investigación en Ciencias del Mar y Limnología, University of Costa Rica), David O. Obura (Ph.D., 1995, MBF; Coral Reef Degradation in the Indian Ocean, Mombasa, Kenya), Héctor Reyes Bonilla (MBF Ph.D. student, Universidad Autónoma de Baja California Sur, La Paz, México). Bottom row: C. Mark Eakin (Ph.D., 1991, MBF; NOAA/National Geophysical Data Center, Boulder, Colorado), Christiane Hueerkamp (MBF intern, 1999-2000; Center for Tropical Marine Ecology, Bremen, Germany), Peggy Fong (RSMAS Fellow, 1992-94; University of California, Los Angeles), Andrew Baker (Ph.D., 1999, MBF; Wildlife Conservation Society, New York), Héctor Guzmán (MBF associate, 1984-present; Smithsonian Tropical Research Institute, Panamá), and Fernando Rivera (MBF associate, 1984-present, Ph.D. student, University of Melbourne, Australia).

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Ian G. Macintyre getting ready for the "JAWS" exhibit, 1985. (Photo R. Clark)

GEOBIOLOGICAL CORAL-REEF STUDIES

BY

IAN G. MACINTYRE

The amazing world of natural history made its first impact on me during World War II on the tropical island of Barbados. My Scottish father had become manager of the British Union Oil Company there, a small outfit that explored for oil and gas on the island and refined crude oil from Venezuela. I had arrived in Barbados in April of 1939, and with the outbreak of war and appearance of German U-boats in the region, my family became trapped on the island. As a result, I spent seven of my formative years in a house in the tropics that had a golf course at the back and the emerald Caribbean Sea a short distance in front. This is where it all began.

The golf course became my hunting ground for butterflies and bird and reptile eggs. What a challenge it was to try to net those elusive butterflies or reach a nest way out on a thin limb. Molluscs were also highly sought after. Sunrise Tellins (*Tellina radiata*), which we called “auroras” because of their radiating colors, were a special prize. To capture them, I had to pump holes in the shallow back-reef sand with my feet until I could feel one of these slippery bivalves and dive it up. When slit open and spread out to dry, they resembled butterflies.

About the time I started school, I met the “ultimate collector,” Dr Alfred Senn, the British Union Oil Company’s geologist. I used to spend hours after school watching him picking microfossils — mostly foraminifera, I think — out of samples and filing them in endless rows of small vials. This was the life for me: I decided then and there to become an oil company geologist.

When the war ended, my father, convinced that an 11-year old who was spending all his spare time in trees or in the sea was simply not taking life seriously enough, decided to ship me off to boarding school in Scotland. There I was expected to develop character and apply myself to more serious studies. So in 1946, still set on becoming an oil company geologist, I headed for the land of chilblains.

After completing my secondary education, I turned my gaze to the New World because of the expanding opportunities for work in oil companies there. Though I was accepted at MIT, my father’s Scottish roots ran too deep and I entered Queen’s University in Canada. Upon graduation in 1957 with a BSc. in geological engineering, I was hired as a stratigrapher in the Exploration Department of Shell Oil Company of Canada in Regina, Saskatchewan (a.k.a. “Saskaberia”). Needless to say, it warmed my heart when I heard that I had been selected to participate in a short course on Recent

carbonate sediments in Florida and the Bahamas, under the leadership of Robert N. Ginsburg of Shell Development Company. This course and accompanying field trips (Fig. 1a,b) introduced me to a vast new world of marine science. This is really where I belonged, I thought, but it would take some time to get there.



Figure 1. 1959. Shell Development Company Field Trip on Recent Carbonate Sediments: (a) Bob Ginsburg (with hat) encouraging a larger to wade across a mud bank in Florida Bay. (b) Gene Shinn, Bob's new field assistant, encouraging participants to sample raw conch.

After working for three years on the Mississippian of Saskatchewan, I was transferred to Calgary, Alberta, to work on the Devonian (Fig. 2a). I was eventually assigned to work with Leslie V. Illing, noted at that time for his classic study of Bahamian sediments. I worked with Les on the Devonian of the foothills and Rocky Mountains of Alberta (Fig. 2b). It was during a field trip to show Bob Ginsburg some of our work that I told him that I was interested in modern carbonate studies. He said: "If you are going to make a move, do it now."

This meant abandoning my lucrative but sometimes tedious life as an oil geologist for the meager existence of a graduate student at McGill University. McGill's Bellairs Research Institute in Barbados brought me right back to my old home. It was there, with its director, John B. Lewis, that I collected data for my dissertation on the sediments and reefs off the west coast of the island. The most striking sea-floor topography off this coast consisted of two ridges cresting at 20 m and 70 m and running parallel to the entire west coast. This was my first encounter with what I interpreted to be relict submerged coral reefs that probably were flourishing shallow-water reefs during preexisting lower postglacial sea levels (Macintyre, 1967).



Figure 2. 1962. Working for Shell Oil Company of Canada, in Calgary, Alberta, Canada. (a) In my Exploration Department Office. (b) Mapping with Les Illing (no hat) in the Rocky Mountains.

A short time later, as a postdoctoral fellow (Fig. 3a,b) at the Duke University Marine Laboratory, I collected further data during several Caribbean cruises on the R/V *Eastward*. I was then able to demonstrate that these relict reef ridges were common features on many of the insular slopes and shelves of islands in the eastern Caribbean and even off the east coast of Florida (Macintyre, 1972).

There were skeptics aplenty in those days (the late 1960s and early 1970s). The prevailing thought at that time was that the development of Caribbean coral reefs had been restricted by cool postglacial temperatures, and hence that these reefs were less than 5,000 years old, being thin veneers with inherited relief from the substrates on which they were established. So my relict reefs could not be postglacial. It became clear that we needed to see the fossil record preserved within these deeper ridges and the shallow platform reefs if we hoped to arrive at a better understanding of the Holocene history of western Atlantic coral reefs.

It was during my stay at the Duke University Marine Laboratory that, along with Orrin H. Pilkey (Fig. 4a), we discovered numerous patches of tropical reef corals offshore in Onslow Bay, North Carolina (Macintyre and Pilkey, 1969). These patches consisted of *Solenastrea hyades* and *Siderastrea siderea* (Fig. 4b), usually established on flat Miocene sandstone outcrops in water depths of 20 to 40 meters. The intriguing question was: how had these corals survived in water temperatures that dropped to less than 16 °C for three months of the year and also withstood periods of temporary burial by migrating sand waves.

In 1970, I joined the staff of Smithsonian Institution's National Museum of Natural History. My first responsibility was to manage an NSF pre-proposal grant to develop a long-term, multidisciplinary, and multi-institutional proposal to study coral-reef ecosystems. This project — Comparative Investigations of Tropical Reef Ecosystems

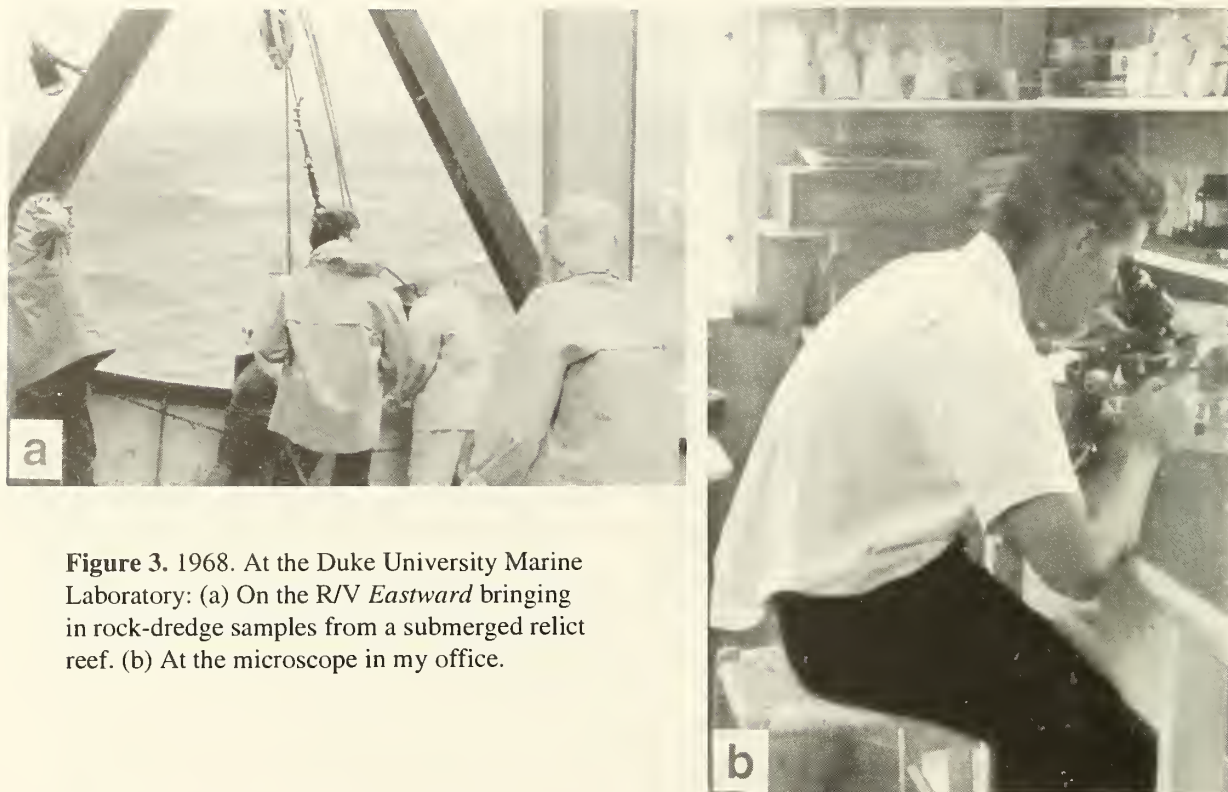


Figure 3. 1968. At the Duke University Marine Laboratory: (a) On the R/V *Eastward* bringing in rock-dredge samples from a submerged relict reef. (b) At the microscope in my office.



Figure 4. 1968. Tropical reef corals off North Carolina. (a) With Orrin Pilkey inspecting rock-dredge samples. (b) The two corals *Solenastrea hyades* (top) and *Siderastrea siderea* (bottom), both scales 5 centimeters.

(CITRE) — gave me a tremendous opportunity to meet and interact with some of the world's most respected coral-reef scientists. The initial phase was to survey potential reef sites where this work would be undertaken. One of these reef-site visits turned out to be somewhat hazardous. When Walter H. Adey and I set off to check Acklins Island in the southern Bahamas in October 1970, we had to charter a small plane to get there. The airport runway on Acklins was in poor shape, so the pilot decided to land on a recently bulldozed road. As he made his approach, he clipped a large boulder, lost a wheel, and crash-landed. Luckily, there were no injuries to the passengers, but the plane was a wreck. This dramatic arrival may have been a bad omen. We found that the reefs of this area were unsuitable for a research site.

It was during these early stages of developing the CITRE proposal that I also met the renowned David R. Stoddart of Cambridge University. I was in charge of the Geology Working Group, scheduled to meet at the University of Hawaii to discuss our research objectives, and en route was supposed to join Stoddart on the flight west from San Francisco. All I could think of was Cambridge's entrance requirement of Latin and my own lack of talent therein, hoping the conversation wouldn't turn to that subject. Much to my delight, David talked incessantly and enthusiastically about coral reefs and their associated islands — by the time we arrived in Hawaii, we were friends for life!

Then in November of 1971 a two-week workshop took place on Glover's Reef, Belize, where 41 participants (Fig. 5) representing a variety of reef-related disciplines worked on developing a computerized coral-reef ecosystem model that integrated all the various aspects of coral reef research. The model that resulted from this workshop was designed to depict the flow of carbon through a coral-reef ecosystem. Although the proposal was never funded, it proved to be the catalyst for many future studies on coral reefs (*Atoll Research Bulletin* Nos. 172-73 describing this model have been out of print for many years). Another important contribution resulting from this grant was the discovery of Carrie Bow Cay on the Belizean Barrier Reef, where the National Museum of Natural History established a field station in 1972. This is still a very active field facility, which has provided field support for over 600 publications on coral reef and mangrove investigations.

Part way through the development of this NSF grant, I was officially transferred to Smithsonian's Department of Paleobiology. With the setup funds provided, I was finally given the opportunity to achieve my goal, which was to look inside coral reef frameworks. To this end, I developed a submersible drill by adapting a hydraulic impact wrench to accommodate standard drill equipment (Macintyre, 1974). This drill, which could be operated by three people, would allow us to collect cores across an entire reef transect from the exposed reef flat (Fig. 6a) to the deep fore reef (Fig. 6b). My initial studies of the geological record preserved within coral-reef frameworks were undertaken with Peter W. Glynn off the Caribbean and Pacific coasts of Panama (Macintyre and Glynn, 1976; Glynn and Macintyre, 1977). Here we found out that modern Caribbean fringing coral reefs were not thin veneers but were up to 14 m thick and were quite capable of forming their own relief. Our success was quickly appreciated and colleagues in the United States, Australia, Japan, and Germany began assembling their own hydraulic drilling equipment to study Holocene coral-reef history.



Figure 5. 1971. Group photograph of participants of the CITRE Workshop on Glover's Reef, Belize. 1. Frank H. Talbot 2. Ray S. Birdsong 3. Marie-Hélène Sachet 4. Bernard C. Patten 5. Edith H. Chave 6. Peter W. Glynn 7. Arthur Barnett 8. David W. Greenfield 9. Clyde Moore 10. Joseph C. Zieman 11. Robin Ross 13. Tom S. English 14. Amada Reimer 15. Andrew C. Vastano 16. James E. Böhlke 17. David R. Stoddart 18. F. Raymond Fosberg 20. Sir Maurice Yonge 21. Shauna Woods 22. James W. Porter 23. Klaus Ruetzler 24. C. Lavett Smith 25. Stephen V. Smith 26. Ralph W. Schreiber 27. Nelson Marshall 28. Thelma Richardson 29. Donald W. Kinsey 30. Arthur L. Dahl 31. Amfried Antonius 32. Michael E.Q. Pilson 34. Judy Lang 36. Ian G. Macintyre 38. Charles E. Birkeland 39. Jon N. Weber



Figure 6. Working with hydraulic-operated coring equipment: (a) Above water drilling the Holandés Ridge, Panamá. 1993. (b) At a depth of 15 meters, coring a relict reef off the southeast coast of St. Croix. 1976.

In 1976, I was able to look at the internal structures of some relict submerged coral reefs. First, with Walter H. Adey (Adey et al., 1977) we drilled a shelf-edge ridge system off the southeastern coast of St. Croix (Fig. 6b); then, with Robin G. Lighty we collected samples from exposure through a shelf-edge ridge that had been dredged by a pipeline company off the east coast of Florida (Lighty et al., 1978). In both cases, we discovered relict shallow-water *Acropora palmata* reefs that were stranded 7,000 to 8,000 years ago by stress conditions related to the flooding of the inner shelves. By the time conditions improved, the water at the shelf edges was too deep for the shallow-water communities to reestablish. Therefore these shelf-edge ridges were indeed relict Holocene reefs that had flourished some 9,500 to 7,000 years ago.

My work on Holocene reef history continued with Robin G. Lighty on Abaco Bank, Northern Bahamas (Lighty et al., 1980); with Randolph B. Burke in the Gulf of Mexico (Macintyre et al., 1981); Randy Burke and Eugene A. Shinn in Belize (Macintyre et al., 1982; Shinn et al., 1982); with Bill Raymond off Puerto Rico (Macintyre et al., 1983); with H. Gray Multer and others off Antigua (Macintyre et al., 1985; with Randy Burke and Walter Adey in Tague Bay, St. Croix (Burke et al., 1989); in the eastern Pacific with Peter Glynn and Jorge Cortés off mainland Costa Rica, Caño Island, Cocos Island and the Galápagos Islands (Macintyre et al., 1993); with R. Pamela Reid and

Robert S. Steneck in the Exumas, Bahamas (Macintyre et al., 1996); and finally with Peter Glynn and Bob Steneck (Macintyre et al., in press) in the San Blas Islands, Panamá (Fig. 6a).

Information on the history of the relict reefs on the deeper outer slopes of Caribbean islands was obtained by Richard G. Fairbanks. He used offshore drilling techniques to recover cores from depths of over 100m off the south coast of Barbados (Fairbanks, 1988). These cores revealed coral reefs trying to cope with the rising seas caused by the melting of Pleistocene ice sheets. The earliest record of flourishing shallow-water *Acropora palmata* reefs dates to about 17,000 to 12,000 years ago; these reefs were stranded by IA melt-water pulse that resulted in a very rapid rise in sea level. A shallow-water *Acropora palmata* section established on a Pleistocene surface at a depth of about 80 m, which dates from 12,000 to 10,000 years ago, correlates with the relict reef ridge that I studied off the west coast of Barbados many years earlier (Macintyre et al., 1991). Fairbanks' data suggest that this 10-m ridge is a reef that flourished during the relatively stable seas of the younger Dryas chonozone, but that it too was unable to survive a second meltwater pulse (IB) that started about 10,000 years ago. In this major contribution to our understanding of the post-Pleistocene history of Western Atlantic coral reefs, Fairbanks has indicated that more deep-sea drilling is essential not only to document the early history of Holocene reefs but also to record the history of the waters in which these reefs grew. Biologists also need to investigate in more detail the deeper water communities that still flourish on some of these relict reefs and their relationship to shallow shelf reefs — particularly their role in recruitment.

As a carbonate petrologist, however, I have had many other interests beyond the history of coral reefs. One particular challenge has been to document submarine lithification in coral reefs and try to pin down the processes responsible for the precipitation of this Mg calcite and aragonite (Macintyre, 1977; 1984; Macintyre and Marshall, 1988). Many have worked on this problem without arriving at a final answer. At this point, I favor the hypothesis that submarine lithification is related to the release of ammonia by-products associated with the decay of organic material trapped within the reef framework.

In other studies, Richard R. Graus and I have employed computer model simulation techniques to investigate the factors controlling the growth form of colonial corals (Graus and Macintyre, 1976; 1982) and the distributional patterns of coral-reef internal facies (Graus and Macintyre, 1984; 1988; 1998; Graus et al., 1985). Our coral studies demonstrated that the variations in colony shape of *Montastraea annularis* with depth appears to correlate with the changes in the light field associated with increasing depth. The reef model, which constructed zonation changes, was based primarily on bottom wave velocity and depth (i.e., light), which allowed us to predict storm damage with data on the bottom topography and storm characteristics. From information on the water characteristics of an area, we were also able to replicate the pattern of the internal facies of a reef that had been drilled as well as the zonation patterns of various Caribbean coral reefs.

Some of my other studies have focused on the skeletal mineralogy of some marine organisms. In an investigation of the mineralogy associated with stylasters, Stephen D. Cairns and I found that the carbonate mineralogy there is controlled for the most part by phylogenetic rather than environmental factors (Cairns and Macintyre,

1992). Likewise, Frederick M. Bayer and I found that the skeletal minerals in a great variety of octocorals are taxonomically rather than environmentally controlled (Bayer and Macintyre, 2001). Of particular interest was the discovery of carbonate hydroxylapatite found in only one family—Gorgoniidae. We interpreted this unusual occurrence of apatite in modern invertebrates to represent a vestige of an earlier history of phosphatic skeletal mineralization in coelenterates (Macintyre et al., 2000).

R. Pamela Reid and I have undertaken extensive studies of the recrystallization of carbonate grains in shallow tropical seas. To our surprise, we found that recrystallization of porous skeletal grains such as *Halimeda* plates (Macintyre and Reid, 1995) or porcelaneous foraminifera tests (Macintyre and Reid, 1998) occurs when the organism is still alive. In other words, the original skeletal needles or rods break down to form a microcrystalline texture before the grains become part of the sediment floor. This alteration was always thought to be a post-mortem phenomenon. Indeed, further alteration does occur after death (Reid and Macintyre, 1998), much of it related to a micritization process produced by an endolithic cyanobacterium *Solentia* that fills its microborings with fibrous aragonite concurrently as it penetrates the grains (Reid and Macintyre, 2000).

In addition, I have worked with a multidisciplinary group on the processes involved in the accretion and lamination of modern marine stromatolites in the Exuma area of the Bahamas. These phenomena, it turns out, are related to a complex interaction of cyanobacterial communities that trap sediment grains and then alter them by introducing chemical changes within these mats of trapped sediment (Reid et al., 2000). My particular interest was the role of the endolithic cyanobacterium *Solentia* that I mentioned earlier. When there is a hiatus in the accumulation of these stromatolites, *Solentia* extensively bores and infills grains commonly passing from one grain to another welding them together. This process forms the dominant laminated horizons in the Exuma stromatolites (Macintyre et al., 2000). In other words, a microborer, commonly thought of as an agent of destruction, has now been shown to be a major factor in the preservation of well-lithified stromatolites.

Recently I have been working with Richard B. Aronson, William F. Precht, and others in a study of the open-framework reefs of the southern lagoon of the Belizean barrier reef (Aronson et al., 1998). With many simple push cores (Fig. 7 a,b), we have shown that recent major changes in the coral communities of this area, related to both white-band disease and bleaching, have not occurred on a regional scale for the last 3,000 years (Aronson et al., in press) and could have some relationship to human activity (Aronson et al., 2000). Similar studies have been extended to the lagoon reefs inside Bocas del Toro, Panamá.

Perhaps one of the greatest rewards of my career has been the opportunity to have participated in a period of major new discoveries in coral-reef research. Even more exciting has been the chance to collaborate with an outstanding group of scientists from across many disciplines. All of these research colleagues can be credited, in part, for the Darwin Medal (Fig. 8) that I received in 1996 from the International Society for Coral Reef Studies.

A full record of my work can be found on the web at:

<http://nmnh.si.edu/paleo/macintyre>

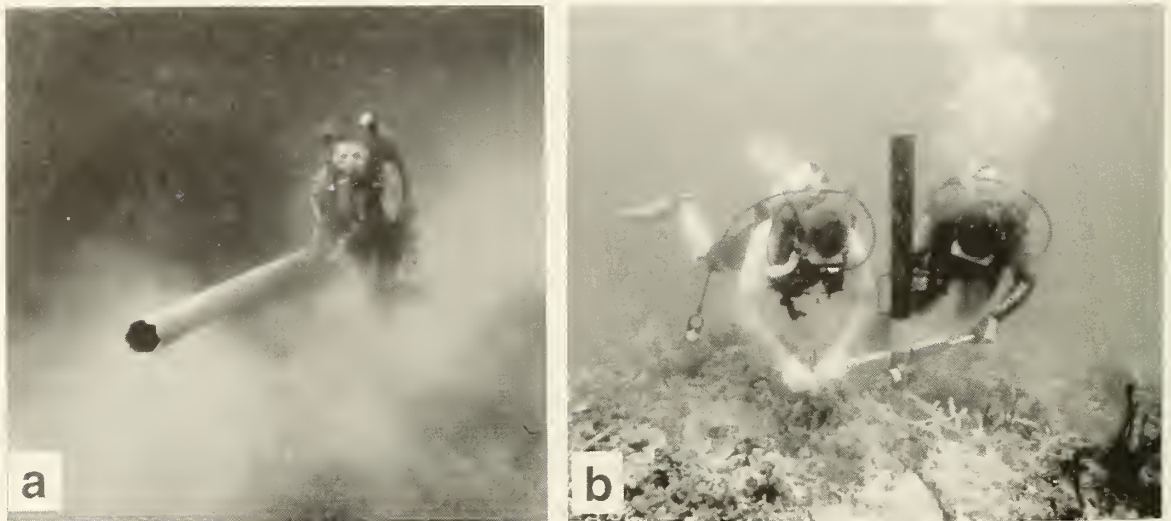


Figure 7. 1997. Push-core studies in Belize: (a) Taking a 5-meter core tube to a core site. (b) Coring with Bill Precht on the left. Note the dark sliding hammer weight on top of the core tube. (Photos by R. B. Aronson)



Figure 8. 1996. With the Darwin Medal awarded to me that year by the International Society for Reef Studies. (Photo by W. T. Boykins)

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Improvisation is the mother of discovery. Klaus Rützler on Carrie Bow Cay in 1974, long before day-time electricity through solar power had become a standard feature at this coral-reef field station. (Ernst Kirsteuer looking on, outhouse and reef crest in the background.)

EXPLORING NEPTUNE'S GARDENS: FROM LANDLUBBER TO REEF BIOLOGIST

BY

KLAUS RÜTZLER

INTRODUCTION

This special issue of ARB provides an excellent opportunity to reminisce, without doubt a satisfying experience, even though it is safe to assume that few except the volume editor will read these accounts. But it may be useful for our successors to have a reference that may explain old mistakes, or at least point to the motivation and thinking that guided us, thus paving the way for interpretation, correction, and improvement.

I am not really sure what to call myself professionally, what my real scientific persona may be, though many would say I am a sponge biologist, with a strong inclination toward ecology. Yet my interests range from rocky (Mediterranean) shores to reefs and mangroves, and I have strayed into the systematics, morphology, and biology of microbes, algae, and entoprocts, mostly sponge symbionts, and some “pseudosponges” that turned out to be agglutinating foraminiferans. I have also been an expedition leader and research coordinator and facilitator, probably because diving became a research discipline for me during the early pioneering years when scuba shops were not just around the corner and every piece of equipment had to be invented or copied from someone else's.

My father, Karl Rützler, was an accountant, my mother, Maria Hermine, a “homemaker.” Both loved nature and climbed (particularly my father), hiked, and skied in the Alps of our native Tyrol and spent summer vacations touring Austria and the surrounding countries and swimming (particularly my mother) in lakes and off the rocky coasts of Italy, Slovenia, and Croatia. They spent the rest of their leisure time at the theater or the opera, or listening to classical music. I was dragged along but disappointed my parents by not wanting to become a classical pianist and refusing to learn to swim until I turned seven.

My childhood years were strongly influenced by *Das gläserne Unterseeboot* (The Glass Submarine); I can't remember the author and don't know what happened to my copy of the book, but its story remains vivid. It was about a U-Boot designer in the 1930s who built a special model with large windows cut into the hull. He took his family, wife, and two children on a journey around the world, discovering foreign lands and studying sea life along the way, from the frigid rocky slopes of the island of

Helgoland (North Sea) to the splendid coral reefs of Pacific atolls. The book had many pictures, and there is no doubt that it was instrumental in fixing my gaze on the sea. These early tendencies were further reinforced by Jules Verne's *Twenty Thousand Leagues Under the Sea*. Since my parent's resources did not allow for a submarine of glass or any other material, I had to direct my interests to other means of ocean exploration. By the time I was 17, I had read all the books by the pioneers of "skin diving" and underwater photography, such as Hans Hass (e.g., Hass, 1947), Jacques-Yves Cousteau (e.g., Cousteau and Dumas, 1953), and Dimitri Rébicoff (e.g., Rébicoff, 1956). After reading several versions of Paul de Kruif's *Microbe Hunters* (e.g., de Kruif, 1926) I became fascinated by all kinds of microscopic creatures at the bottom of the evolutionary tree, and I shall be forever grateful to my parents for giving me a good-quality microscope for my sixteenth birthday despite its considerable cost (Figure 1).



Figure 1. At age 16 in Vienna, with new Reichert RC microscope examining life forms in pond water.

Since my ventures into the field of scientific diving for the purpose of sponge research are described elsewhere (Rützler, 1996), I shall limit the following paragraphs to a few highlights of my career that I did not mention there.

MATERIALS AND METHODS

This topic can be summarized as follows: Arrived by the traditional means of human reproduction, with the early years and primary education in Vienna (compromised by Adolf Hitler's maniacal expansionism and retaliation by Allied Forces). Life then consisted of voracious reading of biology-related literature, tending to aquaria filled with local pond life, and discoveries under the microscope donated by understanding parents. Secondary and university education also took place in Vienna.

Then came post-World War II travel through Austria and Italy by hitch-hiking and motor scooter to dive in lakes and finally in the sea. Scrap metal, rubber, plexiglass, and cookware were rededicated to the manufacture of personal dive gear. Student course work and a Ph.D. dissertation were completed in the Croatian Adriatic.

RESULTS

This is a chronological report on key events as I remember them without thorough research of dates and details. Though I can mention only a few individuals

who helped and influenced me during my professional development, I have not forgotten any of the others.

The Early Years

The Diving Adventure. My first diving gear, in the early 1950s, was built by myself from scrap, with the aid of photographs and figures in the aforementioned books. Nothing along these lines was commercially available in post–World War II Austria, not even a mask and fins. Those were the days of breath-held or aqualung diving, and using a snorkel would certainly have been considered unprofessional. Luckily, I eventually found other sports-diving enthusiasts who had access to “industrial” facilities such as rubber presses and aluminum-melting furnaces so was able to custom-build the necessary gear without undue cost. Eventually, my original jerry-rigged oxygen rebreather was replaced by a much healthier compressed-air regulator (“aqualung”). The pressure cooker that served as my first underwater housing for a spring-wound Robot 24 x 24 mm camera (activated by a stuffing-box-sealed trigger rod) was traded in for a compact cast-aluminum case with multiple-function controls.

Despite rapid technological advances in compressed-air diving, this pleasurable and safe (if done in moderation) technique remained out of the reach of my meager resources. It was also difficult to get access to compressors. Most underwater activities through my late twenties consisted of free diving to a depth of 6 to 8 m, although my colleagues and I were able to reach below 20 m after weeks of getting in shape.

My earliest diving excursions took place in the frigid lakes of Austria, where crayfish, pike, and catfish provided the excitement. There I saw my first live sponges, one of the few freshwater species. These sponges look more like a patch of moss than an animal and were not fully appreciated by me.

My first dives in the sea were in the Adriatic near Umag (now in Slovenia) and Sistiana (near Trieste, Italy), along stretches of rocky shore covered by seaweed and in waters teeming with fish and colorful invertebrates. My companions at the time included a group of dare-devil physicians who had formed a dive club in Carinthia (southern Austria) and my good friend from high school days, Hans Pulpan (Puli), now a geophysicist in Fairbanks, Alaska. These trips were a great adventure for us all, though my friends cared less about the biology of the exciting habitats we visited than the diving. The following summers, Puli and I (and one or another game friend from high school) visited the Mediterranean islands of Elba and Corsica. We hitchhiked from Vienna to Livorno, in northwestern Italy, where ferries depart for both islands. We explored their beautiful steep rocky shores to our heart’s content and had no trouble at either location finding a fisherman with a few fields and a vineyard by the coast who would let us pitch a tent and use his freshwater well, and who would sell us grapes, figs, bread, tomatoes, some fish, and a little red wine. During these trips we contributed very little to science but a whole lot to our personal development, which included honing our skills at catching fish, lobster, and squid, or finding mussels and oysters, and cooking them on small camp fires. We soon became adept at peeling the fruit of *Opuntia* (prickly pears) without getting thousands of hairlike spines into our fingers and learned that

hooks and hand lines were more desirable fishing tools than our crude homemade speargun (then still legal), which was difficult to aim precisely and made big holes in the poor victims. But the best part of it all was observing and photographing sea life in situ.

Going Professional. My career in marine biology began in the fall of 1955, when I entered the University of Vienna and met Rupert Riedl, the teacher who influenced my professional life the most (and that of a good number of fellow students, many still among my best friends). At the time, Rupert was “assistant” to the university’s one and only full Professor of Zoology (a comparative morphologist with no interest in the sea). Under the university’s antiquated system, Rupert had no students of his own but could act as a dissertation adviser. Although I was far from needing one of those, he recognized my enthusiasm and got me on the right track from the outset. I was put into a laboratory together with two other students who were assisting him in his research (between attending lectures and labs) and were about to start their doctoral dissertations. The senior one was Ernst Kirsteuer, who became a fine histologist and world expert in nemertean biology (ending up as curator and chairman of invertebrates at New York’s American Museum of Natural History). The other student was Hellmuth Forstner, a rather independent fellow with a great knack for setting up aquarium systems, understanding decapod crustaceans, picking up pretty women, and mastering biophysics; he retired last year from his position as physiologist at the University of Innsbruck. Together we had a fantastically good time and developed strong bonds with each other and the sea we loved, the Mediterranean.

Rupert’s research in those days revolved around the systematics and ecology of hydroids and turbellarian worms. He and colleagues under his leadership had returned from a series of diving and collecting expeditions to the Gulf of Sorrento, south of Naples, and were about to complete a series of monographs dealing with the diversity and quantitative distribution of animals and plants inside and outside intertidal caves. Lucky for me, his “sponge man,” Kurt Russ, a trained agricultural entomologist who wanted to experience the sea, was tied up with his insects. Russ had neither the time nor the inclination to describe the sponges he had collected and sorted during his trip or to deal with the statistics explaining their distribution. So the job of writing up the results and publishing them as coauthor fell to me. Needless to say, I felt honored to be accepted into the team and to be given a chance to write something that would actually be published (Russ and Rützler, 1959).

Two years later, I was ready to start a dissertation. With Rupert’s encouragement, I continued working with sponges and the ecology of marine caves where they abound, and where the lack of light keeps algae, their principal and fast-growing space competitors, at bay. Colorful cave sponges had already caught my eye before then because they made excellent subjects for underwater photography (we used disposable-bulb flashguns in those days), but no one seemed to be around who could identify them. Fortunately, before my mistakes in self-taught systematics had become too serious, Rupert connected me with an Italian colleague, sponge biologist Michele Sarà, then professor in Naples. Michele imbued me with confidence, for he treated me like a

colleague and old friend and seemed pleased with my work. We collaborated in the description of a new sponge, my second publication.

The First Austrian Indo-West-Pacific Expedition. This memorable adventure with a pretentious title was planned with my friend Ernst Kirsteuer. Our goal was to dive on a coral reef and experience its splendor before completing our dissertations and their defense and having to face the real world of employment (considered equal to enslavement). Initially, we hoped to persuade a larger group of colleagues to participate and to obtain the support of a well-heeled sponsor, who would readily see, as we did,



Figure 2. In May 1959, preparing the field laboratory on a beach at Tany kely, a small island off Nosy Bé (the land mass of Madagascar is in the background) (top). At the 16 mm Eumig movie camera filming marine life in a variety of photo cuvettes (bottom).

that our proposed endeavor had great import for humankind. Our destination was to be the Seychelles, where we would undertake the First Austrian Seychelles Expedition. Early on, we learned some valuable lessons about organizing people and fund-raising, above all, that both are exceedingly difficult to do, especially by greenhorns like us. After our proposed collaborators had dropped out one by one and we were unable to arrange affordable passage to the Seychelles, we switched course. We found that a cabin was available on a French freighter (for the price of deck passage) going from Marseille to Nosy Bé, Madagascar. There we enjoyed the hospitality of the director of a French oceanographic station, who provided inexpensive field facilities: a government-owned storage shed on a tiny offshore reef island, Tany kely (“small island”) (Figures 2, 3).



Figure 3. Barracuda for dinner at Tany kely; spearfishing, accepted and legal as a sport in those days (1959/60), provided food and was an excellent training device for breath-held diving (left). Exploring a coastal swamp on Nosy Bé, where rain forest meets mangrove (right).

Supported by fish that we caught, local rice and coconut, and some 10 crates of food and other donations-in-kind from sponsors back home, we spent ten months on the island (during 1959/60), living in a small tent and working in a field lab installed on the porch of the thatched-roof shed, with only one three-week break when we visited the Island of Réunion. Our expedition crates served as lab benches. We had brought along borrowed or donated dive equipment, microscopes, movie and still cameras, photo cuvettes, fixatives, darkroom supplies, and so on. The Nosy Bé lab loaned us one dive tank with regulator (the underwater cameraman of the day got to use it) and a small aluminum dinghy to get around near shore. From this base, we conducted quite a

thorough study of sponges and nemertean of the small fringing reef that was only 20 meters away from the sweaty air mattresses that served as our beds. We photographed animals and plants under water and close up and produced a one-hour documentary film (16 mm format) that was later shown on Austrian television and of course, along with many of our transparencies, in numerous lectures to the public. Among other discoveries, we observed periodic gathering of the voracious crown-of-thorns starfish (*Acanthaster*), many years before this phenomenon was reported in the literature and led to worldwide concern for the survival of the planet's reefs. After almost a year, we returned to our alma mater, maybe not a lot smarter than when we had left but certainly richer in experience and with a working knowledge of Sakalawa French. It was now time to finish my dissertation (Rützler, 1965) and get serious about a professional life.

Coming to America

At the MCZ. After graduating in 1963, I was offered a life-changing opportunity for postdoctoral work at Harvard's Museum of Comparative Zoology. Renowned evolutionary biologist and then MCZ Director Ernst Mayr needed someone to reorganize and reevaluate certain marine invertebrate collections, and I just happened to be available and (thanks to Ruper Riedl, who also recommended me) had the taxonomic knowledge to do the job. Ernst encouraged me to continue my research on sponges and arranged for a grant that allowed me a one-month stay at the old Lerner Marine Laboratory (of the American Museum of Natural History), on Bimini, Bahamas. My travels during that time took me to various places, including Washington, D.C., where my Harvard buddy Ken Boss (he graduated while I was there and accepted a job at the Smithsonian-based National Marine Fisheries Service) introduced me to Don Squires, chairman of the newly created Department of Invertebrate Zoology. Don wanted a sponge expert but no one was available in the United States. I was hoping to hook up with a dynamic and expanding center of systematic and evolutionary biology, which was not far from what Don had in mind. He told me he was about to establish a powerhouse of marine research where fieldwork, which was so important to me, would be at center stage for anyone who agreed to join the team. Though I had to leave the country for two years in order to get an immigration visa, he said the paperwork would be done in time (and he was quite right in his predictions). While still at Harvard, I was "found" by some of the organizers of the International Indian Ocean Expedition (IIOE) at nearby Woods Hole Oceanographic Institution and offered travel expenses and research space on the participating Stanford-owned three-mast schooner *Te Vega* later that year. This promised to be an outstanding experience and despite my misgivings about boats (which invariably made me seasick, even while still in the harbor), I signed up.

The Te Vega Cruise. When I finally got to meet her, in Singapore Harbor, it was love at first sight. The *Te Vega*, built in Germany in 1928, with a steel hull and mahogany decks and fittings, was, for a quintessential landlubber like me, a dream of a sailing ship. Although the *Te Vega* was refitted as a research vessel, with an ugly yellow-painted lab structure on its lovely wooden deck, it was sleek and elegant and

became “home” for the next three months. I was never seasick on the *Te Vega*. It was very stable in the water, counterbalanced by a heavy, deep-draft keel that gave the ship a gentle pitch, with hardly any rolling at all (to be sure, I started out with a substantial dose of Dramamine).

This cruise under the auspices of the IIOE was designed primarily for the investigation of shallow (scuba-depth) water, particularly the reefs there. Hence the *Te Vega* was outfitted with whalers and outboard motors, scuba, and, to my great delight, a complete, brand new Rolleiflex underwater camera system with flash (this outfit, called Rolleimarine, was designed by Austrian underwater pioneer Hans Hass in cooperation

with the camera company). I was assigned the camera and tested it for close-up and correct flash exposure, using Ektachrome processing in the ship’s own darkroom.

There were 15 crew members, all skilled volunteers and many expert divers. Five, including myself, were scientists, under the leadership of Rolf Bolin, a Stanford ichthyologist (and personal friend of both Ed Ricketts and John Steinbeck, hence a source of fascinating tales). Seven were graduate students, who served as our assistants and thus gained hands-on training in our special fields (Figure 4). My other colleagues among the senior scientists were Alan Kohn, a *Conus* (poisonous marine snail) specialist from the University of Washington; Joe Rosewater, malacologist at the



Figure 4. As postdoctoral Fellow in 1963 (third from left, with graduate students), on board the research vessel *Te Vega* (Stanford University) off Sumatra during the International Indian Ocean Expedition.

Smithsonian (who later became a close friend); and Llewella Colinvau, my principal dive buddy and specialist on the ubiquitous calcified reef algal genus *Halimeda*.

Starting from Singapore, our cruise headed north along the west coast of Malaysia and Thailand, crossed the northern tip of Sumatra, turned south, stopping at many remote and seldom-visited (by scientists) islands of the Nias and Mentawai group, all the way to Mega. Just before we turned northwest toward Colombo, *Te Vega*’s mainshaft broke and the cruise, lacking wind for sailing at this point, ended somewhat prematurely in the harbor of Padang, Sumatra.

At the Smithsonian

The Immigrant. After returning to Austria from the Indian Ocean, I knew that there would be a gap when I had to fend for myself. I teamed up with my friend Hellmuth Forstner (who had just finished a postdoc at the marine laboratory in

Plymouth) and wrote a proposal to an Austrian science fund (the President T. Körner Award), which was accepted and provided enough support for a project at the University of Vienna and the Marine Biological Institute at Rovinj (now in Croatia). The study explored the idea that individual marine benthic organisms are subjected to microclimatic conditions that are different from and shorter lived than parameters measured by the precise instruments (such as mercury thermometers, devices for sampling and analyzing water, mechanical current meters, radiometers, and Secchi discs) ordinarily used to monitor large-scale and slow oceanographic processes. Using the newest (then) semiconductor technology (and Hellmuth's electronic skills), we designed, machined, and assembled miniature instrumentation that would record the microclimate surrounding, say, a sponge. We field-tested the new equipment with results good enough to publish, although the principal paper (Forstner and Rützler, 1970) did not appear in print until much later.

With my immigration visa in hand, I reported for duty at the National Museum of Natural History in October of 1965. I was warmly welcomed by my new colleagues and started to look for opportunities to conduct field research in the Caribbean. Almost instantly, I was invited to be the first marine biologist to participate in the ongoing Biological Survey of Dominica (West Indies). Several Museum colleagues and outside collaborators were already studying the terrestrial fauna and flora of this beautiful volcanic island. I was given funds to purchase a dive compressor with tanks and other field equipment and to bring along a collaborator as dive partner. Since there were no divers in my department, I asked Ernst Kirsteuer who was then at the American Museum of Natural History in New York. We established two field sites at opposite coasts of Dominica, windward and leeward, and collected the same kind of samples as at Tany kely, Madagascar: sponges for me, dead coral rock for nermertean extraction for him.

The fieldwork in Dominica was followed by excursions to Bermuda, Puerto Rico and the Virgin Islands, Santa Marta (Colombia), Bimini (Bahamas), and Jamaica, where I examined sponges potentially useful for pharmacological purposes. I also returned to the Mediterranean for similar work and a special project on commercial sponges in Tunisia funded by Public Law 480 funds, which derived from money owed to the United States by countries with nonconvertible currencies and made available for research applications. Tunisia already had a Smithsonian- operated Mediterranean Marine Sorting Center (MMSC) in place for pre-identifying and distributing to specialists organisms and sediment samples collected by various expeditions. It was to serve as a staging area for my project. Since the host country had the right to determine which kind of research could be conducted, I negotiated a permit for a commercial-sponge project, for the simple reason that Tunisia was looking for more applied research, with an immediate benefit, than we usually do and had a large but declining bath sponge industry. While examining several species of the commercial genera *Spongia* (bath sponge) and *Hippospongia* (horse sponge) and related noncommercial *Ircinia* and *Cacospongia* (Rützler, 1976), I was surprised to discover some useful biological facts relating to commercial quality and cultivation potential, such as the incorporation of an iron oxide in the primary elastic fibers. However, this work probably did little to improve the living standard of the local fishermen and merchants, who, I'm

certain, continue to sell nicely wrapped but substandard and overprocessed souvenir sponges to the tourists. Coincidentally, I was able to continue more academic work on my main interest at the time, the rock-excavating clionid sponges, though it was conducted with meager scientific equipment and supplies and without qualified assistance.

In the late 1960s, my long dormant attraction to Bermuda was revived with the arrival of Wolfgang Sterrer, a friend from university days who was now director of the Bermuda Biological Station (BBS). Back in Vienna, Wolfgang and I used to spend long evening hours pondering the meaning of life and where our careers might lead us. This friendship was enhanced when he came for a postdoctoral appointment with our mentor Rupert Riedl, then Professor of Zoology at the University of North Carolina at Chapel Hill, to study Gnathostomulida, an obscure (he would call it “enigmatic”) worm group living in the grayish and bad-smelling deeper layer of poorly aerated marine sand, also known as sulfide ecosystem. We bonded further during a field trip to the Smithsonian Tropical Research Institute (STRI) in Panamá, where we were delegated to examine the effects of a major oil spill on the reef communities of Galeta Island near the Atlantic entrance to the Canal. We found that the reefs were doing well during this crisis as long as the oil floated on the water surface, beyond the reach of corals, but that the adjacent mangrove was seriously endangered because oil coating the stiltroots was clogging the air vents (lenticells) that supply oxygen to the mud-buried underground roots. This discovery, published as a trip report in *Bioscience* (Rützler and Sterrer, 1970), was later acknowledged as the first report on the biological effects of an oil spill in mangroves and led to a whole new chapter in my life. Soon after Wolfgang took the helm at BBS, he invited me to conduct some research there. I obtained a grant to support a study of bioerosion by sponges and set up lab facilities at the biostation, equipped with a photomicroscope and an inflatable boat with outboard motor that could take me over most of the Bermuda reef platform. In parallel with the work in Tunisia, I explored the systematics, fine-structure morphology, ecology, and rock-excavation mechanisms of the Clionidae and other calcium-carbonate-burrowing sponges (Rützler and Rieger, 1973; Rützler, 1974; 1975).

Toujours Smithsonian–Marine Shallow-Water Ecosystems. Though highly satisfying, my fieldwork at Bermuda was also “lonely.” The ecologist in me had long wanted to hook up with other disciplines that would allow me to better understand the role of sponges within the community. It was clear that I needed to work with specialists in taxonomic groups other than Porifera and fields related to benthos ecology, such as physico-chemical micro-climatology, nutrient cycling and food chains, microbiology, reef geology, ecophysiology, competition, and symbiosis. Surprisingly, considering that I worked in a natural history museum and not a marine science institute, I found that colleagues in other departments were of a similar mind, and we started discussing the possibility of collaborative coral reef studies, recognizing that a group effort would be more scientifically productive and correlated and would attract more support if it were a highly visible team project. One day in the late 1960s, we decided to launch a multidisciplinary, long-term program named Investigations of Marine Shallow-Water Ecosystems (IMSWE), a rather generous title designed so it would not lock us into

studying just one ecosystem in a single biogeographic region, although our primary aim at the time was to investigate coral reefs. Our core group consisted of Ian Macintyre, a carbonate sedimentologist interested in the historical development of reefs; Arthur Dahl, a marine botanist working on fleshy algae ecology; Walter Adey, a paleoecologist and expert in crustose coralline algae and their role on reefs and rocky shores; and myself; as well as several associates, such as paleobiologists Porter Kier and Tom Waller, and



Figure 5. With Arnfried Antonius (left) in 1970, loading a charter boat to explore reef sites in the Bahamas for the new IMSWE Program

invertebrate biologist Mary Rice. Soon after our initial meetings, we were joined by my postdoctoral collaborator Arnfried Antonius, an old friend from the University of Vienna and a specialist in microscopic marine worms who wanted to switch fields and study stony corals. Walter Adey led our first fund-raising effort, which brought a grant that allowed us to purchase some diving equipment and an inflatable boat with outboard motor to scout out Caribbean locations for the ideal site to start our program. We also connected with a new program at the National Science Foundation (which does not usually fund Smithsonian programs) called International Decade of Ocean Exploration, IDOE) and proposed to collaborate with a dozen other

institutional applicants on a comprehensive, 10-year coral reef study. NSF awarded a project development grant, and Ian Macintyre was appointed lead investigator for the Smithsonian group. During phase one we conducted surveys of suitable study locations (Figure 5). We were looking for a site with high geological and biological diversity, low environmental impact by humans, and easy access. Credit goes to Arnfried Antonius, an active member of the survey team, for recommending Glover's Reef in Belize (then still British Honduras), a pristine atoll outside the largest barrier reef in the Western Hemisphere and an ideal geological formation for comparative research in the Indo-Pacific at a later date. The entire NSF-sponsored group was very enthusiastic about the location, and after an on-site workshop and development of a



Figure 6. Carrie Bow Cay on the barrier reef of Belize, aerial view (1975) looking north.

conceptual model of a reef ecosystem, a grand proposal was submitted for 10- year funding. Within a few weeks, we learned that our monumental effort had failed, that the proposal was rejected, and that we were back to where we had started.

It fell to Antonius and me to retrieve some equipment stored at Glover’s Reef. We chartered a boat in Belize City for the trip. When we returned from the atoll, the boat crew aimed too far south and missed the Tobacco Cay cut across the barrier reef. Instead, we entered the great lagoon through a passage just south of a small, unfamiliar coral island (Figure 6). Stopping to examine the location, we found an unoccupied building bearing a sign that said “Welcome to Carrie Bow Cay.”

IMSWE–Belize. Despite the initial setback in obtaining funding for our field research, we were convinced that Belize offered the best conditions to carry out our program. Glover’s Reef and the two other off- shore atolls were too remote to provide affordable facilities, but the barrier reef and the huge lagoon offered unlimited access to all kinds of reefs, to large seagrass meadows, and to spectacular mangrove islands. The country was virtually undiscovered by tourists or developers, and Belize City was just two hours away by plane from Miami. Antonius had already determined that Pelican Beach Motel in Dangriga (Stan Creek District) on the mainland of Belize would keep our equipment in storage until we found a convenient research base. When we inquired about Carrie Bow Cay, we learned that it was the property and vacation place of Henry and Carrie Bowman, parents of the motel’s owner, Henry Bowman Junior. It turned out that “Sir Henry” did not object to leasing part of Carrie Bow for our research program. In February 1972 we signed the deal that marked the founding of the Carrie Bow Marine Field Station (Figure 7). At this point I had also decided to stay in the United States and become a U.S. citizen, a double celebration.



Figure 7. Carrie Bow Cay looking southwest across the reef flat (1975); the low building to the left was the original laboratory-kitchen unit that also included one bedroom (left). Sign identifying the new use of the island as research base (right).

We initiated our program in Belize by mapping habitats, determining reef zonation, and conducting biological inventory (Figure 8). Ian Macintyre and colleagues probed the reef’s past development. I collected sponges as part of the survey and studied



Figure 8. Preparing helium balloon (with Hans Pulpan, right) for reef mapping with remote control camera (1976) (top). Reflecting on biodiversity (1982; bottom).

their distributional ecology (Rützler, 1978), but I had also taken on the responsibility of coordinating the research and, with Arnfried Antonius as first station manager, the logistical maintenance of the field operation, including scientific facilities and diving and boating equipment. We prepared detailed reports on our results, maintained a rapidly growing program publication list, and were able to obtain a multitude of small in-house grants for travel that also helped maintain our simple research station. To our disappointment, administrators did not agree that large-scale support of a coral reef program and a field station should be one of the Institution's priorities. Upon our constant urging, however, an inquiry from the EXXON Corporation about a modest but multiyear donation to enhance the company's image in Central and South America was channeled our way, and we succeeded in attracting this source of funding.

When our results became more substantial, we decided to prepare a contributed volume to more convincingly demonstrate our abilities as a research team concentrating on one particular reef complex over the long term (Rützler and Macintyre, 1982). During his study of diversity and morphological variability of reef corals, Arnfried Antonius encountered many competitive interactions, including disease-like phenomena and started the new discipline of coral pathology. I continued some of this work on black-band disease to expand my experience in cyanobacterial systematics and biology from parallel studies of sponge symbionts (Rützler et al., 1983; Rützler, 1990).

Launching SWAMP. Every autumn, I traveled to the (then) EXXON headquarters in Manhattan to present our IMSWE coral-reef program's progress report and renewal proposal and pressed for an increase in support, with reasonable success. In the early 1980s, I was warned that the company might soon look for another project to fund. As it happened, my own interests at the time had focused on the fascinating sponge fauna of the Twin Cays mangrove, a lagoon island just 10 minutes by boat west of Carrie Bow Cay. Not only was the sponge fauna on subtidal mangrove roots and peat banks very rich, but other invertebrates, algae, seagrasses, fishes, and birds abounded in the area's deep channels, tidal creeks, interior lakes, ponds, and mudflats. The occasional crocodile, boa, and manatee lent an aura of mystery to the place. Because the body of water surrounding the island was often exchanged by tidal flushing, this was an ideal spot for underwater observation and photography by snorkeling.

On behalf of my IMSWE colleagues from the departments of Invertebrate Zoology, Paleobiology, Botany, and Vertebrate Zoology, I prepared a proposal for a study of the Twin Cays Mangrove ecosystem, dubbed the Smithsonian West Atlantic Mangrove Program, or SWAMP, certainly an appropriate acronym. EXXON agreed to renew its funding of our group, and we received a substantial Smithsonian Scholarly Studies Award. Ian Macintyre immediately agreed to core the peat base of the island to determine whether it was built on an old patch reef or some other base. Part of my overall plan was to analyze both the aquatic and terrestrial communities (Figure 9), and thus to invite yet another Museum department, entomology, to join the effort. This work would be reported in a semipopular monograph illustrated by artists working along with scientists in the field. This project (originally titled *Art in a SWAMP* and partly funded by the Smithsonian Women's Committee) continues to be successful, thanks in part to the contributions of talented scientific illustrators such as Ilka ("Candy") Feller, Molly Kelly Ryan, and Mary Parrish. With their help, we were able to show communities in three dimensions (including below the substrate) and over time, and were motivated to examine more carefully the properties of organisms, associations, textures, colors, and the patterns of distribution and behavior.

The collaboration with Candy Feller itself took on a new dimension when she decided to return to her early love, the biology of plants and insects, and to complete a dissertation on the Twin Cays mangrove. From then on, Candy collaborated with us as a postdoc (at the Smithsonian Environmental Research Center, or SERC) and mangrove expert (Rützler and Feller, 1996). Now a member of the SERC staff, she and nine colleagues from different disciplines and institutions recently landed a large, five-year NSF Biocomplexity grant to study nutrient production and cycling at Twin Cays.



Figure 9. Surveying the Twin Cays mangrove swamp in the Belize lagoon just west of Carrie Bow Cay (1983). Tidal canals are navigated by inflatable boat (left); small fishes in ponds and lakes are caught (when lucky) by throw net (top right); sponges on mangrove stilt roots and peat banks are studied by mask and snorkel (bottom right).

Caribbean Coral Reef Ecosystems. In 1986 our highly improvised financing (both EXXON and Scholarly Studies support had ceased by then) received a boost from an unexpected source. Congress, under the Caribbean Basin Initiative, authorized an increase in our Museum's budget for a program based on the IMSWE–SWAMP study. It was named the Caribbean Coral Reef Ecosystems Program (CCRE). I was appointed CCRE's scientific director by then National Museum of Natural History Director Richard Fiske, and Marsha Sitnik became the program administrator, to help me jump bureaucratic hurdles and streamline logistics. This infusion of new support allowed us not only to continue current projects but also to remodel the old, termite-riddled buildings on Carrie Bow Cay that served as our lab and living quarters. We were able to acquire badly needed instrumentation and better and safer diving and boating equipment, transfer a dedicated assistant, Mike Carpenter, as operations manager to the program, hire Kathleen Smith as my research assistant (Figure 10), and establish a modest but cost-effective grants and fellowships fund.

The Caribbean Coral Reef Ecosystems Program is dedicated to the study of the barrier-reef complex of Belize, including its coral reefs, mangroves, seagrass meadows, and blue-water ecosystems. Instead of directing projects toward "mission-oriented"



Figure 10. Renovating the Carrie Bow Marine Field Station with funds from the new CCRE Program (1987); Operations Manager Mike Carpenter is installing a large storage tank for the seawater flow-through system (top). Research assistant Kathleen Smith positioning sponges for filming water-pumping activity made visible by fluorescent dye (bottom).

research, which is expensive and academically restrictive, we encourage proposals from Smithsonian staff for travel grants, and from outside collaborators nominated by Smithsonian scientists and representing complementary disciplines or having special skills. Because our funds are limited and constantly at risk of being reduced in favor of the Institution's executive priorities, proposals are competitive and rated by a program steering committee, which judges relevance to program focus, appropriateness of methods, and previous record of research production and quality. Basing project awardees at the well-equipped Carrie Bow Marine Field Station not only ensures optimum use of travel funds but concentrates research efforts on a well-defined area and stimulates scientists to discuss their results, to interact, and to collaborate. Few

bureaucrats whose primary aim is to balance their books understand that scientific results are of a far higher quality when investigations take place close to the study environment and the investigators are free to formulate their experimental designs, change project objectives if the situation so demands, interact with like-minded colleagues in an informal setting, and have easy access to functional facilities. As every CCRE participant can attest, our program has unquestionably satisfied these requirements and has done so over a sustained period for a much smaller per capita expenditure than most marine laboratories can boast of doing.

Since its inception, CCRE has made many fine contributions to science, such as the discovery of pathways of nutrient requirements of mangroves; the study of symbioses ranging from microbial associations to endobiotic shrimps societies; the assessment of coral stress, competitive threats, disease, and recovery; and installation and fine-tuning of oceanographic monitoring equipment. We are particularly proud of our environmental conservation research, which entails analysis of a marine biodiversity hotspot, the Pelican Cays reefal mangrove community in southern Belize (Macintyre and Rützler, 2000).

In December 1997, disaster struck when an accidental fire broke out in our field station's library, following a suspected short in the electric generator circuit that was abetted by insulation-devouring cockroaches and wood-destroying termites and fanned by a strong Northeaster. The fire spread rapidly through both laboratory buildings, which included the kitchen and most of our living quarters. The tremendous heat vaporized our microscopes and other scientific equipment, exploded gas cylinders, burned all our books, tools, refrigerators, and photovoltaic system, even storage tanks filled with water. All but a small cabin on the far south of the island and the separately stored boat and dive equipment were lost. Luckily, no one was injured, for only the very brave volunteer station manager, Josh White, was on the site.

My colleagues and I had several debates about where to go from there, but I do not recall a single person in favor of terminating the field operation. Therese Bowman-Rath, our local agent, and Norma Bowman, current co-owners of Carrie Bow Cay, were supportive of our plans to rebuild the station and were willing to dedicate their insurance reimbursement to this end as long as we met special construction requirements to suit the workings of a field station. We designed a new building with wet and dry laboratories, aquarium room, workshop, library, and kitchen facilities and had it built by the talented young, Cuban-born architect-builder Hedel Góngora. A three-room cabin for scientist's accommodations was added a year later. Operations manager Mike Carpenter and his volunteer station managers took care of countless finishing jobs. Despite a setback caused by the brutal forces of Hurricane Mitch in 1998, we were able to rededicate the Carrie Bow Marine Field Station in August 1999 (Figures 11, 12) and resume our dynamic research in the spring of 2000. By the time this essay is published, we will have celebrated the thirtieth anniversary of our discovery of Carrie Bow Cay and founding of one of the most successful program-dedicated coral-reef laboratories in existence.



Figure 11. Concluding remarks at the rededication ceremony in the entrance of the new laboratory building of the Carrie Bow Marine Field Station (August 1999); looking on: Barbara Schneider (left), representing the Smithsonian Provost, and Paula DePriest (right), representing the Natural History Museum's Senate of Scientist (top). Carrie Bow Cay and the completed new field station (2001) looking southwest (bottom).

CONCLUSION

When my teacher Rupert Riedl was still a rising marine biologist at the University of Vienna and not yet “The Master” (as we students used to refer to him) that he would become, he once told me, over a jug of Dalmatian red wine, about a big conflict in his life. He could not decide whether to take the career path of a Charles Darwin, the evolutionary theoretician, or that of an Anton Dorn, the founder and director of the famous Zoological Station of Naples.

In the forty-some years that have elapsed since that discussion, Rupert has evolved from invertebrate systematist and morphologist to marine ecologist, evolutionary philosopher, and recognizance theoretician. One could say that he ended up

following the Darwin trail. I, on the other hand, have always liked to keep both feet anchored in reality, examining organisms and communities in situ and refining laboratory techniques, determining morphological structures and functional mechanisms,



Figure 12. It's over! With the Carrie Bow Marine Field Station rededicated, Klaus Ruetzler looking forward to another 30 years of research on the barrier-reef complex of Belize.

analyzing environmental needs and influences, and experimentally testing ideas on interrelations derived from observations in nature. I may also have found a niche as a guide and facilitator in a down-to-sea marine research program. Our research station may never become as famous as the Stazione Zoologica di Napoli, but it will be remembered as an efficient and highly productive operation located at the heart of a coral reef ecosystem rather than on the flank of some polluted city harbor, as many marine stations are.

My research may not have broken any scientific sound barriers, but it has been, and continues to be, an honest attempt at understanding and recording conditions and processes in nature and compiling useful information about sponge biology. I hope that my practical approach has enabled me to make reasonable decisions about scientific priorities, to act as a catalyst for fruitful disciplinary collaboration, and to provide efficient facilities for all those who study biology more in praxis than in theory.

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My career owes much to many friends, teachers, and sponsors. Indeed, the list is too long to itemize here. The people mentioned above are but a small sample, and I apologize to those I have left out. I am particularly grateful to my long-time research assistant Kathleen Smith and my close current collaborators Mike Carpenter, Michelle Nestlerode, and Marsha Sitnik. Pictures shown here were contributed by Mike Carpenter (Fig. 11 bottom); Chip Clark (Figs. 8 bottom; 9 left; 9 top right); Paula DePriest (Fig. 12); Candy Feller (Fig. 9 bottom right); Ernst Kirsteuer (Figs. 2, 3, 6, 8 top); Ian Macintyre (Fig. 11 top); Mary Rice (Fig. 5); Karl Rützler (Fig. 1); and Kjell Sandved (Frontispiece, Fig. 7 right). Vicky Macintyre is acknowledged for her editorial work. Last but not least, I thank Molly Ryan for her friendship and for processing the illustrations in this article.

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Frontispiece. Mark M. Littler and Diane S. Littler holding a rock sample with the deepest-known plant life -- a coralline alga collected from 268 m in the Bahamas (p. 306, Guinness Book of World Records, 1998).

MARINE BOTANICAL STUDIES

BY

MARK M. LITTLER AND DIANE S. LITTLER

Our good friend, Ian Macintyre, coerced (bullied?) us into doing this article on why we do the work we do—so here goes! Mark's interest in nature began as a child in the Appalachian surroundings of southeastern Ohio, where his father was a country-club golf professional. Mark spent many an hour angling for fish using his own home-made tackle (Fig. 1)—an endeavor that was to establish a sense of value for all living things aquatic and that became a life-long passion. A pivotal event occurred when Mark's father moved the family to Key Biscayne, Florida, following the blizzard of 1950. They spent the winter months there throughout his young adult life. Mark immediately became enthralled with all of the fishing, snorkeling, spearing, shelling, boating, and water-skiing opportunities to be found in Florida. He made his first scuba dive in 1955 in the upper Florida Keys at Fowey Rocks.

In 1961, Mark obtained a bachelor's degree in botany/zoology from Ohio University (OU) and then began working as a chemist for the Ohio State Highway Department Testing Laboratory in Columbus. With the money he saved, he planned to do graduate work in marine fisheries biology. However, a fortuitous encounter in 1963 with Art Blickle, a former botany professor, led him to apply for a teaching assistantship at OU and study limnology/phyecology, the closest thing to marine biology at that institution. Mark's master's studies at OU focused on the seasonal cycles of phytoplankton in a state wildlife management lake, forever sending him on a botanical trajectory. The training provided not only a classical phyecological background but also an opportunity for Mark and Diane to meet. She was taking a phyecology class and he was the teaching assistant. Diane had grown up roaming the forests (and dominating golf events) near Salem, Ohio, with a vision of pursuing a career in forest ecology. Mark eventually convinced her that seaweeds also formed interesting forests, albeit underwater. After their marriage, and being accepted into several graduate programs, Mark and Diane chose to study at the University of Hawaii (UH) because of its comprehensive curriculum in marine algal ecology.

While working for the legendary oceanographer/ecologist/phyecologist Max Doty at UH, Mark and Diane learned broadly based coral-reef ecology and state-of-the-art approaches to research, most importantly, from a "big science" perspective. One of Max Doty's quotable quotes (re. alpha taxonomy) that stuck was, "If you don't do the taxonomy, you don't know what you are talking about, but if all you do is taxonomy, you don't have anything to say". Early on, Max assigned the crustose coralline algae, at that time an extremely intractable calcareous group, as Mark's dissertation topic. This led to much struggling and little progress until Izzie Abbott, a renowned and delightful systematist, saved the situation by patiently demonstrating a useful *in situ* sectioning technique for coralline algae. This "foot-in-the-door" permitted the taxonomic pigeonholing of key coralline reef-building species and, as detailed below, ultimately



Figure 1. Mark at age 10 displaying a nice stringer of fish. The inset (right) shows that time has not changed this behavior. Immediately after Mark lands an exceptional fish, one invariably hears "Diane get the camera, get the camera!!! Diane, Diane, where the h... is Diane???"

led to rekindled interest in the importance of these calcareous plants to coral-reef biology. Because UH had a very active visiting scientist program, the Littlers also came to know most of the pioneers in coral-reef research and many other "big-name" scientists in the field of marine phycology.

Surprisingly, Mark was invited to interview for assistant professorships by faculty at the University of Washington, California State University, and the University of California—all several years premature relative to the completion of his dissertation. Furthermore, Mark was

pressed by Peter Dixon (one of Europe's "phycological giants"), who was Chairman of the Department of Ecology, into accepting a tenure track position at the University of California, Irvine (UCI), a full year prior to completion of his Ph.D. How times have changed! Mark finished his doctorate requirements at the end of the first year in residence at UCI and received tenure three years later, ultimately achieving the rank of full professor in 1980. While at UCI, Mark and Diane directed very large research programs (up to 55 full-time employees), focusing (with Roger Seapy and Steve Murray) on the ecology and physiology of rocky intertidal ecosystems (Fig. 2). They also supervised three Ph.D. candidates, Keith Arnold, Frederic Briand, and Mark Hay, who now hold full-professor positions at major universities (one other, Phil Taylor, is a division director at the National Science Foundation) and produced seven master's students who all advanced to Ph.D. programs. In 1982, Mark was recruited for the Chairmanship in Botany at the Smithsonian Institution, and Diane finally completed her masters and doctorate degrees with Pacific Western University on the functional morphology of marine algae. Mark and Diane have been at the Smithsonian Institution since that time, with Diane also holding a senior-scientist position at Harbor Branch Oceanographic Institution.



Figure 2. Diane and Mark collecting marine plants on a rocky intertidal shore.

The third member of our team, Barrett Brooks, and ourselves (Fig. 3), have been among the handful of Smithsonian scientific divers certified to a depth of 60 m. We were most fortunate in being able to add Barrett (B²) to our team shortly after arriving at the Smithsonian Institution. However, the struggle with the SI Office of Personnel (OP) had been monumental! We had advertised for a person with both biological and scuba experience. What we received was a short list of five candidates that OP had selected which were none-of-the-above, with



Figure 3. Our research team (L>R), Diane, Barrett, and Mark, entering an underwater cave on the Great Astrolabe Reef, Fiji.

one having long-since moved (no forwarding address) and, incredibly, another that was actually deceased. Luckily, B² had begun to badger OP, having just returned from his Peace Corps assignment in the backcountry of Liberia, because he was in jeopardy of soon losing his governmental noncompetitive (i.e., preferential-hiring) status. OP begrudgingly sent him to us for an interview, even though he was an experienced scuba diver with excellent psychological training from the University of Colorado. Barrett was a bit (whole lot!) noncommittal during the interview process, although we anticipated that his Peace Corps background reflected a “can-do” willingness to undertake almost any onerous assignment, including enduring work under less than tolerable conditions. This assumption could not have been more “on-the-money” and his addition to the team has led to a most productive and entertaining partnership. Unlike B², neither of us are “party-animals,” so it has often fallen on B²’s capable and enthusiastic shoulders to serve as “designated reveler” for the team, under “other-duties-as-assigned” (a caveat included in all SI position descriptions).

We decided to focus on marine plants because they are among the most important and attractive inhabitants of coastal ecosystems throughout the world. Our interests and personal satisfaction derive from being able to add in some measure to the overall picture of the unique ocean planet upon which we live, instead of narrowly focusing on human activities and services, as most non-science professions do. By contrast, the biologist perceives the relatively recently arrived human species (with both admiration and trepidation!) as the most dominant intruder among the millions of other species that have occupied the planet since the origin of the blue-green algae (Cyanobacteria) several billion years ago. Such differences in perspective are not

trivial, particularly in the restrictive intellectual atmospheres that are increasingly being fostered by politicians and administrators from the “people professions” who impose values that inhibit fundamental principals of scientific inquiry.

Having said that, we now need to provide a little background information on the organisms and ecosystems that have held our interests for more than three decades. The 25,000 described species of marine algae comprise one-third of all phyla of the plant kingdom and have been evolving longer than any other group of organisms on earth. They are responsible for building many of the world’s reef systems, contribute substantially to the planet’s primary production, permanently scrub anthropogenic and natural carbon dioxide from the atmosphere as carbonate, and their extracts are used in more than one-third of all processed food items and pharmaceuticals. We are continually amazed by coral-reef biocomplexity, with the plants alone having evolved along five dramatically disparate evolutionary lines. The variety of their sizes, shapes, and colors is remarkable, as is the versatility of their intricate life histories and species richness. We have concentrated nearly 20 yrs of collecting and curating effort into documenting the enormous morphological plasticity, variability, and taxonomic diversity within all five of these difficult phylogenetic groups. We take considerable pride in the fact that no major tropical marine-algal field guide, monograph, or serious flora could be completed successfully without consulting examples from the tens of thousands of specimens our group has added to the U.S. National Herbarium.

The biochemical and physiological complexity of coral-reef plant life is also unequalled. Tropical seas, in general, host an immense biodiversity that is critical to the world’s biogeochemical cycles and serve as an important source of foods and pharmaceuticals. However, algal natural products and metabolic pathways have not yet been explored adequately and methods for sampling the more intractable and cryptic seaweeds have only recently been developed. Because of the rapid worldwide degradation of tropical reefs, we feel that it is imperative that they be studied from all aspects in a timely, efficient, and scientifically verifiable manner. It is of paramount importance to characterize changes in the physical, chemical, and living attributes of the coral-reef environment and to understand the response of the biota to such changes. As mentioned, marine plants from five diverse phylogenetic lines dominate and, in conjunction with coelenterate corals, are the major primary producers and builders of all reef systems. Considering the critical roles these organisms play in reef ecosystems, they must be taken into account in other fields of marine sciences, whether it be the study of fisheries resources, marine chemistry, ecology, geology, or any of the associated zoological disciplines. Much remains to be discovered about the plant species on tropical reefs and how they interact with each other and with the abiotic and biotic components of their environment. We strongly believe that a better understanding of the diversity of marine plant life is a key component to preventing further damage to this living resource.

Unfortunately, the overall biodiversity research in the coral-reef realm is relatively sparse compared to that of terrestrial systems, such as rain forests, particularly as it relates to the extraordinary diversity of higher levels of taxa (Sepkoski 1995). Ecological studies also have been greatly hindered due to inadequate taxonomy but are required to understand the fundamental consequences of both anthropogenic and natural changes to the biocomplexity of marine plant life. Vastly more of the world’s genetic diversity resides among the array of different phyla than in a profusion of closely related species. The diversity of the photosynthetic

pigment apparatus of the green algae (Chlorophyta) alone greatly exceeds that of all terrestrial plants combined (Andersen 1992). The polyphyletic nature of the algae means that they embrace a much broader range of diversity than many higher plant or animal groups. For example, relatively more classes, orders, families, and genera of algae are being discovered at a comparatively rapid rate; no less than 20 new classes have been erected in the past three decades. Continuing discoveries of new families, orders, and even phyla (e.g., Prochlorophyta) of marine plants foretell a wealth of biodiversity still unrealized. Also, exciting new information, novel techniques, and heightened awareness now permit dramatically improved sampling, species identifications, and process-oriented research at larger and larger geographic scales.

Our research within this fascinating domain has been both exciting and, at times, dangerous. When the International Finance Corporation of the World Bank asked us to conduct surveys of the Moroccan coastline to assess the feasibility of their funding a new algal harvesting industry, our team found itself many hundreds of kilometers south along the West African coastline in the occupied territory of Western Sahara. Unbeknownst to us at the time, this region was at war with Morocco. We were befriended by a nomadic group of Bedouin tribesmen who cordially invited us to partake in a delightful gunpowder tea ceremony in one of their tents (Fig. 4). Later it dawned on us that this was quite likely a unit of freedom fighters, since only young men were present and because a plane was shot down inland from the same area soon afterward.

We also encountered a flourishing cottage algal-harvesting operation in the region. Its harvests of the agarophyte *Gelidium sesquipedale* were bought by an elderly gentleman in Laayoune, Morocco, who originally had been bankrolled by a large Japanese agar-producing corporation. He was proud to show us his quarterly stockpile of harvested material (Fig. 5), which we estimated to be worth approximately \$2.5 million in profit. His large home appeared quite dilapidated from the exterior but was palatial inside, where we were graciously served by a staff of servants and a daughter, home from university in Spain; his son was attending university in Switzerland. Needless to say, we immediately questioned whether or not we were in the most rewarding end of the algae business!

Since then our group has pioneered the use of shipboard expeditions specifically for marine plant systematics, i.e., collections, *in situ* photography, and inventory surveys. One of our most productive projects came as the result of an NSF grant to Diane that made possible oceanographic expeditions to the far reaches of the tropical Western Atlantic with colleagues such as Brian Lapointe, Esperanca Gacia, and Dennis Hanisak. Our 2,000+ ship-based scuba dives have taken us to the coastal ecosystems and reefal habitats of Florida, the Bahamas, Greater Antilles, Lesser Antilles, Southern Caribbean, Western Caribbean, Gulf of Mexico, Mediterranean, Morocco, Seychelles Archipelago, and many island groups in the tropical Pacific. One of our more enjoyable oceanographic cruises was the first joint USSR-USA Expedition in Marine Biology, hosted by our friend and Chief Scientist Ed Titlyanov, aboard the *R/V Nesmeyanov*. Our group, and fellow Americans Andy Benson, Phil Dustan, Len Muscatine (Fig. 6), along with 62 Russian scientists, embarked on a halcyon cruise to study primary production phenomena in the Seychelles Archipelago (published in *Atoll Research Bulletin* Nos. 365-378, 1992a). It is amusing that Diane was virtually “put under a microscope” as the first example of a “typical” American woman to be seen by most of the Russians (see Fig. 7). Such ship-based expeditions (e.g., Figs. 6 and 8) have emphasized



Figure 4. Nomadic Bedouin tribesman serving ceremonial gunpowder tea in a remote location in Western Sahara. Inset at top shows tribesmen welcoming us to their encampment.



Figure 5. Arab gentleman and a small portion of his stockpiles of the commercially valuable red alga *Gelidium sesquipedale* in Laayoune, Morocco ready for shipment to Japan.



Figure 6. Mark and Barrett conducting a physiological/ecological experiment on fungiid corals with Len Muscatine aboard the *R/V Nesmeyanov* at Astove Atoll in the Seychelles Islands, Indian Ocean.

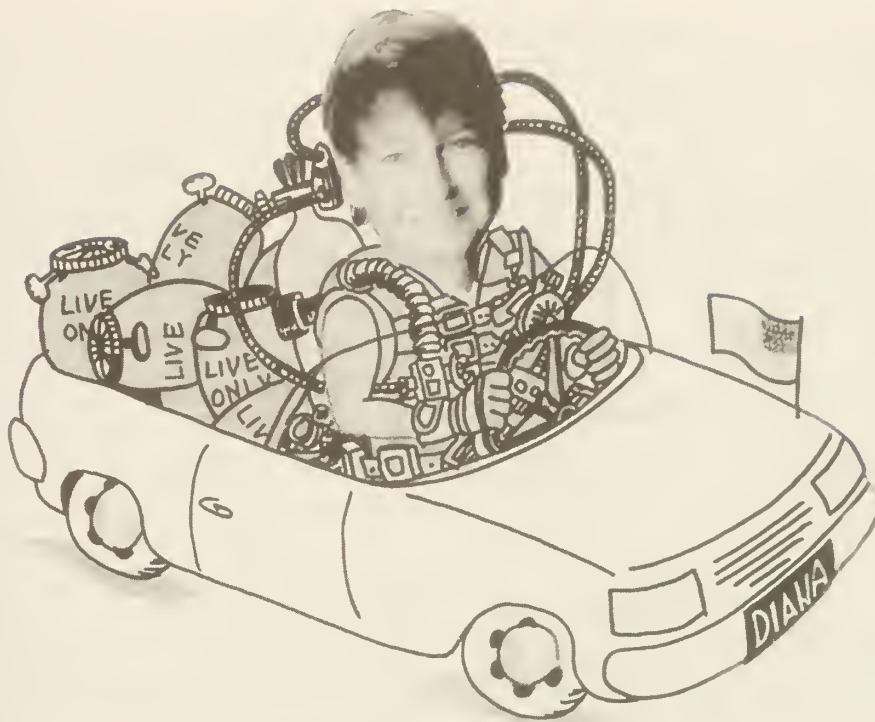


Figure 7. Russian illustrator's 1989 depiction of Diane, who has become renown in the Soviet Far East as "the woman who dives as deep as Lake Baikal".

pristine systems of extremely high biodiversity (e.g., Martinique), areas that had not been sampled well (e.g., Seychelles, southern Lesser Antilles), and environments with unique floras (e.g., Fiji, Panama). Oceanographic cruises have proven to be the most effective and efficient way to gain access to remote regions and also facilitate more thorough collecting and rapid processing than can be achieved by working only from land sites.

Lest all of this fieldwork appear too glamorous, it should be pointed out that between us we have endured a dislocated shoulder in Martinique, had an eye-ball badly lacerated in the Florida Keys, experienced severe ciguatera poisoning in Fiji, contracted malaria in the Solomon Islands, overcame amoebic dysentery in the Galapagos, and suffered through innumerable mandatory kava ceremonies in Fiji. For the uninitiated, kava is a traditional and locally revered drink (made from a plant root) that looks like muddy river water but does not taste nearly as good.

For the past 30 years, our group has addressed a broad spectrum of research topics ranging from systematics, functional morphology, and evolutionary studies of macroalgae to applied inventory and ecological work contracted by managers of tropical reef ecosystems. As an example, we were asked by the National Marine Sanctuaries Program of the National Oceanographic and Atmospheric Administration (NOAA) to initiate a rapid-response study (Littler et al. 1987) for assessing and monitoring the damage caused by the catastrophic grounding of the freighter *Wellwood* to the most popular recreational diving site in the world, Molasses Reef, Key Largo, Florida (Fig. 9). This work contributed to obtaining restitution in excess of \$3,000 per square meter of impacted reef substrate (\$22 million total). Additionally, baseline biodiversity inventories of Looe Key National Marine Sanctuary (Littler et al. 1986)

resulted in substantial penalties for the destruction of back-reef habitats following another ship grounding, a first for damage to seaweed-dominated communities. Furthermore, some of our biological inventory and survey programs (conducted for state and federal agencies such as the Bureau of Land Management, Office of Water Resources Research, State of Hawaii, NOAA, NSF, National Park Service, Minerals Management Service) culminated in the development of standard large-scale monitoring methods (Fig. 10, Littler and Littler 1985, 1987) as well as the conservation of major coastal ecosystems (Littler and Murray 1977, 1978, Littler 1979, Littler et al. 1986, 1987).

Our future research will continue to focus on complex interactions in marine ecosystems. These studies will take up both theoretical/experimental and systematic issues, often in collaboration with other researchers. As highlighted in the following selections, our studies have included the development of new theory with predictive capability as well as usable systematic field guides of applied value in shaping the management programs of environmental agencies as well as academic institutions.



Figure 8. The Littler's conducting PI vs. irradiance curve studies on Mediterranean deepwater algae aboard the *R/V Seward Johnson* (the submersible *Johnson-sea-Link II* is in the background).



Figure 9. Underway to conduct an assessment of the Freighter *Wellwood* grounding site with colleagues (L>R) Brian Lapointe, Jim Norris, and Katy Bucher (Littler et al., 1987).



Figure 10. Diane conducting photogrammetric/video assessments of benthic biota by open-sided helicopter (see Littler and Littler, 1987).

We regard the following as some of our most rewarding contributions (in chronological order):

(1) The first to quantitatively document the importance of coralline algae to the structure and functioning of tropical coral reefs by means of an experimental approach that combined distribution and abundance information with determinations of primary productivity and calcification rates. This resulted in one of the earliest reports of eutrophication as a cause of coral-to-algal phase shifts on tropical coral reefs, now a central theme of modern coral-reef research. Innovative photogrammetric methodologies also were developed and utilized for measuring standing stocks of benthic biota. (nineteen papers published; e.g., 1973. The population and community structure of Hawaii fringing-reef crustose Corallinaceae (Rhodophyta, Cryptonemiales). *J. Exp Mar. Biol. Ecol.* 11:103-120)

(2) Provided the first comprehensive overview of seasonal distribution and abundance patterns for invertebrate and seaweed populations for all major rocky intertidal ecosystems of the Southern California Outer Continental Shelf (OCS) and mainland from Point Conception to the Mexican border. Established the major biogeographical boundaries for the Southern California OCS and showed that these correlated with dominant oceanic currents and temperature isoclines. This program was part of a large national team effort (Bureau of Land Management) to ascertain the natural baseline levels of variability of U.S. continental shelf biotas prior to oil exploration and development. Sophisticated video and photogrammetric methods (e.g., Fig. 10) developed during this study are now in general use by marine biologists; whereas some are standardly required for marine biological inventory and monitoring studies by various U.S. federal agencies (e.g., Minerals Management Service, U.S. Park Service, NOAA). This research also was instrumental in impact prediction and helped exclude large tracts containing unique and/or sensitive marine ecosystems from lease sales. (eighteen papers published; e.g., 1991. The Southern California intertidal and littoral ecosystems. Pp. 273-296. *In* P. Nienhuis and A. C. Mathieson, eds. *Intertidal and littoral ecosystems of the world*. Vol. 24. Elsevier Sci. Pub., Amsterdam.)

(3) Major architects in the field of “marine algal functional morphology and evolution”. Our 1980 model incorporating convergent anatomical, physiological, and ecological features that transcend phylogenetic lines represented a dramatic improvement in realistically explaining the evolutionary interactions that have resulted in the broad array of morphological forms shown by marine plants (Fig. 11). Now that ecologists have sufficient understanding of functional-morphological groupings, they are able to interpret patterns as well as predict environmental or biotic relationships within complex communities without having to laboriously study each component species. (seventeen papers published; e.g., 1980. The evolution of thallus form and survival strategies in benthic marine macroalgae: field and laboratory tests of a functional form model. *Amer. Nat.* 116:25-44.)

(4) Discovered unknown deep-sea plant communities and their ecological roles (Frontispiece), which opened up a new area of biological oceanography. Stimulated research by a diverse following (e.g., malacologists, oceanographers, sediment geologists, biomass mariculturalists, reef ecologists, physiologists) concerning deepwater macroalgal roles in primary productivity, food webs, sedimentary processes, and reef building (Fig. 8). Authors of subsequent textbooks on oceanography and general botany have included illustrations and data from this finding. (Five papers published; e.g., 1985. Deepest known plant life is discovered on an uncharted seamount. *Science* 227:57-59.)

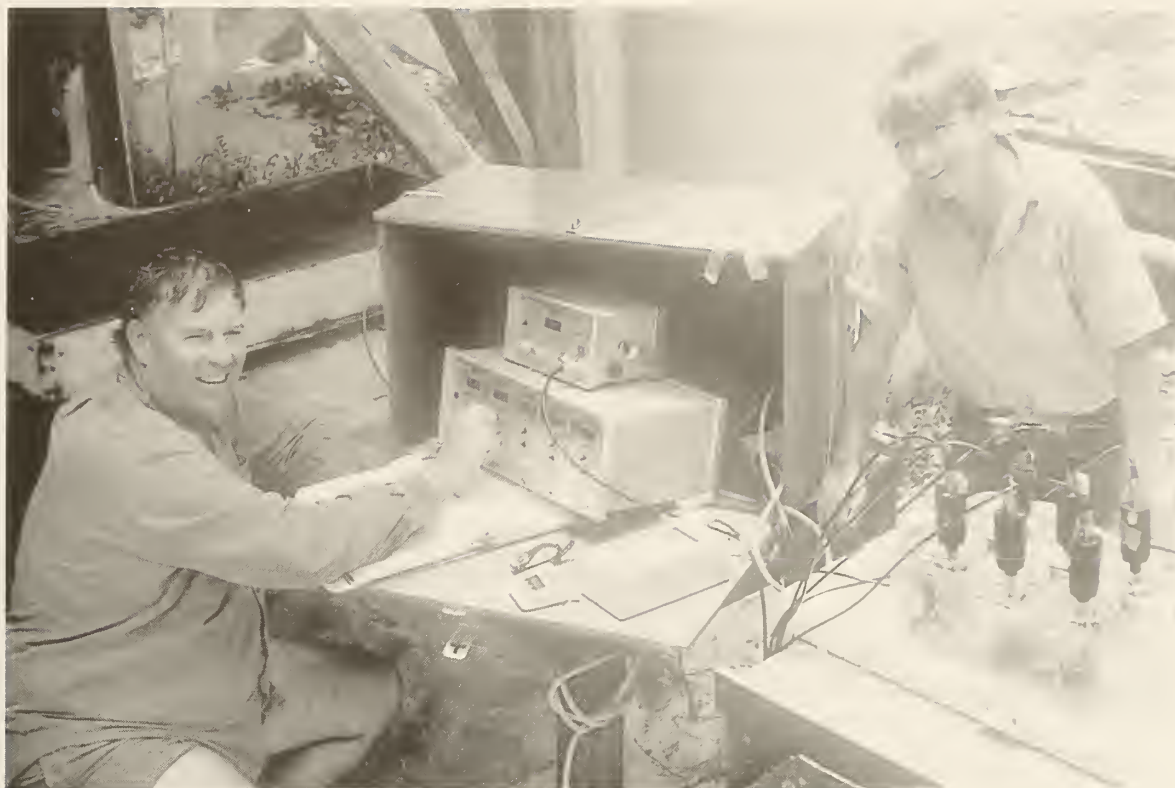


Figure 11. Performing functional morphology studies of tropical coral-reef algae in Belize.

(5) Developed and began testing a new theory of tropical reef biogenesis [Relative Dominance Model (RDM)] having major predictive significance to managers of reef ecosystems. This model posits that stable-state shifts in dominance from coral to various algal systems (accelerated by destructive forces) are ultimately a function of complex interactions between the major controlling factors of nutrient levels and herbivorous fish abundances, both of which are highly sensitive to anthropogenic modification. (Fifteen papers published; e.g., 1991. Comparisons of N- and P-limited productivity between high islands vs. low carbonate atolls in the Seychelles Archipelago: a test of the relative-dominance paradigm. *Coral Reefs* 10 (4):199-209.)

In conducting the above-mentioned basic and applied research, we were amazed to learn how few of the macroalgal species from the ocean realm, particularly the Caribbean region, have yet been discovered and properly documented. We suspect that there are more macrophyte species undescribed than described. As examples, our three generic monographs, incorporating extensive scuba-depth collections (Littler and Littler 1990, 1991, 1992b), more than doubled the number of known taxa for each genus (i.e., *Anadyomene*, *Avrainvillea*, and *Udotea*) in the Caribbean and tropical western Atlantic. Consequently, we came to the conclusion that a simple identification manual was urgently needed by a broad spectrum of marine investigators and conservationists.

Since producing the popular photo guide (with Jim Norris and Katy Bucher, Littler et al. 1989), we received numerous requests to develop a systematic treatment comparable to its user-friendly design but more comprehensive, with complete specimen documentation

(morphological and anatomical), thus enabling users to key and anatomically verify their identifications. Such requests came from various colleagues in adjacent areas of research (e.g., physiology, chemistry, ecology, geology/sedimentology) as well as from numerous systematists. The result, and currently our “present most significant work”, *Caribbean Reef Plants*, written in language comprehensible to any scientist or advanced amateur, provides a source to identify tropical western Atlantic marine plants without the liability of shipping materials to dwindling numbers of overcommitted specialists for determinations (see www.erols.com/offshoregraphics). This 15-year effort has already been a “best seller” and begun to facilitate advances in (1) the ability of coral-reef researchers to identify Caribbean marine plants and document their distributions, (2) knowledge of local and regional natural patterns of plant biocomplexity, and (3) understanding the processes that create and maintain important temporal and spatial patterns.

As we enter the twenty-first century, the diversity of marine plant life is being dramatically altered by the rapidly increasing and potentially irreversible effects of human activities. Although the potential for anthropogenic degradation of coral reefs was suggested nearly three decades ago (Littler 1973), their demise has continued to escalate. Repeatedly, the attention of both the scientific community and the general public has been directed to the accelerating decline of tropical reef systems (e.g., Lapointe 1989a, 1989b, Buddemeir 1992, Wilkinson 1992, and Ginsburg et al. 1993). In particular, many picturesque and diverse coral/algal-dominated reefs (e.g., Fig. 12) are rapidly undergoing phase shifts to monotonous algal monocultures, with the formerly complex communities becoming overgrown by several undesirable species. As



Figure 12. Diane diving on a garden-like coral patch reef in Fiji.

pointed out in the RDM, the most critical vectors are eutrophication (Littler et al. 1993, Lapointe et al. 1997) and destructive alterations to herbivory (Russ 1991, Hughes 1994). We believe that these two factors are primary in determining “stable states” on reefs and controlling phase shifts following major (catastrophic) disturbances such as physical habitat alteration/ destruction (Rogers 1985), diseases (Antonius 1995), invasions of exotic species, and sedimentation associated with land-based development (Kuhlmann 1988, Ogden 1988). Such stresses, along with global-warming phenomena, have the potential to cause serious and widespread social, economic, and biological problems.

Nevertheless, the situation may not be hopeless. Some governments have begun to protect significant tracts of reef systems (e.g., Australia, Philippines, Turks and Caicos, Netherlands Antilles, Belize, Mexico, and United States). We count ourselves among the increasing number of concerned coral-reef workers who are committed to providing conservation advocacy groups and governmental administrators with the tools and unbiased data needed to effect some of their management decisions. Coral reefs are remarkable ecosystems and extremely attractive to the recreational diving community (Fig. 12), but because they are under the surface of the sea, changes brought about by pollution and destructive fishing are not obvious to the casual observer. It is well known that tourism can bring a substantial economic return, and it is only by sound scientific inventories of the biodiversity of plant and other life on tropical reefs that a case can be built for establishing attractive and fully protected marine reserves. However, as human populational pressures escalate and continue to harm vulnerable coral-reef resources, it will become increasingly difficult to protect these resources unless sound scientific reasons for doing so are presented in a manner that will be convincing to conservation biologists, politicians, economists, sociologists, resource managers, governmental agencies, the general public, and, most importantly, the local populace.

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Michael Lang demonstrating a 1954 Air Lung double-hose regulator made by one of the first U.S. diving equipment manufacturers, Northhill Company, Inc. of Los Angeles, California.

SCIENTIFIC DIVING ON CORAL REEFS: A PERSONAL ACCOUNT

BY

MICHAEL A. LANG

My first contact with saltwater was the North Sea Channel. Growing up in Ghent, I spent many family weekends at Knokke on the Belgian coast, walking the flat sand beaches and crawling out onto algae-covered rock jetties. The cold, turbid brown water usually discouraged all but the hardiest souls from swimming, except for perhaps three weeks from the end of July to mid-August. Belgium is internationally renowned for its fabulous variety of beers and the finest chocolates, but not necessarily for its balmy summers. Our main activity was to catch grapsid crabs on a string with a piece of mussel attached by a clothespin. The next level of interest along the beach was to watch the crevettes fishermen ply their trade using huge Belgian draft horses to pull shrimp nets in the sandy shallows of the outgoing tides.

At that early age, I did not know that marine biology and scientific diving would be my calling, but when my family purchased a vacation home in Torrazza, near Imperia on the Italian Riviera in 1972, the blue Mediterranean's pull was intense. Alpine skiing was within an hour's drive during winter and spring breaks, and almost three months of summer vacation in this seaside town were stimulating to a teenager. Italy has its share of world-class free divers. Reading of Enzo Maiorca's lung capacity and breath-hold times and the Confédération Mondiale des Activités Subaquatiques (CMAS) diving championships with Jacques Mayol led to my first snorkeling excursions around Porto Maurizio in Imperia.

With the Musée Océanographique (Fig. 1) in Monaco just a 45-minute drive away, frequent trips to Monte Carlo and its aquarium provided countless viewing hours of marine creatures through glass panes. *Octopus vulgaris* on display was a magnet of fascination. Octopodes were interesting, but would be more so, I thought, if I could get in the water with them. To add to the allure of diving, the Oceanographic Institute's Director was Jacques-Yves Cousteau. Curiously, in Europe the "father of diving" was generally considered to be Austrian professor Hans Hass who, with his wife Lotte, had already made underwater black and white films of sperm whales in the Indian Ocean in the late 1930s. But Commandant Cousteau's business sense made him by far the most visible diver for years to come, especially in the United States through his underwater films and books. It is interesting to note that the history of diving in bells, free-diving or surface-supplied, precedes that of free-swimming scuba diving by over 2000 years (Table I).

Table I. Milestones in the History of Diving

30 B.C.	Alexander the Great's diving bell <i>Colimpha</i> .
+/- 100 A.D.	Active free-diving by Ama of Japan and Korea (Rahn, H. (ed.) 1965. <i>Physiology of breath-hold diving and the Ama of Japan</i> . NAS/NRC Publ. 1341. Washington, D.C., National Academy of Sciences).
1535	Guglielmo de Lorena (Italy): developed a type of diving bell.
1691	Sir Edmund Halley (England): designed a forerunner of the modern diving bell.
1715	John Lethbridge (England): creation of first One-Manned Atmospheric Diving System (OMADS).
1774	Freminet (France): surface-supplied diving to 15 m for 60 minutes of bottom time.
1799	Smeaton (England): dived with "diving chests" that used a forcing pump to replenish air supply.
1808	Friederich von Drieberg: <i>Triton</i> diving apparatus (bellows in a box).
1819	Augustus Siebe: invention of the diving dress.
1823	John and Charles Deane (England): Deane's Patent Diving Dress, a protective suit with a separate helmet and surface-supplied air.
1825	William James (England): first autonomous diving apparatus (compressed air carried in a circular iron reservoir around the diver's waist).
1832	Charles Condert (USA): horseshoe shaped waist-mounted air reservoir that provided a continuous flow of air to a flexible helmet.
1837	Augustus Siebe: full-body, airtight diving suit with attached helmet and free-flow valve.
1865	Rouquayrol and Denayrouze (France): metal back-mounted canister charged to 40 bars and an ambient-pressure demand regulator; semi-self-contained diving suit.
1879	Henry Fleuss (England): closed-circuit oxygen-rebreather SCUBA.
1913	Georghios (Greece): sponge diver free-dived to 61 m.
1918	Ogushi (Japan): Peerless Respirator connected to air cylinder charged to 150 bars.
1920s-1930s	Guy Gilpatrick (USA): used old flying goggles, plugged with putty and painted over. Karamarenko (Russia): first rubber mask with a single-pane window. De Courlieu (France): patented rubber foot fins. Steve Butler (England): first successful snorkel tube.
1926	Captain Yves Le Prieur and Msr. Fernex (France): continuous flow SCUBA system.
1935	Yves Le Prieur: lightweight compressed air apparatus with semi-automatic regulator and full-face mask.
1937	Georges Commeinhes (France): first fully automatic aqualung with full face mask.
1939	Dr. Christian Lambertsen (USA): Lambertsen Amphibious Respiratory Unit (LARU); oxygen rebreathing equipment for neutral buoyancy underwater swimming.
1943	Jacques-Yves Cousteau and Emile Gagnan (France): new fully automatic regulator with inlet and exhaust valve ("Aqua Lung"); first demand double-hose regulator.
1950	First single-hose regulators appear.
1959	Carlo Alinari (Italy): designs SOS decompression meter, a pneumatic device simulating nitrogen uptake through a ceramic membrane.
1962	Edwin Link (USA): first open-sea trial of saturation diving (8 hours at 60 feet in a one-man chamber).
1967	Edwin Link (USA): Perry-Link 4: first modern-day lockout submersible.
1969	Robert Croft (USA): breath-hold dive record to 75 m (1968 Cover of <i>Science</i> Vol. 162)
1975	Jacques Mayol (France): world breath-hold dive record to 99 m.
1970s	Personal flotation devices (buoyancy compensators) in the form of horse-collar bc's appear.
1979	Dr. Morgan Wells (USA): oxygen-enriched air (nitrox) tables published in NOAA Diving Manual.
1983	ORCA Industries mass produces electronic diver-carried decompression computers that allow for multilevel repetitive diving versus the square-wave U.S. Navy table tracking of nitrogen on- and offgasing.



Figure 1. Le Musée Océanographique in Monte Carlo, Principality of Monaco. (Photo M. Lang)

After I completed the classical humaniora curriculum in Belgium, it was time to leave the belt of storminess and its depressing gray climate behind. My father, a research chemist and professor, allowed my siblings and me to select the college of our choice, as long as we enrolled in the same one. My major interest now was oceanography and marine biology. In 1978, Humboldt State University in Arcata, California, became our home for the next two years. Not having much knowledge of the State of California, I was taken aback by the all-too-familiar northern European weather and rain. Having selected this university campus on the advice of our American friends, we wished in retrospect that they had been more specific when they recommended a California school: what they really meant was any campus located south of Santa Barbara! Many of my early scuba diving training courses through the university took place in harsh northern California ocean, river, and lake conditions. The entrance channel jetty to Humboldt Bay (Fig. 2) was the site of my first observation of *Octopus dofleini* in its natural habitat. Its size alone accelerated my air consumption and shortened my dives considerably.

Arriving in San Diego in 1980, I participated in a National Association of Underwater Instructors (NAUI) scuba instructor course at San Diego State University (SDSU) as the first order of business (Fig. 3). Light-duty commercial diving provided a



Figure 2. An early scuba training dive off the Humboldt Bay entrance jetty, 1978.



Figure 3. First southern California kelp dive at Casa Cove, La Jolla, California, 1979.

reasonable income for the next three years as an undergraduate student. The kelp beds (*Macrocystis pyrifera*) and rocky intertidal areas became my office for the next eight

years. I was hired by the Department of Biology as a staff biologist in charge of marine collections in 1982. During my years of graduate studies I investigated the life history and population dynamics of *Octopus bimaculoides* (Lang, 1997) in Agua Hedionda lagoon in Carlsbad, site of the recent discovery of an introduced green alga (*Caulerpa taxifolia*), known for outcompeting and displacing native species through the northern Mediterranean. During my tenure at SDSU, I participated in numerous scientific diving expeditions to the California Channel Islands (Lang and Hochberg, 1997), Baja California, and a most stimulating trip to the Antarctic on an *Euphausia superba* research diving project with Bill Hamner from UCLA.

Scientific diving in the United States can be traced to Scripps Institution of Oceanography in 1951. Conrad Limbaugh (Fig. 4a) was appointed by Roger Revelle as the first Diving Officer of the University of California's scientific diving program. This program provided for scuba training, equipment maintenance, medical and operational underwater research procedures—elements that are still found in diving programs today. Jim Stewart (Fig. 4b) succeeded Limbaugh in 1960 after his tragic death in a submarine

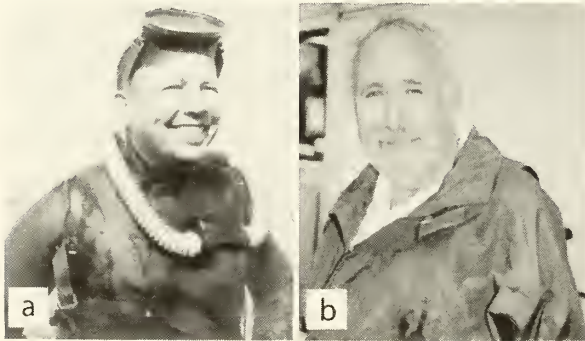


Figure 4. Scripps Institution of Oceanography Diving Officers: (a) Conrad Limbaugh, 1951-1960. (Photo courtesy Scripps Institution of Oceanography) (b) Jim Stewart, 1960-1991. (1993 Photo M. Lang)

cave diving accident in Port Miou, France at the age of 35. The *Capricorn* expedition to the South Pacific in 1954 was the first research diving cruise to study coral reefs. Those were the days of double-hose regulators and twin tanks. There were no buoyancy compensators, submersible pressure gages or dive computers. When it got hard to breathe, divers came up following their smallest bubbles, or sometimes completely disregarding the ascent rate based on the length of time they had been out of air! J-valves were incorporated on scuba cylinders in the 1960s as a safety mechanism in order to provide a reserve

supply of air of approximately 300 psi. At issue was the pull-rod often activating the valve during the dive, unbeknownst to the diver, leaving no reserve air supply. Furthermore, empty cylinders needed to be filled with the valve in the open position to incorporate the reserve volume of compressed air. The advent of submersible pressure gages provided an analogous reasoning as to why you don't run out of gas in your car on the freeway.

Buoyancy compensation was solved in the late 1960s through the introduction of the frontal horse-collar buoyancy compensator (b.c.), which was perhaps the most uncomfortable of all personal flotation devices, riding up around the neck at the surface and providing a continuous reminder to not cinch the crotch-strap too tightly.

Scientific diving techniques had established themselves in the peer-reviewed literature as particularly useful by putting the trained scientific eye in the underwater environment. In a 1953 letter, Roger Revelle, Director of Scripps, wrote to University

of California President Sproul stating that scuba should be accepted as a legitimate means of conducting research. Mainstream journals such as *Limnology and Oceanography*, *Marine Biology*, *Journal of Experimental Marine Biology and Ecology* and *Bulletin of Marine Science* regularly contain articles where scuba techniques are described in their materials and methods sections. Scuba equipment had also evolved from the days of double-hose regulators and twin cylinders with no pressure gauges when scientists monitored their decompression status through the use of the U.S. Navy Decompression Tables. Commercially available diver-carried decompression computers first arrived on the market in 1983, manufactured by ORCA Industries. These dive computers revolutionized the effectiveness of our research time under water by allowing for the tracking of nitrogen loading through a multilevel algorithm compared to the square-wave depth and time profiles the U.S. Navy tables required.

I had become intensely interested in diving physiology and how it affected our scientific diving operational procedures. Through the American Academy of Underwater Sciences (AAUS), with which I had been affiliated since 1980 and during the days of pursuing our Occupational Safety and Health Administration (OSHA) scientific diving exemption from commercial diving standards, I initiated a three-part diving safety research project that involved an interdisciplinary, industry-wide effort. The initial phase (Lang and Hamilton, 1989) was to investigate the applicability and effectiveness of dive computers. Subsequently, rates of ascent and safety stops were examined in an effort to further reduce rates of decompression illness (Lang and Egstrom, 1990). I was also interested in learning how many dives one could make for how many consecutive days prior to having to stay on the surface for a day to allow for significant off-gassing of the slow-tissue compartments of the dive computer algorithms (Lang and Vann, 1992). My colleagues in the hyperbaric medical community and the diving industry can be credited, in part, for the 1991 DAN/Rolex Diver of the Year Award for my significant contributions to diving safety. Further diving research included a two-year National Science Foundation (NSF) Ocean Sciences project evaluating diving safety from research vessels, an NSF Polar Programs project examining diving in extreme polar environments (Lang and Stewart, 1992), critically examining and modifying one of diving's most hallowed rules of "dive deep first followed by subsequent shallower exposures" (Lang and Lehner, 2000) and, most recently, reviewing the status of oxygen-enriched air (nitrox) diving (Lang, 2001). The Undersea and Hyperbaric Medical Society's (UHMS) Craig Hoffmann Award (2000) recognized this diving safety research as an outstanding contribution to the medical and diving communities.

My first diving involvement with corals, other than *Corallium rubrum* in the Mediterranean on a 1983 collecting trip with the Aquarium in Monaco and cup corals *Balanophyllia elegans* and *Paracyathus stearnsi* off California, came immediately after accepting my current position of Scientific Diving Officer at the Smithsonian Institution in January 1990. Jim Norris of the National Museum of Natural History (NMNH) invited me to the Smithsonian Marine Station to dive with Bob Sims and Sherry Reed in the Florida Keys on his *Liagora* project. My first impression of beautiful high-relief coral reefs at Looe Key was of a greatly reduced three-dimensionality compared with

the majestic *Macrocystis* forests I was so familiar with. Nevertheless, tropical water temperatures and visibility were most agreeable. Clyde Roper, Mike Sweeney, Jaren Horsley and I conducted a diving research project on *Octopus chierchiae* in Panama in 1992 to continue the earlier work done at Smithsonian Tropical Research Institute (STRI) (Rodaniche, 1984). In 1993, I accompanied the NMNH Fish Division staff (Jeff Williams, Carole Baldwin, Bruce Collette, Dave Johnson and Dave Smith) to the Kingdom of Tonga for several weeks to collect and document the ichthyofauna in collaboration with the Tongan Ministry of Fisheries. Of note were the pigs foraging in the intertidal, royal fruit bats, and upside down *Pseudanthias* (Fig. 5).



Figure 5. *Pseudanthias* swimming upside down under coral overhang in Tonga. (Photo M. Lang)

Hines, Smithsonian Environmental Research Center (SERC), José Espino, my STRI Diving Officer, and I taught a scientific diving course at the University of Honduras in Tegucigalpa, conducting the open water training dives at the Cayos Cochinos Lab funded by the Honduras Coral Reef Fund (Fig. 6). Several research cruises aboard STRI's *R/V Urraca* to Honduras included Jeremy Jackson as chief scientist and my dive buddy searching for bryozoans at 130 feet on mud bottoms. A Kodak moment materialized as the bottom trawl was near the surface and Captain David West accidentally knocked Jeremy's glasses into the water. Jeremy's immediate response was to cancel the cruise; we were going home because of an imminent onset of severe

The STRI San Blas station in Panama was for many years the home base of many Smithsonian dive buddies, including Jeremy Jackson, Haris Lessios, Ross Robertson, Nancy Knowlton, Ken Clifton, Hector Guzman and scores of visiting coral reef scientists. In 1992, I accompanied Nancy Knowlton on a research cruise to Cayo Salar for her continued work on the *Montastraea* sibling species complex (Knowlton *et al.*, 1992).

Between 1995 and 1997, STRI conducted a major coral reef research project at Cayos Cochinos, Honduras, headed by Hector Guzman (Guzman, 1998). Tuck



Figure 6. José Espino (2nd from right), STRI Diving Officer from 1992-1999, and Tuck Hines from SERC (3rd from right), with students from the University of Honduras at Cayos Cochinos during scientific diving training course, 1995. (Photo M. Lang)

migraines and his lack of a spare pair of glasses. Captain West said “adelante, pues!” Within two minutes the trawl was hauled. Lo and behold, it contained not only invertebrates, corals, and bottom fish, but also the lost glasses, still intact.

Coral reefs are unique biogeological structures that thrive in clear, nutrient-poor (oligotrophic) tropical oceans and support a rich and diverse biological community. Reef systems are driven by the symbiosis between scleractinian corals and microscopic dinoflagellate algae (zooxanthellae) as their chief energy source. The largest, best-developed, least-polluted and least commercially exploited coral reef in the

Atlantic region is the Belize Barrier Reef. This 250-km-long complex of reefs, atolls, islands, oceanic mangroves, and seagrass meadows has been declared a World Heritage Site. Carrie Bow Cay, in the early 1970s, only three hours by plane and boat from Miami, was found to be an ideal Smithsonian logistical base because of its location on top of the barrier reef, only meters away from a variety of habitat types (reef flat, deep spur and groove, patch reefs, seagrass meadows and mangroves), and its undisputed ownership by a Belizean family. In the years since, the Caribbean Coral Reef Ecosystems (CCRE) Program, under Klaus Ruetzler’s leadership, has amassed an enormous database consisting of thousands of specimens of marine plants, invertebrates, and fishes. CCRE has also helped the Belize government shape its coastal conservation policy, has participated in the Caribbean-wide reef monitoring network (CARICOMP), has established the first meteorological-oceanographic monitoring station in coastal Belize, and above all, has published well over 600 scientific papers in reviewed journals, as well as several books, doctoral dissertations, popular articles, and photo and video documentaries. Several projects in the past several years were centered on the Pelican Cays, an undisturbed and highly diverse group of reef-mangrove islands 15 km SSW of Carrie Bow Cay. The atoll-like reef structure on which the cays are located is obvious only from the air.

Emmett Duffy’s study of a sponge-inhabiting shrimp (*Synalpheus* spp.) confirmed its eusociality and advanced social structure for the first time in a marine animal (Duffy, 1996). I had dived with Emmett at STRI’s San Blas station in 1990, and also at Carrie Bow Cay, to collect sponges in 1992. A monitoring program was established to quantify the long-term temperature change effects on the distribution and progress of black-band disease in reef corals. A bleaching event in 1998 killed almost all the corals in the Pelican Cays and those in the surrounding lagoonal area (Aronson et al., 2000).

Hurricane Mitch (1998) could not claim to have done to Carrie Bow Cay

facilities what a devastating fire did in December 1997. New and improved for the 1999 season, the CCRE program continues to host Smithsonian scientific divers in their quest for increasing knowledge of the marine environment and its component parts. Ruetzler and Macintyre (1982) published the early coral-reef work at Carrie Bow Cay on the Belize Barrier Reef. A significant number of these studies were accomplished using scientific diving techniques. On numerous diving trips to Carrie Bow Cay since 1990, I have had the pleasure of collecting and photographing fish with Jack Randall, scientific diver extraordinaire, Carole Baldwin (Fig. 7), Kassie Cole and others.



Figure 7. Carole Baldwin (NMNH Curator) collecting blennies at Carrie Bow Cay, Belize, 1993. (Photo M. Lang)

About 25 minutes into this dive, Wolfgang gave me the out-of-air signal, wanting to share air. I obliged by giving him my AIR II alternate regulator connected to my buoyancy compensator dump hose. After verifying that his submersible pressure gauge did in fact read zero psi of pressure, I motioned for him to dump his 5-gallon bucket of sand, which he wasn't willing to do. Ascending with my left hand on our boat's anchor line, my right hand on his bucket's handle, Molly gracefully swimming up with us, and Wolfgang with lockjaw around my second regulator, I had an enlightening moment. I contemplated dumping the bucket. Then I realized I could not dump air out of my b.c. since Wolfgang had the free end with the regulator in his mouth. Reconsidering, I was now worried that the rusted bucket handle might give way and we would suddenly lose this negative ballast and rocket to the surface together, not a particularly enticing option. After a few Belikins (Belizean beer) and lots of deep philosophical discussions we opted to avoid that scenario in the future.

Also at Carrie Bow Cay, Mark Littler, Diane Littler, Barrett Brooks, and I spent a week diving at 190 feet in 1995 off the outer ridge to collect deep algae (Littler and Littler, 2000). We also further explored the underwater cave at Columbus Cay described in Ruetzler and Macintyre (1982). Some of the CCRE program research focus has shifted in recent years to a greater emphasis on mangrove ecology and the unique environment of the Pelican Cays where teams of divers led by Ian Macintyre, Jim Tyler, Mark and Diane Littler, and Ken Sebens worked on biotic and abiotic aspects of this fragile "mangrove-on-coral" ecosystem. A year earlier, Carole Baldwin and I conducted

Another Kodak moment was snapped when, after a dive on the ridge, Klaus Ruetzler and I had to swim a zodiac with a recalcitrant engine all the way back to the island through swarms of *Linuche*. Zodiacs and old Johnson outboard engines, it turns out, are not very effective as artificial reefs, much to the disappointment of Mike Carpenter, long-time CCRE Operations Chief.

In another diving incident, Wolfgang Sterrer, Molly Ryan, and I were diving in the sand trough at 90 feet collecting the top 5 cm of sand for Wolfgang's Gnathostomulida research.

a series of “black-out” night dives at 20 m on the outer ridge in search of the same elusive flashlight fish that had been collected just 70 miles away in Roatan. Despite some large swimming shapes outlined by the disturbance of bioluminescent plankton in the pitch-black water, the blue-green blinks of the flashlight fish were not observed.

Coral reef research experienced a banner year at the Smithsonian in 1996. The 8th International Coral Reef Symposium (8ICRS) was hosted by STRI (Lessios and Macintyre, 1997) and included six diving field trips. I was responsible for authorizing its 58 participants under the Smithsonian’s Scientific Diving Program. Field trip destinations were the Galapagos Islands (Wellington, 1997), Belize (Macintyre and Aronson, 1997), Curaçao and Bonaire (Van Veghel, 1997), San Blas (Clifton et al., 1997), San Andres and Providencia (Geister and Diaz, 1997), and the Pacific coral reefs of Panama (Glynn and Maté, 1997). As I had never dived off Galapagos or Curaçao/Bonaire before, both trips as Diving Supervisor were rewarding, especially with Jerry Wellington (Fig. 8) and Manfred Van Veghel serving as fearless trip leaders.

Also during the Smithsonian’s 150th anniversary year, I hosted and chaired the 16th annual AAUS scientific diving symposium “Methods and Techniques of Underwater Research” in Washington (Lang and Baldwin, 1996). Coral reef research papers presented by SI staff (Ken Clifton, Carole Baldwin et al., Bruce Collette, Haris Lessios, Mark Littler et al., Ian Macintyre, and Klaus Ruetzler) focused on scientific diving and collecting techniques of sponges, fishes, algae and drill cores.



Figure 8. Jerry Wellington in the Galapagos during 8ICRS field trip with a pair of mockingbirds, 1996. (Photo M. Lang)

Several of Ross Robertson’s STRI underwater research projects investigated the biological characteristics of small-island endemics in the eastern tropical Pacific and have taken his team of Smithsonian scientific divers to the Revillagigedo Islands, Clipperton Island, Cocos Island, Galapagos Islands and Malpelo Island (Allen and Robertson, 1994). Clipperton Island, which is the only atoll and the largest coral reef in the eastern Pacific, is the most isolated reef in the tropical Indo-Pacific, 950 km from the nearest shoals (Sachet, 1962). The following four biological aspects of endemics relevant to the question of mechanisms of their persistence at the island were studied in 1998: population size, longevity, larval biology and historical biogeography. The transit time aboard the R/V *Urraca* from Acapulco to Clipperton was three days. The two weeks of diving on site provided for encounters with silvertip and Galapagos sharks (Fig. 9a), manta rays (Fig. 9b), and

Mexican tuna fishing boats at this remote French territory. Our standard diving procedure was to launch two rigid-hull inflatable boats (RIBs) from the R/V *Urraca* with a team of eight divers. Toward the end of one dive, as we approached the anchors of the two RIBs moored side by side, I noticed two anchors but only one attached to an anchor line and ascending rope. Looking up, the silhouette of only one boat was visible. The 5-m RIB’s shackle pin had unscrewed itself (no safety wire) and set the boat adrift. Ross and I immediately surfaced to see the 5-m inflatable rearing vertically and performing headstands in the 4-m high breakers in the surf zone about 400 m away. We ditched the anchorline of the 4-m RIB and our dive buddies and rescued the 5-m from a

potentially terminal reconfiguration.

A two-week research diving cruise to Cocos Island, Costa Rica in 1997 with Robertson (Fig. 10) and others ranks among the greatest fish biomass dives I have ever logged, bar none, including Galapagos in 1996 and 1998. Noted in my dive logs are records of huge schools of *Caranx sexfasciatus*, baitballs, 300-pound yellowfin tuna

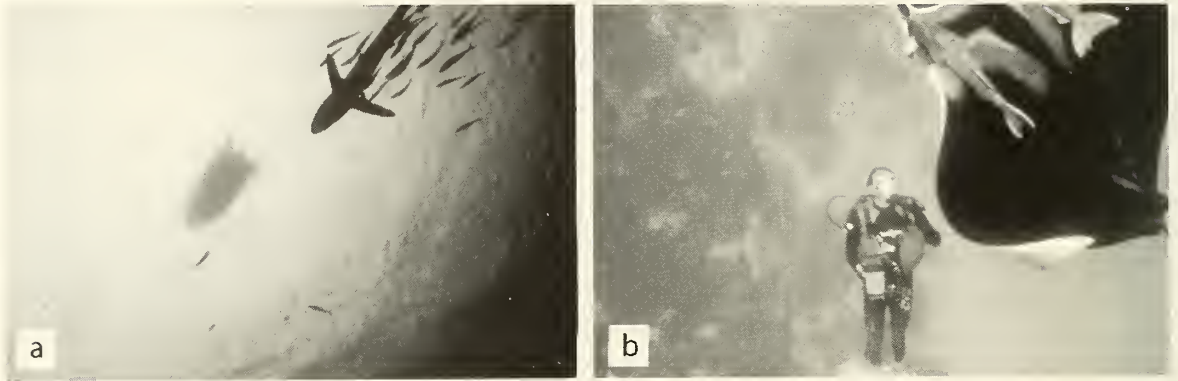


Figure 9. Underwater wildlife at Clipperton Island, 1998. **a** A precarious ascent from a dive. (Photo M. Lang); **b** Michael Lang eclipsed by a manta ray with attached remoras. (Photo K.Clifton)

feeding on reef fish, enormous schools of hammerhead sharks, huge Galapagos sharks, and manta rays. Perhaps the most unusual observation was the schooling behavior of white-tip reef sharks (50 or more) due to their sheer abundance on the reef.

A week of night diving at Bocas del Toro in 1999 to observe coral spawning came close to some of the underwater scenes in the movie *The Abyss*. Transects were marked at 5-m intervals with chemical lights and divers swam around with bright underwater lights searching for coral colonies. In the *Montastraea annularis* complex in the Caribbean, mass spawning usually occurs over a several-day period following the August full moon. Polyps in a colony produce gamete bundles (one per polyp) that contain both sperm and eggs. These bundles are constructed before spawning and become obvious about 30 minutes prior to spawning as the bundle works its way



Figure 10. Ross Robertson (STRI) collecting cryptic fishes at Cocos Island, Costa Rica, 1997. (Photo M. Lang)

through the pharynx of the polyp (termed “setting”). After release, the gamete bundle floats slowly to the surface. As it approaches and reaches the surface, the gamete bundle breaks apart releasing the eggs and sperm into the water column. The eggs are positively buoyant, but the sperm are neutrally buoyant. Because self-fertilization is rarely successful, sperm from one colony must find eggs from another colony. Members of the *M. annularis* species complex (Knowlton et al., 1992) are the major reef-building corals in the Caribbean and

a model group for studies of the ecology and reproduction of reef-building corals. The major focus of Nancy's spawning study was to provide a comprehensive understanding of the spawning behavior, gamete compatibilities, and fertilization rates of the three species that make up the complex (*M. annularis*, *M. franksi* and *M. faveolata*). All three spawn in approximate synchrony, typically seven to eight days after the full moon in August. However, *M. franksi* spawns one to two hours before the other two species, and the two species that spawn together have barriers that block fertilization between them.

When a "setting" colony (Fig. 11a) was encountered at Bocas del Toro, Panama, a numbered chemical light (attached to a weight with an identification number) was activated to mark the colony. The matched numbers allowed for mapping of the coral colonies and matching them with the fertilization samples. When a colony started to spawn (Fig. 11b), the diver recorded the time and detached the chemical light from the weight. The gamete bundles were followed to the surface (off to the side to minimize scuba bubbles interfering with the gamete bundles) and periodically during the ascent a sample of gamete bundles were collected with 60 cc syringes (Fig. 11c). The chemical

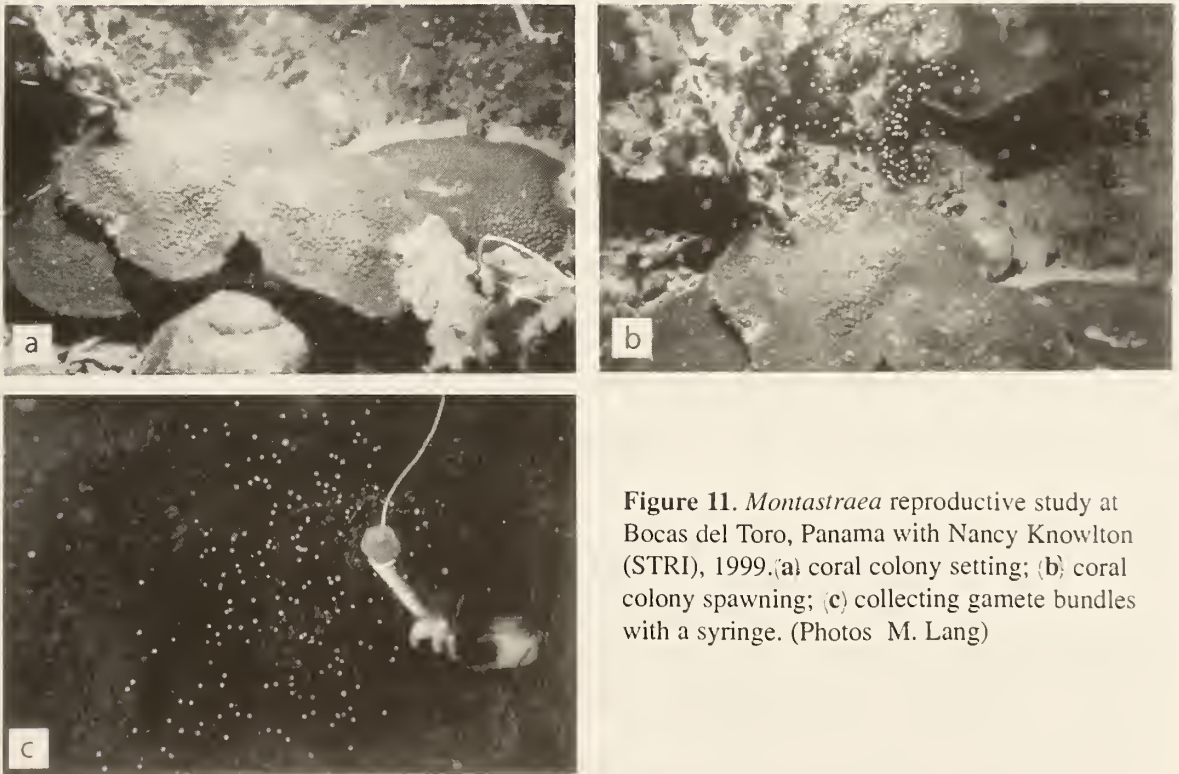


Figure 11. *Montastraea* reproductive study at Bocas del Toro, Panama with Nancy Knowlton (STRI), 1999. (a) coral colony setting; (b) coral colony spawning; (c) collecting gamete bundles with a syringe. (Photos M. Lang)

light on the water's surface then acted as a drogue around which plankton sweeps were made to collect eggs by other scientists in boats, during which the time and a Global Positioning System (GPS) reading were also recorded. By following the lights over time, the distances typically traveled by eggs and sperm can be estimated. This work provided the first information on in situ rates of fertilization success and the mechanisms that influence fertilization rates for any Caribbean hard coral. This fundamental process is often difficult to study because small larvae are inherently

difficult to track, and reproduction itself may be a brief event, which is easily missed by divers (especially at night) who necessarily spend a relatively limited amount of time under water.

In case you have hung up your fins or are not a scuba diver, the closest you might come to reminiscing about the underwater world or contemplating certification is to visit the NMNH Johnson IMAX Theater. The Smithsonian's 3-D IMAX film *GALAPAGOS* is your virtual ticket to the underwater world. We spent June and July of 1998 aboard Harbor Branch Oceanographic Institution's R/V *Seward Johnson*, complete with underwater film crew, scientific staff and the *Johnson Sea-Link* submersible. A number of the Galapagos islands were visited and most spectacular were Wolf and Darwin, the northernmost islands. The El Niño conditions of 1998, a tragic ultralight accident, and the technological difficulties of filming with a 1,700-pound underwater 3-D IMAX camera and housing made it necessary to reshoot certain sequences in February and March of 1999.

The Smithsonian Marine Science Network (Lang and Hines, 2001) is uniquely positioned to monitor long-term change at its component sites (SERC, SMSFP, Carrie Bow Cay, Bocas del Toro, Galeta, Naos and Coibita Island). It has an extensive array of programs involving scientific diving that address many of the most pressing environmental issues in marine ecosystems including: biological invasions, eutrophication, harmful species and parasites, plankton blooms and red tides, linkages among coastal ecosystems, global warming including sea-level rise, El Niño/La Niña, UV radiation impacts, habitat destruction, fisheries impacts, ecology of key habitats (estuaries, coral reefs, mangroves, seagrasses, wetlands), and biodiversity inventories.

More complete and continuously updated information on the Smithsonian's Scientific Diving Program (www.si.edu/dive) and the Institution's Marine Science activities (www.si.edu/marinescience) can be found on the Smithsonian web site.

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David R. Stoddart (Fiji, mid 1970s)

BE OF GOOD CHEER, MY WEARY READERS, FOR I HAVE ESPIED LAND

BY

DAVID R. STODDART

Scholarly and indeed devout readers of the *Atoll Research Bulletin* will at once recognize my title as a loose translation of an aphorism of Diogenes published in the eponymous *Nashes Lenten Stuffe* by Thomas Nashe in 1599. Pressed to say something on the fiftieth anniversary of the journal, which has been so central to the emergence of coral reef science as an independent discipline and in which I have played a part over some four decades, I might well paraphrase both Diogenes and Nashe to say, “Life is landfalls.” This is particularly true for anyone concerned with coral reefs: there is always a sense of both relief and amazement to be back on dry land and to have escaped the dangers of the deep. Perhaps more particularly for me, since I have made the study of the ephemeral sediment accumulations on the top of the reef and the plants and animals that came both to espy them and survive on them one of the chief scholarly concerns of my life. In this reminiscence I will pretend, as in writing a scientific paper, that life’s strategy has from the beginning been planned with sagacity, wisdom, and common sense. My many companions on reef studies around the world will know that a quite different version can readily be written. But then—as they also know—I could do the same for them. So let me try to recall some of my salient moments on coral reefs around the world and in the emergence of coral reef science as a discipline.

Why I have always been attracted to foreign lands in general and the tropics in particular remains a mystery. Certainly, during my early life my parents never traveled more than 50 miles from the small market and industrial town in northeast England where I grew up. It was famous for being at one end of the first passenger-carrying railways in the world (the Stockton and Darlington Railway) in 1825, as well as the home of John Walker, the inventor of the Lucifer match in 1827. My father was a frugal and deeply honest man who had served in France and Belgium in biplanes with the Royal Flying Corps during the First World War. My mother was a nurse in France at the same time. Both their travels were, of course, involuntary. My father, engaged in some construction project, later lived for a time in Ekaterinburg in the southern Urals, where the Tsar and his family were murdered. It was the beginning of Stalin’s Great Terror, when comparable engineers from the British firm of Vickers were seized and put on show trial for their counterrevolutionary activities. My father had the wits to get out while he could, by taking the train to what had become Leningrad and then back home.

It was at this euphoric point (and doubtless in consequence of it) that my own life began. The family at that time could scarcely be described as comfortably well off.

It was the period of the Great Depression: my mother's proudest boast in later years was that in contrast to many of her neighbors, her own three children always had shoes on their feet. So it came to be that I was born in 1937 and am thus of roughly the same age as the Golden Gate and Bay Bridges in the area in which I now live. It was also the year of the ridiculous introduction of *Rhizophora* into Moorea from New Caledonia. Likewise, it was the year of the disastrous introduction of *Mikoinia calvescens* to the botanic Garden in Tahiti, whence it escaped with utterly devastating consequences for the vegetation of the Society Islands.

One of my earliest memories was in September 1939 when I reached for a banana in a fruit bowl. My mother said: "You may as well have it—you will never see one again." Why should that be? Where did it come from? The next several years were strange ones in which to grow up. My father had dug a partly underground air-raid shelter in the garden behind the house. It was a nightly occurrence to be carried into it in the middle of the night; as a special treat, he would let me peek out of the heavy timber door at flaming airplanes crashing out of the sky. Occasionally, I would be taken to see the remains of neighbors' homes demolished in the night.

My much older brother was in the army. Three times during the war, my parents had a telegram pronouncing him "missing, believed dead." Kindly neighbors, I recall, assured my mother that once you had one of those they never came back. In fact, on the first occasion he had simply fallen asleep on sentry duty while the Army fled during the fall of Norway. Then he disappeared at the fall of Tobruk. Later he was in the Long Range Desert Group (the Desert Rats) creating havoc far behind enemy lines; he never liked to talk of the killing this involved. About once a year, he used to turn up unexpectedly at home with extraordinary stories of dare-and-do, together with a collection of service revolvers, German naval dress swords and bayonets, and swastika flags. Perhaps the only occasion my father was truly cross with me was when I put one of the latter outside my bedroom window on VE Day; it is amazing the house was not burned to the ground. I still have photographs of Field Marshall Goering that my brother had taken from a German corpse. It early became apparent to me that there was a world beyond the limited horizon of Stockton-on-Tees.

There were three excellent schools in our town, two of which were devoted entirely to girls. I never knew until years later that one of the founders of the International Society for Reef Studies and later its vice-president, Barbara Brown, was a pupil at one of them. At the age of 11, I went to the senior of these. It has since been razed to the ground but at that time had a quite bizarre collection of deeply memorable teachers. It was a turning point in my life that my first geography teacher there was Colin Nichols, who had been a student at Fitzwilliam in Cambridge under N. J. G. Pounds. It was he who suggested I go to Cambridge (my school had never had a pupil go to a great, or perhaps any, university before). My parents were appalled. Cambridge was not for the likes of us—we would always be the poor man at the gate—so it was laid down in the Church of England *Hymnal*, and one could not argue with that. I was sent out of the house, and Mr. Nichols argued them into submission. It was the first great turning point of my life: it was for me a commitment to science and letters at the

greatest university in the world (and the most user-friendly library system I have ever found). Colin soon moved on. His place was taken by Ken Stott, a Dalesman from the north of England and a true gentleman and friend; sadly he died suddenly a year ago. Both were wonderfully supported in their outreach to students by their wives Betty and Joan.

So it came to pass that in December 1955 I went up to Cambridge to take the entrance examination, having scraped through the mandatory Latin qualification (I never had Greek but wife has, which is a help). My school days had been mundane, but I had two passions. One was for the geography of Tibet and the Himalayas. I spent two years compiling an atlas of Tibet (the local public library borrowed books for me from all over the country). It had one map I drew that I think fondly of in retrospect—accessability in Tibet by yak-days from Lhasa (years later, I learned that British explorers had done the same thing for camel routes in Arabia). This passion was strongly fueled by British success on Mt. Everest at the time of the coronation of Elizabeth II, in 1953. Only last year, the *Times* reporter on that expedition, then known as James Morris, came to our home. She (as he became after rather horrendous surgery in Casablanca in the 1960s) clearly enjoyed life and indeed her own consequent notoriety to the full. The leader of the expedition, Lord Hunt, was rather bemused by the transition. I remember his deep puzzlement when he told me at dinner one evening that the body of a lone eccentric British mountaineer had been found on the north slopes of Everest in the late 1930s, dressed in women's underclothing. To change sex was one thing, but to be a transvestite quite another, especially for a British officer and gentleman-mountaineer.

I did in fact end up looking at climatic change in Tibet in the 1980s, a couple of times surveying in horizontal sleet above 17,000 feet and rephotographing glaciers recorded during the Younghusband Expedition in 1904. Absurd, really, because I have no head for heights. As a child I was quite petrified with vertigo at climbing to the top of the tower of Durham Cathedral. It was even worse the first time I stood on the medieval wall at Constantinople, and it occurred also on the absolutely vertical drop-off of the reefs around Palau.



Another important childhood experience—the reason for which I cannot now recall—was that I got the local library to obtain from the university in Newcastle-on-Tyne a copy of William Morris Davis's book, *The Coral Reef Problem* (Fig. 1). This attracted me greatly, partly because of the logic of the arguments (some of which I have since shown to be a delusion), but also because of the aesthetic appeal of many of Davis's own illustrations, particularly his elegant block diagrams. I copied many of them, as much for the detail

Figure 1. How it all began. I wish that I could say that the book I was holding was *The Coral Reef Problem* by Davis—it would have been much more interesting. 1950.

of the humid tropical landscapes in the background as for the reefs in the foreground. After all, as Davis famously said, what is the point of going to look at a reef when you can work out all its history in your head without recourse to facts?

So when I went up to Cambridge as an Exhibitioner from the provinces in 1956, I had determined to be a tropical geographer; coral reefs as such really did not enter the equation. I had the good sense to be admitted at the College of St. John the Evangelist, where the resident geographer was Benny Farmer, a noted specialist in south Asia and especially what was then Ceylon. He was extraordinarily broad-minded on subjects academic, and equally liberal with sherry during his weekly hour-long meeting with students. One came to know many of the leaders of British geography in his rooms in the evening. The alternative had been another Cambridge college where the lead geographer was an archreactionary who, in the old Cambridge tradition, had never published anything of note at all. It did not help that I had by then decided that I was not prepared to waste my life shaving every day and he absolutely refused to be associated with anyone with a beard.

Cambridge opened every intellectual door. One could still borrow eighteenth-century scientific classics—Hutton’s *Theory of the Earth*, for example—and cycle away from the university library with them in one’s basket; in the summer months, E. M. Forster would be put outside the front gate of King’s to be photographed in his dotage. My own doctor had also been Wittgenstein’s, who died in his house. The Master of the College was Sir James Wordie, who had been with Shackleton on *Endurance* when the ship was crushed in the ice and the ship’s company made their extraordinary open-boat voyage to Elephant Island; he subsequently achieved fame as an Arctic explorer: I once found him when I was an undergraduate wandering around Second Court asking where he was; I was able to take him back to the Master’s Lodge. Those were the days when to be outlandishly eccentric was a mark of distinction in a don—it greatly added to the interest of life. There was a lady in Cambridge who walked everywhere—across the street and onto buses—with a red plastic bucket over her head. Another character who carried a family of rats on his person was reputed to have been a former Fellow of Trinity. Much later one of my students, a direct descendant of Whistler, released a rat (which lived inside his shirt and which he always took to lectures—once it got out) at a dinner for my fiftieth birthday: he dropped it at the doorway to the kitchens and called the butler to protest at the stage of college hygiene. My tutors were Redford Bamborough, the philosopher, whom one met with him behind his desk and directly in front of him but 5 yards away in a single isolated chair: conversations in those circumstances were interrupted by very long pauses in which he reflected philosophically on what one had said. I once heard him say to a dinner companion: “Yes, indeed, I do believe I *was* born.” There was also the distinguished behaviorist Robert Hinde (later Master of the College) who supervised the great lady students of the higher apes in Africa. He surrounded his study with photographs of the higher primates, and as you talked with him he glanced repeatedly from you to one portrait or another. I myself occupied the rooms once lived in by L. S. B. Leakey; I was able to tell him I had slept in his bed when I met him on the high cliffs near Mombasa. Many of my fellow students had recently returned from the Korean War. They seemed to have an endless

supply of high explosives, which they routinely detonated in the underground lavatories in the market square, to the point where the vice-chancellor was obliged to make the entire center of Cambridge out of bounds on Guy Fawkes Night, November 5. The explosions rocked the entire square as well as destroying the toilets, which were closed down for years.

By the time I went to Cambridge I determined that I would be a tropical geographer. The place was ideal: in addition to my director of studies many of the active members of the faculty were also tropical geographers. It was ideal in another way, too: the academic year consisted of two terms of eight weeks and one of five, and even these numbers could be shaved—a fact I later made considerable use of when appointed to the faculty there myself. No American university has the intelligence yet to know that the advancement of knowledge and understanding depends in large degree on having the opportunity to browse in libraries, to go where the spirit takes you, simply to sit and think: going to a great university should be a leap into intellectual freedom and opportunity, not endless drudgery of marginally useful mandatory courses that exhaust both teacher and student with 45-hour lecture loads over 15-week semesters. And in England it was made possible by the excellence of the education system in the schools: teaching at the university became a matter of civilized conversation with students without the need to define words like “equator” or “longitude” or to explain where places like South Africa or Argentina (or dare I say Vietnam or Afghanistan?) are and what they are like.

Be that as it may, I planned my entry into tropical geography meticulously (this, of course, is retrospective justification). What kind of geography and where were still to be determined. It was clearly necessary to have a look at all three major tropical continents. I chose Asia first as there had been a long family connection with India (and indeed I suspect that could be said of most British families during the two centuries of the Raj). So during my first long vacation (the four-month break between academic years) I went to India, taking the train from London to Calcutta. The fare was 27 pounds one way, equivalent to perhaps \$125. In retrospect it was somewhat foolhardy. The previous year, there had been the disastrous Anglo-French intervention in Suez, and the Middle East was in turmoil. In fact, the railway line went through Syria and was out of bounds. So after Munich, Belgrade, Sofia, and Istanbul, I was obliged to fly between Ankara and Baghdad, where the train to Basra resumed. It was the year before King Faisal and his family were assassinated and the prime minister dragged to his death behind a vehicle in the streets of the capital. Through Iraq I was constantly violently unwell, a situation that has recurred repeatedly throughout my exploratory life.

In Basra, the only way to Bombay was by deck passage on a British India liner. I think it cost 9 pounds. This greatly annoyed the British India management: they did not want white men cavorting with natives on the decks of their ships. I was told: “We will allow it this time, but never again, and you will only be fed the slop we give to the rest of them.” And so we traversed the Persian Gulf: Bandar Abbas (where a Berkeley taxi driver has recently told me there is the finest brothel in the Middle East), Kuwait, and Muscat, in unbelievable temperatures and humidified. In Bombay, of course, the trains still ran on time. Lunch at the Calcutta Yacht Club was a great restorative. I had decided

to have a look at industrial locations in Bihar and traveled about quite a bit. In Jamshedpur (the location of the Indian Iron and Steel Corporation, which was overrun by sleeping cows) I was in an intensely crowded train compartment when a frenzied man burst in and stabbed my neighbor to death. It turned out he had just been fired by his victim. But I emerged from that and in the end got back to Cambridge (though I chickened out—I flew).

Having thus done Asia, in a way, I then turned to Africa. I decided on Sierra Leone, still a British colony and accessible by ship from Liverpool. My mentor, Benny Farmer, put me in touch with Professor Robert Steel, the noted Africanist in that city. Robert invited me to stay in his house before I boarded the ship. He was always an extraordinarily generous person with people he had never met but who were interested in things that he was too. He had done fieldwork in Sierra Leone just before World War II. Even then it was truly Graham Greene country, and he walked at the head of a line of Africans carrying everything needed. My own visit was post-Graham Greene, but there were many places in the diamond country too unstable to visit, but I did come up with a map of the distribution of house-types in the country and spent some time with the nomadic Fulani in the north. I had a look at the coastal landlords and mangroves in the south, around Bonthe: the barge I took there from Freetown was infested with the most gigantic cockroaches I have ever seen—it was sad that they hung from one's lips at night to drink and ate one's toenails to the quick while one slept. It was still the Empire: prisoners engaged in mindless rolling of 45-gallon drums of fuel around in a circle were detached to carry my bags. It was impossible to imagine the savagery into which the country descended after independence.

After that, there had to be South America. I decided on Colombia—no one in his right mind would do that now and anyone prudent would not have done it then. I decided to look at a tribe called the Tunebo, on the eastern flanks of the Andes in a catchment of the upper Orinoco. This involved taking a ship from Liverpool via Bermuda, Curaçao, the Panama Canal, the Pacific coast of Costa Rica, and ultimately Buenaventura in Colombia. We then proceeded by air to Bogota and onward by truck to the Sierra Nevada del Cocuy. This was all done from Bogota so rapidly that soroche (the mountain sickness) was inevitable. Once in the Sierra, we hired porters and mule teams for the long haul over the Eastern Cordillera and down the rain-drenched and precipitous eastern slopes of the Andes toward the llanos. It was something of a nightmare. I think I have never experienced more continuous heavy rain. Everything fell apart. Mules fell from the trail into gorges filled with clouds and were never seen again. Some portions of the trail were so steep that to get up them one had to hang onto the tail of the mule in front, and this resulted in living in a horrendous miasma of mule gas. On one occasion the horse I was riding bolted and the poncho I was wearing blew over my head. I could not spare a hand from the reins to pull it from my face and expected to be immediately decapitated by low branches across the trail. We got there in the end, however. Most of our porters had the most horrendous scars from endemic intravillage ambushes, even before the drug wars supervened. It remains the only expedition in my life where I routinely carried both a sidearm (a silver Smith and Wesson) and a rifle. On examining the former one afternoon, I inadvertently put a bullet through our radio.

We came back with recordings of the Tunebo festivals, and it led to a couple of papers. I thought I might become a Latin Americanist, but we were infested with fleas. The only thing to do at night was to put all one's underclothes outside on a line and then to get up sufficiently early in the morning when the fleas were still so soporific from the cold that you could pick them off one by one. We left Colombia by Cartagena and saw something of the Spanish Empire in New Grenada. In spite of all its upheavals, Colombia (like all the former Spanish territories) was an extraordinarily literate society; *Libraria Buchholz*, for example, was and hopefully still is, a world-class bookstore.

So my reconnaissance of the tropics had been completed. But things had changed in a way that was truly to define the course of my life. The head of the Department of Geography at Cambridge when I was a student was Alfred Steers, a man of stature, imposing but rather shy, the leading figure in the coastal geomorphology of Britain. He was both patrician and patriarch, and most ably supported in both roles by his wife, Harriet. To many he appeared remote, but in fact he was a most friendly man. Every year he invited the graduate students to his magisterial home to operate his extensive model railway—entirely constructed by his employees in the department—in the attic. He noticed me as a student at an early stage and was increasingly interested in my growing friendship with my future wife. She was my direct contemporary and a student at the exclusively female Newnham College where, unfortunately, her director of studies (a figure from the past in the historical geography) of Britain had the same name. The elderly Miss J. Mitchell must have been surprised at some of the communications from me that were sent to her in error.

Now here is a case of pure serendipity, on which I am prepared to argue that the future of coral reef science began. There was a tradition in Cambridge that undergraduates as a matter of routine went out on expeditions all over the world. That is how I got to take the party to the headwaters of the Orinoco. You begged and scrounged everything from cheap passages to tomato ketchup and somehow or other made a go of it. It was a quite exhilarating period. On the Colombian Expedition of 1959 I wrote to the Duke of Edinburgh for support: not only did he make a contribution but in mid-Atlantic the ship actually received a telegram from him wishing us “Bon Voyage.” Prince Philip subsequently became Chancellor of Cambridge University and has been without question the most interested, visible, and knowledgeable Chancellor in its entire history.

As we were setting out for South America, a group of fellow undergraduates had started to plan an expedition to British Honduras (now Belize), a country then virtually unknown except archeologically. It was to comprise three components: an archeological survey at the prime site of Xunantunich in the far west of the country (this was carried out by Euan Mackie, later the Keeper of the Hunterian Museum at Glasgow University: he used to wear a solar topee—as indeed did I (both courtesy of the British Honduras Police Department) but unlike myself he had a habit of shooting rats with an enormous revolver while lying in his camp bed at night. The botanical work was carried out by David Hunt, later a grass specialist at the Royal Botanic Gardens, Kew, mostly in the Mountain Pine Ridge. And then there was the reef party (Fig. 2). This was led and inspired by John Thorpe, who later had a distinguished career at various research



Figure 2. Rendezvous Cay reef party and assistants (left to right): David R. Stoddart, Modesto, Paul K. Bregazzi, Jack Reyes, William F.A. Warham, Viola, John E. Thorpe, and John D. Poxon. 1960. Tragically both Jack and Viola died a year later when Hurricane Hattie passed over this cay.

institutes and universities and has for years been senior editor of the *Journal of Fish Biology*. He was joined by Paul Bregazzi, whose doctorate later was on sub-Antarctic crustaceans with the British Antarctic Survey, and Will Warham, a farmer with experience with Guinness and later with Irish Television and with humanitarian aid around the world.

John saw the need for a geographer—someone who could tell these

zoologists where things were on the reef. Through the Head of the Zoology Department, Sir James Gray, he approached Alfred to ask if a suitable geographer might be available. Alfred recommended me. I was committed at the time to the upper Orinoco but agreed to get to Belize as soon as I could. Thus I returned to England from Cartagena at the end of the Colombian expedition and immediately set forth on an oil tanker to Trinidad, Curaçao, and Jamaica, on the way to British Honduras. John had organized with the government the use for a year of a small sand cay (Rendezvous Cay) on the barrier reef. Governor Sir Colin Thornley had had reerected there for his rest and recuperation a disused police station from the mainland, which was to be our headquarters. We also had a prime site in the center of Belize City above the main post office as our headquarters. By the time I was free of Colombia and back to the Caribbean, all the other members of the expedition were on site. There remained one important prerequisite for reef work: a boat. John located a total wreck on Cay Caulker, miles to the north. Somehow the hulk was taken to Rendezvous Cay, and the first months were spent in fitting a complete new bottom, caulking it, and even cutting and sewing sails. All this work was completely new to us and was supervised by another geographer, John Poxon, who hailed from Jamaica and had some idea what to do. Finally in December 1959 the 16-foot boat was put in the water, and immediately sank. But the caulking held, and *Tortuga* became the most important piece of equipment of the Cambridge Expedition to British Honduras, 1959–60. All this is described in a book later assembled by John Thorpe and David Carr, *From the Cam to the Cays*.

Now and undergraduate education at Cambridge was (at least at that time) arguably the best in the world—if in fact you were prepared to make use of it (which too many were not). It suited me down to the ground, and I did rather well there. But when I arrived in Belize it was at once apparent that I knew next to nothing about the coral reefs and reef islands that were to become the focus of my life. I recall precisely the exhilaration I felt on my arrival there, in the knowledge that I knew not a single

thing about any of the plants and animals, both terrestrial and marine, that would have to be central to any understanding of the natural world of the tropics. I know a little more now, but I still have that feeling whenever I go back to Belize.

So how to proceed? It was quite amazing that no one had looked at these reefs and islands before. After his expeditions to the Great Barrier Reef, Alfred Steers had worked with Val Chapman on the Jamaican cays and had in mind to go to Belize after that. But the onset of war both curtailed the Jamaican work and made Belize studies out of the question. Agassiz never went there (which from his work elsewhere in the reef seas was no loss to science); Darwin had little to draw on when he published his coral reef book in 1842. Richard Owen, one of the outstanding British hydrographic surveyors, and who in the 1830s had meticulously charted the reefs off the eastern coast of Nicaragua, had then charted the central barrier off Belize, but it was decades before his work there was followed up (in fact his survey of the Belize area was revised by Edmund Irving on H.M.S. *Vidal* shortly before we were there). So we began from scratch. There were two prerequisites for this.

The first was operational survival on *Tortuga*. We had rice, beans, corned beef, and Spam, and whatever fish we could catch. We had literally hundreds of donated tubes of toothpaste. Water was a problem. It will amaze graduate students today that we did not have enough money to buy plastic water containers. We eventually got one of the Chinese retailers in Belize to give us a wooden barrel that had contained pickled pig's tails imported from China. Try as we might, we could not rid that barrel of the intense flavor of its former contents. But it became our water supply. Then we needed mobility. We had a small Seagull outboard on the order of two and a half horsepower, but the propellor was only half in the water, and it was effectively useless in transportation. So we depended on the sails. Thus there were several occasions when we were totally becalmed, sometimes for an entire day within sight of our destination but unable to move. On those occasions we were roasted alive. But there were compensations. At first light one could go over the side and then breakfast on crayfish, and of course the barracuda were superabundant and when not poisonous totally delicious.

The second prerequisite was information. At that time (1950-60) there were no field guides at all. The best was Walton Smith's book on Atlantic reef corals. And fortunately the reef corals were few: usually about one species per genus. So it was not too difficult to become familiar with the stony corals. But so much else (seafans, sponges, algae) was really opaque. The same was true of the plants on the cays. I had not collected plants in the tropics before and had great problems in drying all of them in such a humid environment, especially the succulents. I shudder to recall how desperately awful many of my first collections were. But gradually we came to know what we were looking at.

The plan was to examine all the sand cays on the reefs, map them, and describe their vegetation. John Poxon successfully navigated *Tortuga* along the whole length of the barrier reef (some 200 miles) and did the same with the cays of Turneffe. Lighthouse Reef seemed a bit much, however, and we were glad to take passage there in the governor's yacht. So we did the cays there too.

Meanwhile the other members of the reef party were doing great things—

experiments on corals and other organisms, and especially the detailed mapping of coral distribution on the reefs surrounding Rendezvous Cay. This was a huge task and occupied John Thorpe especially for months. My job was to accurately locate the many marker buoys used for the underwater surveying. The coral distribution map that resulted was then and remains today the most detailed ever made of any reef in the world. Unfortunately, by the time we got back to England mundane matters like getting jobs supervened and the map was published only in outline, without specific identities of the organisms mapped. Last year, however, I found some money and four of us—John, Paul Bregazzi, William Warham, and myself—were able to look in detail again at



Figure 3. With John Thorpe on Rendezvous Cay, Belize, during the 40th anniversary re-survey in 2000.

a reef we had worked on exactly 40 years before (Fig. 3). With advancing years, I was pleasantly surprised to find that the individual coral heads mapped in 1960 had not changed their positions by the year 2000: that is, we could precisely find them again using identical techniques. The shock was that the wild luxuriance of the reefs in 1960 had become a scene of devastation. But I think that nowhere else in the world is there such a benchmark survey against which to measure the cumulative effects of catastrophic storms, bleaching, and disease as at Rendezvous Cay. This work continues.

When we left Belize in mid-1960, my own work was incomplete. There were cays in the Punta Gorda region in the south we had not seen and likewise we had not seen Glover's Reef at all. More to the point, as I drew up the

results, I realized what truly needed to be done. A priority was to identify the plants. Alfred Steers put me on to the professor of tropical botany at Cambridge, Edred John Henry Corner, one of the last true eccentric and overarching scholars that the university has seen. I went to see him; he said the man you want to talk to is called Fosberg, in Washington. The rest is history.

I was acutely aware of the need to upgrade my work in 1959–60. Alfred again came to the rescue. He knew everyone who was anyone in coastal geomorphology at that time. These included Richard Russell and Andre Guilcher (the latter I was to meet all over the tropics in subsequent years). He wrote to Russell, then director of the Coastal Studies Institute and Dean at Louisiana State University in Baton Rouge. He was then conducting research around the world on topics such as the beachrock problem, funded mainly by the Office of Naval Research. This was headed by the enormously influential head of its Geography Branch, Evelyn Pruitt. It was arranged

that I should go to Belize a second time, in mid-1961, funded through the Coastal Studies Institute and taking in Miss Pruitt in Washington on the way. She provided passage on Military Air Transport Service (MATS), the first of many times. This was



Figure 4. With Marie-Hélène Sachet at the Fifth International Coral Reef Congress in Tahiti. 1985.

my first meeting with Ray Fosberg.

He came to the now-demolished Raleigh Hotel in Washington, together with his co-worker Marie-Hélène Sachet (Fig. 4). It was my introduction to the Smithsonian Institution at a time when Dillon Ripley was Secretary. Ray and Marie-Hélène were then housed in an annex to the National Academy of Sciences close to the Watergate building (which then had quite a normal reputation). That meeting led to friendships with Ray and Marie-Hélène that lasted for their lifetimes. Ray agreed

to take my plants and told me how to handle them. I learned something about their commitment to coral reefs and islands and indeed the *Atoll Research Bulletin*. I left Washington with the distinct impression that I was now part of a great endeavor in a very definite scientific program. Dick Russell welcomed me to Baton Rouge and showed me the campus of ISU with pride. He provided another graduate student to help. This was Stephen Murray, who was to become a noted physical oceanographer and major figure in the Coastal Studies Institute.

Together we went to Belize. This time we chartered a sailing boat from the Young family at Half Moon Cay. In it we did the cays of Turneffe, Lighthouse Reef, and at last Glover's Reef; later we did the Punta Gorda cays and also took the opportunity to resurvey many of the barrier reef cays. At the end of it, I felt I had some understanding of the reefs and reef islands of British Honduras: chipping away to my ignorance I at last knew something of the reef animals and the island plants. The human geography was also not without interest. The colonial secretary acted out his role by apparently living entirely on pink gin. The American consul's previous job had been building nuclear weapons. There was a character aptly called Strangeways-Dixon who worked on disease transmission by insects; he used to draw a polar zenithal projection on his abdomen, centered on his navel, and was thus able to record the effects of any particular insect bite in terms of the altitude and longitude of where he had placed the vial containing it. On the first occasion that my wife came with me in *Ramrod*, Ronnie Young (our skipper) wandered down the dock an hour later. I asked him if he needed anything. He said: a pint of rum. Then he jumped off the quay and fell in a heap on the deck. Not so long after that he was by himself in a different boat on the way to Lighthouse and simply fell off the stern, leaving the vessel to proceed without him. But one staked one's life on such folk in the field.

It was unfortunately extraordinarily useful in another way too. Finishing the work in mid-1961 was followed at the end of October by Hurricane Hattie, one of the

great storms of the century. It passed directly over Rendezvous Cay. The storm surge flooded Belize City to a depth of 5 meters. Many people died, including the couple who we had employed at Rendezvous in 1959–60. I was back in Belize at Easter 1962 to start work again. In a fairly intensive period of fieldwork I resurveyed all the islands and looked at the effects on geomorphology and vegetation, as well as the impact on the reefs. Rendezvous Cay had been stripped of all vegetation. The house we had lived in was gone (I found the kitchen stove down the reef slope). In retrospect I am astonished at the almost manic way I did this fieldwork. But I knew that here was a quite unique opportunity to look at hurricane effects with both immediately before and immediately after studies. I had published my prehurricane work on the three atolls (I rather foolishly thought that the *Bulletin* was only interested in atolls) in *Atoll Research Bulletin* 87; this was followed by *Bulletin* 95 on hurricane effects across all the reefs. Later I went back in 1965 and did it all again. And again—with Ray and Marie-Hélène during the Comparative Investigations of Tropical Reef Ecosystems (CITRE) Workshop on Glover’s Reef in 1971.

I gained the Ph.D. in 1964. By that time I had been appointed to the faculty at the Department of Geography in Cambridge. I was in Belize in 1962 when I had a postcard from Alfred in a hotel on the Isle of Wight. “My dear David, would you like a job in Cambridge? Yours ever, Alfred.” No nonsense about curriculum vitae, referees’ reports, appointments committees: simply straightforward patronage. In the Cambridge context of the time, it worked. Unless one did something quite dreadful, it meant a job for life. My response was instantaneous: “Dear Professor Steers....” (i.e., what a good idea).

So where to proceed from the work in Belize? I took the same strategy as I had as an undergraduate: make it comparative. That meant the Indian Ocean and the Pacific. Professor Maurice Yonge, the leader of the Great Barrier Reef Expedition of 1928–29, had been one of my Ph.D. examiners. I still recall with some embarrassment my temerity when during the actual examination he denied that *Siderastrea radians* ever formed equidimensional free-living colonies (coralliths, as Peter Glynn subsequently called them): Maurice had published on that species from his work in the Dry Tortugas before the war. I excused myself, went to my office, got half a dozen, and rolled them across the table at him. I also pulled much the same trick with what Robert Ginsburg subsequently called rhodoliths, common on the seaward margins of Lighthouse and Glover’s Reef. I got the Ph.D. With anyone else, I wouldn’t. So I asked Maurice what to do next. I had in mind the Maldives. There has since the war been a staging post on the southernmost atoll of Addu (there was also an airstrip close to Male, but that was evoking some political discontent). There had been something of a Cambridge preoccupation with the Maldives since Stanley Gardiner’s expedition at the beginning of the century and then Seymour Sewell’s during the John Murray Expedition (I never met Gardiner but did know Sewell toward the end of his life). There was also the great advantage of free flights to Addu through the RAF (the British equivalent of MATS).

So I took a small party to Addu in the summer of 1964. The grant promised by Maurice was less than expected, but my father had recently died and so there was some money available. I thought of it as an investment for the future. After the Caribbean,

the Indian Ocean was quite a shock. The diversity of the reef corals was overwhelming. Much of the island flora was quite new. Things were also now different from the earlier expeditions: by this time there was a need to know more than the names of the plants and animals. This had been apparent when we first went to Belize: the paper by Odum and Odum on energy flow at Enewetak had just been published but not yet assimilated. Krumbein was publishing constantly on statistical sedimentology. One had to have transects, sampling designs, that kind of thing. We dutifully laid out the transects, but I rapidly found that trying to reach a random point on a reef flat pitted by military borrow pits was not a good idea; it was there I lost 2 Pentaxes (the first of perhaps 20) within hours of unpacking them, simply by falling into a hole in the reef in order to get to some particular random point. But I still have a museum piece, my first underwater camera: Cousteau's Calypsophot, on which the Nikonos was modeled. Not so many of those around today. So Addu was a learning experience: indeed, I broke a finger there and it has bothered me ever since. The results were published in *Atoll Research Bulletin* 116. "Much Addu about nothing," quipped a friend not exactly known for his fieldwork. He could not have been more wrong.

So that was the Indian Ocean. What about the Pacific? Maurice was chairman of the Southern Zone Research Committee of the Royal Society. The committee was well known for its sponsorship of major expeditions to places like Tristan da Cunha and southern Chile. It turned out that they were considering a large project in what were then the British Solomon Islands in the southwest Pacific. It was to be led by none other than Professor Corner. I was round to his office at once. The expedition was to begin in mid-1965. I had to go back for a further survey on the Belize reefs before then, but was otherwise free. There were to be a land party of botanists and zoologists, and a marine party. The latter was to be accommodated in a ketch that had formerly been the Queen of Tonga's yacht *Maroro*, which the Royal Society chartered for six months. So off we went from Guadalcanal, the first stop being Sandfly Passage in the Russells where the only previous scientific party had been killed and eaten on the beach (this habit evidently continued through the Second World War, and indeed we were not allowed to work on Malaita where it was feared it might continue). *Maroro* ranged widely through the Solomons, captained by Stan Brown, who lived in Suva (he later became head of the Fiji navy). He adhered to strong codes of naval etiquette (such as having all the scruffy scientists line the rails when entering or leaving port), but this did not extend to having a functional freezer. I most clearly recall the smell and taste of the rotten lamb served at dinner. The most interesting location we went to was New Georgia, with its tall volcanoes, double and even triple barrier reefs, and substantial tectonically raised reefs. It occurred to me that these latter could be used as an analogue of what happened to reefs during Pleistocene low sea-level stands. Rather foolishly, I spent a good deal of time alone running echo-sounding traverses in the Marovo and Roviana lagoons, which had not been done before. Fortunately, I managed to keep the outboard in the dinghy going when the ship was over the horizon. One was aware that the Pleistocene sea-level notches on the raised reefs were lined with skulls.

This expedition, I have since thought, must have been one of the last to have been carried out in the way that Cook, Banks, and the Forsters worked: to show up on a

remote and possibly hostile shore, collect and survey everything one could, and then sail away, never to be seen again. There were so many outposts of proselytizing faiths throughout the islands that Corner made it his business to plot them so that we never arrived at a community on whatever day of the week it was where any kind of useful activity was prohibited. Corner was quite a remarkable man. His first job after graduating was in the Singapore Botanic Gardens, just before the war. In his autobiography, he recalls the invading Japanese army swarming across the causeway and records that "I decided to throw in my lot with the victor." As a result, he was kindly interned for the duration in the Herbarium, during which time he wrote his first major book. Unfortunately, he dedicated it to the commander of the Japanese forces in Singapore who had just been executed. This did not go down too well, and he was obliged to spend some years in Brazil. But then Cambridge, knowing an eccentric when it sees one, brought him back. Not surprisingly, he finally fell out with his college over the admission of female students and never entered it again. White with rage, he leaped to his feet at the crucial meeting, threw his college keys at the Master, and stormed out, only of course to find that he had also thrown his car keys and house keys, too. The head porter had to negotiate their return. Emperor Hirohito gave him an enormous prize for science and Corner did the emperor's obituary for the Royal Society. I went to see him in England shortly before he died. I told him that I thought his various views on the evolution and distribution of the flowering plants were mutually inconsistent. He rose to the occasion and defended them mightily. I never saw him again. There is a wonderful obituary of him for the Royal Society by D. J. Mabberley, which, if one reads between the lines, memorializes him perfectly.

For me, simply to get to know Corner was one of the great experiences of the Solomons (there were other eccentrics there too). But there were other things as well. On the way there, it was my first experience of California and especially of Berkeley, where I subsequently came to live. And also of Hawaii (all the high islands), Fiji, and the New Hebrides. On the way back, I detoured again through the New Hebrides, then New Caledonia, and finally French Polynesia (the airport was new and the nuclear program had yet to wreak its devastation on the Tahitian economy and social order). Moorea had but a single hotel and few paved roads, and certainly no research stations. I went to Rangiroa in the Tuamotu, where there was still no sign of nuclear modernity or tourism at all. I am glad to have seen it then.

Very well: by the end of 1965 I had worked in the Caribbean, the Indian Ocean, and the Pacific, even if only dipping my toe in the waters. How to proceed? Serendipity again. The truly important outcome of my Addu expedition in 1964 was what I learned there. One became a member of a military community, and in the evenings at the incredibly inexpensive bar one could learn a lot. Specifically, there was much talk of new British military developments on Indian Ocean islands, for which surveys had apparently already begun. These included several islands in the Seychelles (then a colony) and others in the Chagos Archipelago, governed from Mauritius. I was too committed in Belize and the Solomons to do much about this in 1965, but early in 1966 I wrote to George Hemmen, a senior Royal Society staff member who had been with the Solomons expedition, in effect saying that before any of these islands were devastated

for dubious military purposes someone should have a look at them. George followed this up both within the Royal Society (through Maurice Yonge's Southern Zone Research Committee) and with the Ministry of Defence. It appeared that an expedition was shortly to depart for the Indian Ocean atoll of Aldabra. The Royal Society sought and was given two places in the party, for myself and an invertebrate zoologist at the British Museum (Natural History), Christopher Wright, who had previously shown an interest in the atoll.

So we appeared in Mombasa. The party was fronted by the British Broadcasting Corporation, which apparently wanted to have a relay station there, but its true significance was to determine the feasibility and cost of a major Royal Air Force facility. A ship had been chartered in Kenya for the duration (the *Southern Skies*). The whole thing was treated as top secret. So we were all set to go except the ship had no engineer—until a chap walked along the dock and said he was an engineer and could handle everything. He was taken on board at once. He was in fact and oddly enough a Russian. Finally, after several days, we reached Aldabra. It was apparent at once that none of the people we were traveling with had the faintest idea of how to cope with such a place. Brand new outboards were unpacked, lowered over the side on ropes too short to reach the water, and then dropped into the ocean (I am of course aware that this is how the military has gone on since time immemorial). The point was to determine the viability of a major airfield (“a staging post”) at the other end of the atoll, 20 miles away. All supporting facilities had to be located and costed too. The chief civil engineer was environmentally overcome by the place and never went near the proposed site, though he produced precise costings.

Once on shore, Wright and I ran around like lunatics, mapping and collecting. It was immediately apparent that Aldabra was one of the most remarkable islands on earth. We were there so briefly, saw so small a part, and were under such time constraints that I simply brought a number out of my head: at least, I said, a population of more than 10,000 giant tortoises (itself substantially more than the surviving populations in the Galapagos) (Fig. 5). Subsequent work has shown that I was wrong by more than an order of magnitude. It also became apparent that Aldabra was one of the great seabird



Figure 5. Aldabra led to something of a fixation with tortoises and turtles—not a good idea because they seem to breed in the night and soon cover the entire floor space of the house.

nesting areas of the Indian Ocean. It has the world's second largest population of frigate birds. Amazingly, the biggest concentration of these birds was directly in the flight path of the proposed airfield—in fact, a senior officer who was there told me that he would rather resign the service than attempt to land a jet through the spiraling towers of birds. There were also many species of land birds, most of them distinct at the species level and including the last flightless bird of the Indian Ocean islands. It was immediately apparent that

any military development would be an ecological disaster of the first magnitude.

Back in England I reported at some length to the Royal Society and recommended that any plans for military development be scrapped. Maurice Yonge passed my reports upwards to the council. By extraordinary good fortune, the society at that time was in good hands: the executive secretary, Sir David Martin, had been central to the society's expedition programs. The president, Lord Blackett, was at home with members of the Labour government. And the biological secretary, Sir Ashley Miles, proved a major supporter. My report was unanimously approved by the council, and the society made its views public. This triggered what came to be known as "the Aldabra affair." To cut a long story short, the society demanded the abandonment of the military plans and said it would institute a long-term proposal of research. In what must have been an unprecedented move, the officers actually got into a taxi and went to express these views personally to the minister of defence. The press and other media took the matter up, but none more fiercely than the Scottish Member of Parliament, Mr. Tam Dalyell (today, after 40 years in the Commons, the longest-serving member, the "Father of the House"). Every day he asked question after question in Parliament until he was expressly forbidden to ask any more. Politically and scientifically, Mr. Dalyell and Lord Blackett made all the difference. The government finally abandoned its proposal in November 1978 (on purely economic grounds), and the Royal Society immediately announced that it would mount a major expedition to the atoll and that moreover it would build a research station there for long-term scientific work. I was asked to organize the expedition and plan the research station. An Aldabra Research Committee was formed, under the chairmanship of Professor Stanley Westoll.

Now all this was in the mid-60s. It is an interesting part of British law that government files must be made available to the public at the Public Records Office after a 30-year period has elapsed. Over the past several years I have therefore had the opportunity to review files on this matter from all the main Departments of State and indeed from the prime minister himself. A few weeks ago I brought back 8 kilograms of photocopies of what I believe to be about one-third of the total Aldabra files. I am doing a book on these which is not simply about the ecological importance of Aldabra, but also about the ignorance and duplicity of both the government of the day and the military. One file stands out as a sustained criticism of myself. The general proposition at the outset was that Stoddart was useless to the Ministry of Defence. He is only interested in science (which is why I was there in the first place). Pages later, a leading RAF officer put the boot in: the fellow is so useless he does not even carry a bottle opener. I remember the occasion distinctly. I was collecting plants on my hands and knees at the eastern end of the atoll. A shadow looms over me and asks if I have a bottle opener. Alas, I said, I don't think I have. Of course I did (what field worker wouldn't?), but I wasn't going to give it to him. And the idiotic thing was that the reason he asked was that he didn't have one himself.

Thereafter things moved rather rapidly. The society chartered the East African Marine Fisheries research vessel *Manihine*, which must have been the least agreeable vessel available (in the lab it had a typewriter on gimbals, which never failed to make everyone who used it throw up). With Westoll and Duncan Poore (the director of the

British Nature Conservancy) I joined it in Mombasa. Then we visited Dar-es-Salam, Zanzibar, and Diego Suarez in Madagascar. Coming out of Diego Suarez was appalling. Poore and I tied ourselves to our bunks, as the ship was pitching and rolling to an impossible degree. Soon we had to undo the knots. Stanley had a cabin to himself. It was where they stowed the glass carboys of formalin. One broke loose and Stanley had clutched it as it rolled around the floor (he was lame in one leg, which didn't help). But all was well in the end, though the sea would be up above our knees on every roll as we made our way to the heads.

The skipper was a curious chap. He was obsessed with being suntanned all over, so never wore clothes (at least on board); he explained that this was in order that the ladies of the night would not giggle at his pallidity when he took his trousers off. But he took this to extremes—he even shaved his head. Then he went to his bunk and read a book. On one of these occasions, with no one on the bridge, the ship went aground in the northern Amirantes, and on a falling tide. It was round-hulled and simply went over sideways until part of the deck was awash. It seemed obvious to me that when the tide rose it would fill the ship before it rolled over upright. “Right,” I said to Poore, “let's go and do that island over there” (African Banks), and we did. Amazingly, it righted itself with no problem and we went on our way.

Manihine enabled us to look at Farquhar and Desroches (both in the military plans), together with Remire. Later, with Malcolm Coe, who headed the long-term tortoise project in Aldabra, we took in D'Arros and St. Joseph Atolls. And I took in Bird and Denis on the northern margin of the Seychelles bank. In 1967 with the Royal Navy there was Assumption, Astove, and Cosmoledo. Preliminary papers came from each of these (many in *Atoll Research Bulletin* 136). Nobody has done much on any of them since, with the notable exceptions of the birds and the turtles. But the main thing about this odyssey was that on Aldabra I selected the site for the Aldabra Research Station, devised a plan (largely adhered to), and pegged the buildings out on the ground. The station was built in 1968-69 (Fig. 6). It lasted for 30 years before needing replacement. With the generous assistance of the World Bank and other institutions, it has now been rebuilt to last at least another 30.

This is not the place to recount the sometimes bizarre stories of our stewardship of Aldabra—except for two. The first involved the cook, a woman, and a third party. The cook got into the medical store and used its contents to poison the culprit, who took a long time to die. This was in November. The police finally showed up in February. There was the somewhat alarming situation that everybody had to sit down for Christmas dinner prepared by the guilty man. Of course the victim had been put in the ground on the day he died. When the police arrived, they demanded the body. So the remains had to be exhumed, placed in a very large plastic bag, and taken out through the surf. By that time the body was so decayed that the evidence of poisoning had gone, and the man was finally released. On another occasion, one of our Seychellois laborers, a young chap, was afflicted with acute appendicitis. Something had to be done. There was a full surgical kit on the atoll but no one with any medical knowledge. Radio contact was established with the main hospital on Mahe, 600 miles away, and it was decided to proceed. He was put on the kitchen table and anaesthetized. Surgery commenced, being

The Red-Crested Stoddart Bird

Very rare species indeed: there is only one specimen (two would be too much) in existence, closely guarded by the Natural History Museum and by the Royal Society. It is also in great demand in various obscure islands in the Indian Ocean.

Believes in making its presence felt, especially when it is about to migrate. Is garrulous and rather noisy. Is easily recognizable by its curious red plumage, which is found on the top of the head, continuing down around the beak where its colour becomes more startling.

It builds highly complicated and untidy nests in minute quarters. The nests usually consist of books, maps, periodicals, corals, rocks, and other bits of junk. It is by no means the most methodical of bird architects.

The bird can be observed in flight at certain times of the year (usually from creditors, senior tutors' secretaries, eccentric ornithologists, and people demanding a reply to their letter of 1.2.62).

It is extremely fussy in its feeding habits (and refuses to look at any meal under the price of 50/.) and notorious in its drinking habits. This particular species is noted for possessing an uncanny instinct which enables it to fight for survival by discovering where others birds keep their drink. (It is interesting to note that this bird much prefers not to use up its own drink whenever possible.)

The Stoddart call is long, often punctuated by short, sharp exclamatory sounds, and very expensive. If one listens carefully, it is possible to discern an answering call from the Griffin bird. The latter's call is even longer and more frequent. The various sounds of the Stoddart bird have been recorded several times by the British Bird Club in order to give bird-lovers everywhere an opportunity of hearing this unique once-seen-never-to-be-forgotten species.

Figure 6. This notice was for many years on the bulletin board in the Aldabra office at the Royal Society. Suspected author was D.J.H. Griffin of the Royal Society's staff.

talked through with the hospital. It rapidly became apparent that the wall of the abdomen was more complex than anticipated. One of the assistants passed out at the sight and had to be revived. Finally they got through. Nobody could find the appendix—the only printed guide we had there was *The Ship Captain's Medical Handbook*, and this deals mainly with the more social diseases common to sailors. So the instruction came to sew him up again. A passing tanker was contacted and agreed to take him to Madagascar. A month or two later he walked back up the beach again on Aldabra. The next scientist due to go there from England was so shaken by this story that he made a point of having his appendix removed before he went. There are quite a number of stories such as these, but I will recount them in due course elsewhere.

While all this was going on, there was a parallel development elsewhere in the Indian Ocean. Among the military sites selected for “development” was Diego Garcia in the Chagos Archipelago. I was sent there by the Royal Society in 1967 with an American defense team to see if any objection could be made to its military development. The whole business was cloaked in secrecy to a quite absurd degree. When we were there a paragraph about the survey appeared in the *Times* of London:

when it got to Diego Garcia it was instantly taken down to the beach and burned—a security risk, except it had already been seen by every reader of the *Times* in the world. The atoll was by then simply a coconut plantation. I had to say there was no case in the terrestrial ecology to object to the military plans. It is now a major forward base for U.S. conflicts in the Middle East and Asia. I have been distressed by this ever since. I think my original assessment would be confirmed by an independent adviser. But I know perfectly well that at that time my thoughts were on safeguarding Aldabra: trying to save one, you might get away with; trying to save two, you might lose both. The Diego Garcia reports are in *Atoll Research Bulletin* 147. All of this Indian Ocean defense business took an enormous amount of my time and energy; sometimes I was in London for three consecutive days in the week.

The main results of the Aldabra work came in an entire volume (over 600 pages) of the *Philosophical Transactions* of the Royal Society (1971) and in *The Terrestrial Ecology of Aldabra* (1979). The results of a joint symposium of the Zoological Society and the Royal Society, “Regional Variation in Indian Ocean Coral Reefs,” organized with Maurice Yonge, appeared in 1971. Papers on the floristics and ecology of western Indian Ocean coral islands were published in *Atoll Research Bulletin* 273 in 1983. *Biogeography and Ecology of the Seychelles Islands* appeared in 1986.

But then there were several other things to do. The first was the Great Barrier Reef. Maurice Yonge had led the 1928–29 expedition to Low Isles and beyond, and Alfred had been on that and then went back in 1935. Both approached the University of Queensland, and it was agreed that the time was ripe for a further expedition in 1973. The timing was appropriate. The Australian Institute for Marine Science was taking off, and there was an opportunity to focus research on the reefs of Queensland. With Dick Orme at the university, we put together a program that also involved James Cook University in Townsville and the Australian National University in Canberra. This was quite a complex logistical operation, given the different interests involved, and anyone familiar with Cook’s first voyage will have some idea of the sea conditions to be expected. I mapped every island we went to and collected plants: the received wisdom at the time was that the flora of the reef islands on the Great Barrier Reef was limited to 30 or so common Indo-Pacific strand plants. This idea had come from the fact that people were more experienced with the islands of the southernmost Barrier than with those of the northern. On the northern Barrier we increased the total by an order of magnitude (*Atoll Research Bulletin* 348, 1991). The northern Barrier lagoon is much shallower than the southern, where one might think of the islands as oceanic, whereas in the north it seemed to me that the flora (which included many species not usually known from coral cays) was relict from the time when the lagoon was dry. The main results of this were published in *The Northern Great Barrier Reef* in 1978.

Then 1969 was the bicentenary of Cook’s first arrival in the Pacific. He had been sponsored by the Royal Society, and it was natural for the society to seek to mark the occasion. Through the Southern Zone Research Committee, Maurice proposed a commemorative expedition with the Royal Society of New Zealand. There were to be two field parties, one in Tonga, focusing on the high volcanoes (notably Tofua where Bligh briefly landed after the mutiny) and marine work in Vavau, and the second in the

Cooks themselves. For this I chose the almost atoll of Aitutaki, which Darwin had seen from the *Beagle*. I assembled a team from the expeditions to the Solomons and the Great Barrier Reef. At that time it was only possible to get to Rarotonga by ship and then through the reef by barge; tourism did not exist. It was idyllic, and even more so on Aitutaki. This was the first island at which the village elders would assemble on the beach to pray to God for the safe arrival of the voyagers on the other side of the lagoon. I have been back there since—there are now motels and I suspect this practice no longer exists. The girls of Teriora High School on Rarotonga put on a display of Polynesian dancing on the beach that was truly remarkable. The bishop of the Cook Islands, Reverend Bernard Thorogood, a rather large man, insisted on standing between me and them in order to discuss the economic conditions of the Cooks—I am sure it gave him some amusement. Equally memorable has been the dancing throughout French Polynesia. These make the meretricious performances for tourists in Hawaii a deep embarrassment to watch. The Aitutaki work appeared in *Atoll Research Bulletin* 190. The Royal Society had a special meeting for the Cook bicentenary in 1969. For this they erected Cook's tent, his table, and his chair at their rooms in Carlton House Terrace in London: on the table stood one of Harrison's chronometers (normally on the mantel shelf of the society's president), still keeping the perfect time that made Cook's second and third voyage so spectacular. It was extraordinarily moving to be thus reminded of the essential continuity of the scientific endeavor.

The Cook Islands proved seductive. In the following years I took a party to Mangaia, looking at raised reefs and sea-level history; then one to the likewise elevated reefs of Atiu, Mauke, and Mitiaro (see *Atoll Research Bulletin* 341); and finally to the uninhabited atoll of Suvarrow. If I had had the wit to take a metal detector to the latter I would by now be rich, given the history of the place; as it was, we were faced with a population explosion of Polynesian rats. Fortunately, they proved to be diurnal—very difficult not to share your lunch with, but at least you could sleep at night. The people involved in these studies were Terry Scoffin (from the Great Barrier Reef Expedition), former students Colin Woodroffe and Tom Spencer (Fig. 7), and Sandy Tudhope from Edinburgh. Many of the results of this work are yet to appear.



Figure 7. “Me and the Vicar”. On Rarotonga, discussing our program on Atiu, Mauke, and Mitiaro with Tom Spencer. 1985.

During the late 1970s I also became interested in the Cayman Islands. This work centered on the Mosquito Research and Control Unit of which the director was the rather formidable and highly eccentric Marco Giglioli. In fact, his whole family was rather

unusual. I went there first as a member of a team evaluating natural resources and returned repeatedly. Colin Woodroffe and Tom Spencer did their Ph.D. work there, one on the mangroves of Grand Cayman and the other on limestone morphology and erosion rates. As part of this program, I organized an expedition to Little Cayman; the results

appeared as “Geography and Ecology of Little Cayman” in *Atoll Research Bulletin* 241. I have never been in a place worse with mosquitoes. Marco insisted that my students undergo what he called biting tests: they were obliged to go into the mangroves, hold one arm out horizontally, and count the number of bites they suffered during a fixed period. Meanwhile, Marco hovered overhead in the unit’s helicopter to ensure that none of them swatted a single mosquito. He himself, after years of work there, had skin like leather and didn’t feel a thing. On Little Cayman we had a screened house and each morning the maid swept up pyramidal piles of mosquitoes on the floor. On Grand Cayman people regularly went to the market carrying a can of smouldering rags in the hope that it would keep them at bay. It was said that it was commonplace for cattle to die because their nostrils had become clogged with them. Marco had relied on malathion and they had become resistant to it. The fallback was to use trucks belching out noxious suspensions: I was once having supper in a restaurant there when one of these trucks backed up to the window and filled the place with whatever the lethal concoction was. Everyone fell to the floor under the white cloud and crawled toward the door.

In my experience, there was only one place worse in the world: Barbuda in the Lesser Antilles. I took a party there of former Great Barrier Reef people; we were dropped on the south coast by H.M.S. *Hydra*. The place was infested with sandflies. They instantly got through the mosquito nets in our tents and when bloated could not get out. We were forced to take a bottle of Scotch and lie in the sea with only nostrils above water. The same was true on Grand Cayman, where one of the largest hotels was totally enshrouded in mosquito mesh: go outside and it was a matter of conjecture whether you got down to the beach before they got you. Occasionally, I stayed at Government House. The governor was also distinctly odd. For some reason, he could not sit at table for dinner but was obliged to lie horizontally on the floor. I had not previously had experience of how to carry out a conversation in such a situation. Hugh Hefner’s black bunny jet called there frequently: there is an issue of *Playboy* carrying pictures of young women astride the stilt roots of *Rhizophora*. They must have been eaten alive. Routinely people were arrested at the airport with suitcases filled with U.S. dollar bills. At the start of the Little Cayman expedition, a botanist from Jamaica had the plane window blow out next to him at 25,000 feet. He refused to fly again.

In 1984 I proposed to the Cayman Islands government that there was a need for a book summarizing what was known scientifically about the islands, and they agreed. I put together a list of chapters, approached all the prospective authors, secured their manuscripts, and found a publisher. It became a very substantial volume. Unfortunately, toward the end I had become quite unwell and had to hand it over to others. When *The Cayman Islands: Natural History and Biogeography* appeared in 1994, my name was not on the title page, though in the introduction my role was made explicit by those who were obliged to take it over. I regret that after years of toil this book is not formally on my curriculum vitae.

Finally, the Phoenix Islands in the central equatorial Pacific. These are some of the most desolate reef islands in the world, but with extraordinary seabird populations (including the world’s largest population of frigate birds: because of the aridity, there are no trees except on two southern atolls and the normally arboreal frigates are necessarily

ground-nesting, sharing territory with several species of boobies). The individual islands were used fairly roughly in the later nineteenth century by whalers and especially guano diggers. In the 1940s there was a doomed project to settle Gilbertese from the overpopulated islands of what is now Kiribati, but because of the aridity these proved unsustainable and had to be abandoned. Canton Atoll became a major forward base in World War II and this was maintained by the Americans into the 1970s. There has, however, been a long dispute over sovereignty between Britain and the United States, and hence at Canton there were two settlements, one British, one American. This arrangement had the bonus that each side maintained meteorological stations in fairly close proximity. It is highly unusual on atolls to have this kind of duplication. The reason for our interest was that the Phoenix Islands had been chosen as the reception area for intercontinental ballistic missiles launched from Vandenberg Air Force Base in California. There was clear concern about the impact of this program on the terrestrial ecology of the islands. This was headed in Washington by the Smithsonian Institution and in England (because of the sovereignty dispute) by the Royal Society. So it was that Ray Fosberg and I were asked to go there and report. We went twice, in 1973 and 1975 (an exceptionally hectic part of a too hectic life). On the first expedition we were joined by Roger Clapp of the Fish and Wildlife Service. He is the most dedicated ornithologist I have ever met. He had been with the Smithsonian's Pacific Ocean Biological Survey program, which resulted in the most massive issues of the *Atoll Research Bulletin* on the birds and other terrestrial biota of many exceedingly remote islands and groups. The downside was that his body was continuously infested with bird lice. I rather drew the line at that level of commitment, but it never seemed to bother him at all.

These surveys were only made possible by the astonishing cooperation of the U.S. Air Force, which had an array of gigantic helicopters on Canton. The individual islands of the group are so far apart that fuel dumps were needed on each one: these were swung into place in large plastic bags suspended below the helicopter. Then they dropped us and flew away. Now these are islands on or close to the equator, and on most of them in a normal year there was no vegetation higher than about 20 centimeters and hence no shade. It was quite a shock to the system to be on an island like Birnie within 48 hours of leaving England. What was especially interesting was that because of their location the Phoenix are periodically affected by the El Niño phenomenon during which heavy rains fall on otherwise desert islands. We had the good fortune to be there in a "normal" year and an El Niño year. The latter are known through the equatorial Pacific as occasions of catastrophic seabird mortality. The reason for this has always been ascribed to suppression of upwelling and hence in marine productivity and food. On the islands it was apparent that ground-nesting and especially heavy birds such as boobies had grave difficulty in becoming airborne once the vegetation reached a meter in height.

On some islands the Air Force had trailers occupied by rather lonely men who appeared to have very little to read except *Playboy*. The idea was to track where between the islands the ICBMs fell. But by the time of the second visit they were all falling in the Canton lagoon; it was rather unnerving to be told at supper that "two came in this afternoon." But the need evaporated and the Air Force pulled out, leaving these

spectacular islands unpopulated and unpoliced: the huge trailers were simply lifted from each island and dropped in the sea. Several of them have East Asian fishing vessels stranded on the reef, clearly for insurance purposes. On Hull, Ray and I came to the beach crest on one occasion to find a trawler heading directly to the reef. They must have been astonished to see us since they made a right-angle turn and fled. It was on that atoll too that having been put on the beach by helicopter we were met over the beach crest by the unmistakable smell of death. Sure enough, there was a very recent shallow grave. One wondered slightly what was going on as the shadows lengthened and the evening came. The first thing that happens when these ships go on the reef is that they string a line to the shore and the crew are then rapidly followed by the rats, which is not a good idea on great seabird islands. The plants of the Phoenix have been published in the *Bulletin*, but most of the reports are now in near-final form (a substantial amount has been plagiarized, having been given in confidence to a character who has now sunk from sight).

Another incident at this time was distinctly odd. I had a message from the Foreign Office that a Soviet research vessel wanted to look at atolls under British jurisdiction in the western Pacific. They were disinclined to agree unless there was someone from Britain to keep an eye on things. Would I go? Never one to turn down the opportunity to see a few more atolls, I at once agreed. It was fixed that I would join the ship in Madang, New Guinea. The day before I was due to fly to Sydney; I had another call, this time from the Ministry of Defence. You are leaving New Guinea tomorrow to join a Russian ship, said the voice. I could only agree. Then the voice said, "You will be met in the departure lounge at Heathrow by a man in a raincoat carrying the *Daily Telegraph*." I thanked my caller and the line went dead. Off I went to Heathrow and into the departure lounge. Sure enough the door opens (the chap must have had some means of getting through officialdom) and a fellow in a rather grubby raincoat and indeed carrying a newspaper made straight for me: he obviously knew who I was. "Look," he said, "I'm sorry I'm a bit late but we've just had an office party and I'm rather drunk." Then he said, "We can't talk here in the middle of the lounge; we must get our backs against the wall." So we walked backwards to the nearest wall. Then he started unbuttoning his raincoat and knowing the general sexual proclivities of members of British Intelligence I wondered what was coming next. He produced a series of telephoto images of the ship I was to join. "Here," he said, "we want to know what this antenna does and where it goes in the ship." I had a vision of myself crawling round in the bowels of the ship at two in the morning, feeling my way along cables. I had little doubt that the outcome would be that my headless body would be found floating in some mangrove swamp. Finally he left and I called my wife. She instructed me to come home at once and wash my hands of the whole matter. But no, let me see what is going on.

So off I went to Sydney and thence to Madang. "Take me to the Russian ship." I said to the taxi driver. "Russian ship? Russian ship? There is no Russian ship around here." I was obliged to check in to the Madang Hotel. I was there for a month before the ship was found—sailing westward in the Indian Ocean. The ship was called the *Academician Bogorov*, and though it sounds trite to say so, it had simply bogoroved. I

had called the Royal Society for sustenance to support immediate cash-flow problems, and there was in Madang a branch of my own British bank and I got to know the manager slightly. One night I was rudely awakened in the early hours, seized a towel and there at the door was the bank manager with several rather burly locals. “Right,” says he, “where is she?” He evidently had designs on one of the ladies on the front desk and was convinced that I had out-manuevered him. “My dear chap,” I said, “do please come in.” Eventually they trooped away. But when the Royal Society money did not come I was obliged to go to see him in his office and ask him to cash a personal check on a bank on the other side of the world so I could actually pay the hotel bill and get away. He was evidently a gentleman and did so. Back in Sydney I bought a copy of the *London Times*. In it was the obituary of the academician who had invited me on the trip in the first place. I sent a message to the *Bogorov* saying bon voyage. It was returned a month later from Port Said saying the ship could not be contacted. On the good side, I got a paper in *Nature* out of the stay in Madang. On the downside my wife and both children got chicken pox while I was in New Guinea and were not pleased at my absence. What transpired with the lady in Madang I do not know.

Before my gentle readers conclude that I seem to have finally lost it, there is an equally bizarre follow-up. In 1979 the Pacific Science Congress was held in Khabarovsk in the Soviet Far East; I was the British delegate to Council representing the Royal Society. By some means, which I cannot now remember, I had a message from a Russian reef worker (who shall obviously remain nameless) saying that his wife particularly enjoyed *Playgirl* magazine—would I bring some copies? I should say that all kinds of distinguished people were at that time being asked to step to one side at Moscow airport to explain the contents of their brief cases. My wife said, “You must be a total idiot if you do this,” but then I thought that I would trust any reef worker implicitly as members of a common brotherhood. I had not looked at that particular journal before but laid hands on some copies and took off for Moscow. All was well. I met people there whom I had known for years and took off for Khabarovsk. I had not been in my room too long before there was a knock at the door. My friend came in, motioned me to say nothing, then went into the bathroom and turned the shower on. “This is not a safe room,” he whispered; “we must go outside and walk around.” (Parenthetically I have to swear to God that all these stories are absolutely true.) So we did, and I handed him the magazines in a brown paper bag. It is easy to see how people like Philby, Burgess, and Maclean got away with what they did. I saw something of him over the next few days, before going off to the Sikhotin Alin, where amazingly I saw the spoor of a Siberian Tiger (I had heard Bengal Tiger in the Sundarbans in Bangladesh but never seen them—indeed, I thought the armed Bangladeshis the greater danger to my personal safety). He said to me, “You must come to Vladivostok—it is the most beautiful city.” I pointed out that it was a city closed to foreigners. “No problem,” he said, “we will put you in the car trunk.” Even I began to realize at that point that things were getting slightly out of control, and I declined.

But in the early 1980s Vladivostok was opened up. I was at that time president of the Pacific Division of the American Association for the Advancement of Science and had to go to Vladivostok to open up contacts with the Academy of Sciences of the

USSR. Foolishly I did this in January, which is not a good time of the year to visit that part of the world. The statue of Lenin, draped with yard-long icicles still stood outside the Khabarovsk Hotel. Khabarovsk airport was filled with mothballed ships they could not afford to keep afloat. It was there to my amazement that I finally caught up with the *Bogorov*. I wandered round the ship and took pictures of its lifeboats, which still proudly proclaimed the name of the academician. So that was closure on my first New Guinea adventure.

Let me turn for my next reminiscence to the southern Bahamas. In 1985 I was approached by Lloyds of London, the insurance brokers, about the sinking of a huge tanker taking a very unusual route from the Gulf of Mexico into the Atlantic. It had allegedly gone aground at or near East Plana Key, managed to free itself, and then sank in 4 kilometers of water. It was photographed by someone in a light airplane as it sank, and though it had half a dozen compartments it went down horizontally, suggesting that all compartments were equally holed (or that all the cocks had been opened in each). The point was that if such a huge ship had indeed gone onto a reef the signs must be rather obvious. Would I look into it? Well, why not? I was at that time a Regents Fellow at the Smithsonian, and from the days of Tom Goreau I had kept in touch with the University of the West Indies in Jamaica. Indeed, I had been there that same year to look at the Pedro, Port Royal, and Morant Cays, using their newly refurbished research vessel, *Caranx*. This was a 60-foot converted trawler. The Department of Zoology housed a former Cambridge Ph.D. student, Stephen Head, who had wide experience in the Red Sea. He agreed to come along on a reconnaissance. We flew to Acklins Island, the closest to uninhabited East Plana Cay. I told people that we wanted someone to take us there. The locals seemed extremely wary, perhaps because of drugs. Finally, I found someone with a small boat and outboard who said he could do it. Start early, there by lunchtime. A thousand dollars cash, payable in advance, which I did. He would look after all the logistics, like eating. Start at dawn. The fellow could not be found until almost noon, and then we started off. It was late afternoon that I noticed a trace of land far behind us. It was East Plana Key: we were in the open Atlantic heading for Senegal. Finally we got ashore. Time for food. "Right," says he, "I'll get the supper if you get some edible molluscs off the beachrock." His logistics amounted to building a fire with driftwood. Next day we looked at the reefs, which were vast. We saw no sign of the tanker, but Stephen did locate another older wreck, from the plates of which he fashioned me a rusting parrot fish.

I put a proposal to Lloyds to do an intensive survey of the area using *Caranx* and a team from Jamaica. It was agreed the ship and I (plus a senior Lloyds marine man) would meet on Great Inagua. The Lloyds chap and I had to wait for the ship and had a very good lunch. Finally we all made contact and went off to East Plana Key in rather rough weather. The plan was to start the next day doing echo-sounding traverses with *Caranx*, with positions being fixed from shore stations. I called it a day in late afternoon, having worked with Malcolm Hendry plotting the ship's track. We went back to our original anchorage and awaited the ship. It never came. When it was finally pitch dark (no moon) we saw stationary lights on the horizon. Evidently what had happened was that once the survey was complete everyone congregated aft and had a beer, other

than the skipper, a Jamaican, who manned the bridge. He put the vessel on autopilot and proceeded in a straight line. Someone said, "Let's have another beer before that bugger Stoddart gets back" (I quote this absolutely), when suddenly there was an almighty crash and the ship was stranded on the reef. This was on the windward face of the Bahamas and huge swells were surging across the reef.

They begged for rescue. We had only a Zodiac and did not know the area at all, except for our anchoring the previous night. The only thing to do was to get out of the lagoon we had entered and go down in the open sea to investigate. Hendry and I got outside through the passage by which we had entered and proceeded eastwards. There was absolutely no moon and we could locate the reef only by the phosphorescence of the breakers. Finally we got abreast of the ship. It was being punched across the reef by every breaker, and those on board were expecting it to capsize. I refused to go across an unknown windward reef at night in a Zodiac. "No," I said, "we'll come to you through the lagoon." So we turned about in the Zodiac and made our way by the phosphorescence along the windward reef. Simply by dead reckoning I judged the point to turn into the lagoon, and it worked. Happily, it was the highest springs for about 19 years and we could ignore all the patch reefs in the lagoon. Soon we were back at *Caranx*. The swells were 2 meters or more and I was afraid we would be caught under the bow. The ship visibly moved with every wave. There were more than a dozen people on board and some were frozen by fright: their fingers had to be prized from the rails before they could jump into the Zodiac. It took half a dozen runs to the beach a couple of kilometers away to get them and their gear ashore. On the last trip I shouted to Stephen Head, the last on board, and said get a case of rum. I still have a bottle of Appleton Estate Special that my children are instructed to force between my lips as the end grows nigh (they have a problem: when I left Cambridge for Berkeley my colleagues gave me a bottle of port of the same vintage as myself, so they have decisions to make).

We finally got everyone on shore and people tried to sleep. It then became apparent that East Plana Cay was the last refuge in the entire Bahamas for a relict Pleistocene species of *Geocapromys*, which turned out to be nocturnal. Huge animals were soon climbing over everyone as they foraged along the strand line. It is a Red Databook species and I foresaw crises of conscience if we were forced to eat the things.

Mayday calls elicited no response until finally a Norwegian tanker came through. We continued working Mayday (though the ship was full of seawater, miraculously the radios had survived) and finally over the horizon came the U.S. Navy. I was on the ship at the time. It was in a lamentable state—everything was new after its recent refit and all a total write-off. In came a Rigid Hull Inflatable manned by troops with carbines, all at attention, and the captain in the stern. He came on board. I said, "Good afternoon, captain," or words to that effect. He said, "Stand to one side," whereupon they swarmed on board and started tearing the panels off the cabin walls, doubtless looking for substances. They soon realized we were just inept scientists. In resigned tones, he said "O.K., I'll take you to the nearest airport, an overnight trip to Great Exuma. I have 14 people on the beach." I said we could take them too and that I could guide them through the reefs. With some acerbity he replied that would not be

necessary. We got there before him, and as he walked up the beach I came down to him and asked whether he would like a gin and tonic—with ice. It was one of the high points of my life, how to be shipwrecked in style. Well they got everyone on board, and only he was left. What about that gin he says. We had three cases—all emergency supplies of course—of whiskey, rum, vodka, and gin. When we finally got out to the ship, I could hardly get up the rope ladder for the clinking weight of my backpack.

We arrived at Great Exuma on a Good Friday. All the hotels were full and anyway we could not afford such prices. Stephen called Jamaica to tell them the sad news. Then he called Miami airport and chartered a jet to take everyone back to Kingston. The chap from Lloyds and I waved them farewell and then took off for Fort Lauderdale. The people making the insurance claims on the tanker had a field day, knowing that we were trying to show that the reefs in the area of the alleged grounding were more than 5 meters deep, and we managed to go aground on reefs virtually at sea level. Indeed, they lost no time in sending a group to East Plana Key to document the debacle. By that time the locals had stripped the wreck of anything of the least value. The case came to trial in the High Courts of Justice in London. I had a couple of weeks sitting in my suit, shirt, and tie and was never called upon to speak, only to listen to encomiums about my incompetence (which was in fact that of employees of the University of the West Indies). The lunches near the High Court were excellent. The case was finally settled out of court as the plaintiffs' case collapsed, and I found it all rather profitable. As did Lloyds, who saved something like 5 million pounds.

One further anecdote will suffice. Soon after coming to Berkeley I saw the opportunity of extending departmental interests in the Pacific. I had already done a great deal in the Cooks, but these islands were now so much more accessible than the Indian Ocean. I got a substantial grant for work on paleoclimates based on coral evidence. Thinking of good places to go where I hadn't been before, it struck me that Bali might be rather pleasant. I had only been in Indonesia once before, on a UNESCO workshop. There I was accompanied everywhere by Indonesian scientists from the Institute of Oceanography in Jakarta. It was a great delight to reexamine the islands surveyed by Umbgrove and Verstappen decades before. The reefs themselves were a disaster. The slime of the pollution in Jakarta Bay was impossible to get off your skin; the reefs were dead. With no sediment supply, the islands were eroding to the point of total disappearance. But I imagined that Bali would be different. I recruited graduate students and a new faculty member and finally flew there. I arrived in Bali at two in the morning, together with the contents of several other jumbo jets. The students were there to meet me, and my new faculty colleague was traveling with me. She got through immigration sooner than I did. I got to the desk, the passport was stamped, and then someone shouted, "That's him!" I was suddenly surrounded by two dozen troops with rifles. They already had my suitcase. We set off rather briskly down the hall to what I imagined would be the hospitality lounge. Then I saw a sign saying "Departures." The lounge was full but we went straight through the departure gate. There was an armored vehicle into which I was bundled. It then took off across the airport to a jet with its engines running. I had been deported to Guam. No reason was given other than this was a direct instruction from Jakarta (evidently all visitors to Indonesia have their names and

passport details sent ahead to Jakarta before the plane lands). It so happened that I knew the British ambassador in Jakarta from previous discussions in London about the Pitcairn Islands, but at two in the morning that was not much use, and obviously I would not be let loose near a telephone. So having left San Francisco on a Thursday afternoon I landed back again on the Saturday evening. This caused me some annoyance. The British Foreign Office was explicit that whether or not a state had the right to admit people was entirely a matter for them. After endless badgering I learned that I had been deported as “a threat to state security.” It is a sadness to me that the Indonesian scientists I wrote to asking for an explanation—some I had known for two decades—never replied to my letters. I knew that this kind of thing had happened to American scientists. Hence I spoke against the proposal to hold the last International Coral Reef Symposium in Bali. Somehow and at the last minute, the Bali symposium was pulled off, but not without its considerable embarrassment. It remains my view that no one in science or out of it should lend legitimacy to such government actions.

This is, I think, enough about my fieldwork in the reef seas: there is a great deal more to tell, but I shall do that elsewhere. It has taken me to the tropics up to three times a year for some 40 years. But I early realized that there were occasions when it was difficult to go there. And since I am trying to demonstrate that behind it all was some grand strategy, let me say that I ran a parallel program on the morphology and hydrodynamics of salt marshes, using the input of perhaps hundreds of undergraduates over the years. This was highly labor intensive and indeed rather unpleasant work. It turned out that the best tides for recording crucial data were in the middle of the night in December. Often we worked on the Norfolk coast when the seawater on the marsh surface had frozen into sheets of ice. There were memorable occasions through the years of this work. As on the northern Great Barrier Reef, these were macrotidal marshes and thus really rather tricky to handle. There was a rather dire occasion when I took dozens of students to the marsh and unfortunately checked the tide tables for the wrong year. By the time I realized this it was too late: the tide was already flooding in at something like a meter a second and the channel width was about 300 meters. There was nothing for it but to go for it. Students did not seem to understand the urgency of the situation until they saw the smaller lighter women unable to keep their feet on the ground and starting to be swept away. It once happened to my wife, and I could do nothing more than observe the situation through binoculars. She survived. There was another situation when I deployed everyone across the marsh to measure velocities and then had to go to Edinburgh to collect a medal for fieldwork. As I was doing this, there was a storm surge through the North Sea, which means that the tide floods in and doesn't ebb for several hours. People were obliged to stand at their stations up to their armpits in frigid water for hours. It is impossible to move on the marsh during an over-bankful tide because you cannot see where the major channels are. To my amazement, no one ever actually died on these occasions. More to the point, the work resulted in a string of papers that changed the way that marsh morphodynamics are understood, and they led to a number of Ph.D. dissertations, often by students who then went to do the same kinds of thing in mangrove swamps and on coral reefs.

My other back-up, as it were, against the day when active fieldwork on the reefs

would draw to a close, was to look at history. I had the very good fortune to be a graduate student at a time when the vast archive of the papers of Charles Darwin became accessible in the University Library at Cambridge. Indeed, I think I may have been one of the first to get into them. There I found the first statement of Darwin's theory of the development of atolls, which he wrote on board the *Beagle* between Tahiti and New Zealand, and which I transcribed and published in the *Bulletin* in 1962. Thereafter I pursued Darwin and had several more papers, including some of my favorites. It also led me to archival work on Huxley, Wilkes, Jukes, Agassiz, and others, on most of which I have published. Some small part of this appears in my book *On Geography and Its History*, but there was more to come: I have since published a number of papers on these historical topics.

Perhaps I should also mention that my childhood interest in Tibet led to a major commitment in China after that country became more open following the death of Chairman Mao. I was at that time a member of the British National Committee for Geography, and I suggested to it that the time was ripe for an interaction between British and Chinese geographers. The committee set up a subcommittee to discuss the matter, of which I was a member. To no one's great surprise, the subcommittee recommended that a delegation should go to China to begin the process, and it also recommended that the delegation comprise all the members of the subcommittee. So I arrived in Beijing in 1977 on the first of many visits. Apart from traveling widely throughout the country, I also taught later at Nanjing University, and the only thing approaching a textbook that I have ever written derived from one of those courses (*Hai an yu hien pien hua* [Geomorphology of low coasts], 1982). This Chinese work introduced me to a quite extraordinary group of Chinese scientists, most of whom at one time or another came to our home. It also brought me into contact with one of the greatest scholars of the twentieth century, Joseph Needham. My wife and I were quite amazed to be invited to a small dinner party held in Gonville and Caius College (of which he had been Master) to celebrate his eightieth birthday: a group came from Beijing especially for the event. It also brought us into contact with another extraordinary person—Noel Odell, the last person to see Mallory and Irvine on the north face of Everest before they disappeared. I could write a lot about this China experience and the people I came to know, but it has nothing to do with coral reefs. I do remember with great nostalgia, however, a cruise down the Yangtze when my Chinese friends started to sing Christmas carols in English, and it soon became apparent that they knew the words better than I did.

I want now to say something about the formal institutional face of coral reef science, which I have had the good fortune to have been able to assist in creating. I do so because I know that all the most active members of the profession have been born after the events that created it. When I came into coral reef studies 40 years ago, the subject was strictly delineated. There were the *Papers* of the Tortugas Laboratory of the Carnegie Institution, the *Studies* of the Palau Tropical Biological Station, the work of Gardiner and the Percy Sladen Expeditions in the Indian Ocean, that of Sewell on the John Murray Expedition, Kuenen on the *Snellius* Expedition, and the Funafuti report in 1904. In the background was the work of Vaughan and Davis. There were the reports of

the drilling on Bikini and Enewetak. Fairbridge weighed in with his massive paper on the Great Barrier Reef in 1950, and his 1961 paper on sea-level change, which really set the cat among the pigeons. There were the reports of the Pacific Science Board expeditions in the Pacific to Arno, Onotoa, Raroia, Kapingamarangi, and Ifaluk, many of which appeared in the *Bulletin*. And, overarching everything, were the reports of the Great Barrier Reef Expedition of 1918–29 under Maurice Yonge. I have never ceased to be amazed at what the members of that expedition did, given that none of them had ever seen a reef before. The reports on the nuclear test sites in the Marshalls were becoming available. But the point is that, first, the literature was readily comprehensible, and second, that these were clearly going to be extremely exciting times for anyone in that field. It was in that context that I did my earlier fieldwork and then did a summary paper in *Biological Reviews* in 1969 (“Ecology and Morphology of Recent Coral Reefs”); a future Darwin Medallist told me at the time that it had got him through his Ph.D. I doubt if it could be done in so small a compass today. Oddly enough, I find copies of it that I gave away when it appeared priced in antiquarian book catalogs at up to \$90. It is good to know that one’s lifework still has value.

In 1967 Maurice Yonge received a letter from S. Jones, director of the Central Marine Fisheries Research Institute at Mandapam Camp in south India, inviting him to a symposium on coral reefs to be held in January 1969, under the auspices of the Marine Biological Association of India. Maurice did not feel that he could go and suggested that I go in his place. Since I had to be in New Delhi anyway in December 1968 it seemed sensible to stay over and go to the Mandapam meeting. I had a letter from Jones in October 1967, responded, and rather to my surprise was at once appointed chairman of the Advisory Committee. This led to a close association with the meeting convener, C. S. Gopinadha Pillai, whose thesis had been on the corals of India. So before Christmas in 1968 I flew to Madras and then took the train to Mandapam. Christmas itself was quieter than usual, though I had had the foresight on arrival in India to procure a card authorizing me to buy alcoholic liquor in that prohibitionist country; this allowed some kindling of the festive spirit, albeit solitarily.

The meeting began on 12 January and lasted five days. Of the 72 attendees, only 24 were from overseas. But the latter were a very interesting group—people who were or were shortly to become household names in reef science and only a few of whom had previously met others. It is worth listing their names: Gerald Bakus (United States), Werner Barthel (West Germany), C. J. Bayne (United States), J. P. Chevalier (France), Michel Denizot (France), Ray Fosberg (United States), Peter Glynn (United States), Bob Johannes (United States), Ernst Kirsteuer (United States), Lawrence McCloskey (United States), Hans Mergner (West Germany), Len Muscatine (United States), Michel Pichon (France), Y. B. Plessis (France), Klaus Rützler (United States), Bernard Salvat (France), Georg Scheer (with Anneliese) (West Germany), Raoul Serene (Singapore), S. Sukarno (Indonesia), Frank Talbot (Australia), Maria Vannucci (Brazil), and myself. The group seemed to develop a new dynamic when the Scheers arrived later, having been exploring temples, and in a taxi clearly on the point of expiry. They leapt from the vehicle and immediately started dancing. This year Georg had his ninetieth birthday, but sadly Anneliese died a few weeks later. Georg had been among the first underwater reef

scientists, working with Hans Hass. It is remarkable, considering the small number of people involved, how many countries were represented. It took a little time after the meeting, but at last a very substantial volume of proceedings appeared—it must now be one of the scarcest books in the history of coral reefs.

Apart from the formal paper sessions, there were wide-ranging evening discussions and a final plenary session. I had drafted five recommendations resulting from the symposium, and these were unanimously adopted. The most important of these was the fifth: “That in view of the success of the Mandapam Camp International Symposium on Corals and Coral Reefs, such meetings should be held at intervals of three years at centers of reef studies.” A Continuing Committee on International Reef Symposia was elected with myself as chairman; its members included Michel Pichon and Frank Talbot, and anyone who could provide input. There were two other developments from this meeting. One was the symposium I organized in May 1970 under the aegis of the Royal Society and the Zoological Society of London on Regional Variation in Indian Ocean Coral Reefs, at which many of the Mandapam people (including Dr. Pillai) came together again; this was published in 1971. The second was a volume sponsored by UNESCO, *Coral Reef Research Methods*, edited by myself and Bob Johannes, which was published in 1978. This is still quite widely cited but is in urgent need of revision.

There then ensued a two-pronged line of development. First, our Continuing Committee had no formal status in the scientific world: it had a purely ad hoc origin and to be effective had somehow to be linked with entities having a formal connection with the International Council of Scientific Unions (ICSU). My first thought was the Scientific Committee on Oceanic Research (SCOR), of which George Hemmen was secretary. SCOR was meeting in Oban, Scotland, in 1970, and I went there to make the case for the Continuing Committee to be a SCOR subcommittee. Either because of or in spite of the diversity of Highland malts available at the meeting, SCOR did not take the bait: oceanography was a blue-water operation, and they did not want to get involved in coastal shallows. George and I consulted on where to go then. The International Association of Biological Oceanography (IABO) was meeting in Kiel at the end of March 1971. There was a small problem with this: we were expecting our second child at that time. My excellent wife understood the gravity of the situation absolutely: it could be make or break for the Continuing Committee. She did her valiant best. I was present at the birth on the morning of 28 March; I landed in Hamburg that same afternoon and was in Kiel by the evening. George Hemmen again showed his magic. The next day he brought the chairman of IABO, Professor Hempel, out from one of the sessions. The three of us conferred in the rather chill sunshine, and the deal was done. Thus was born the IABO Coral Reef Committee as well as our son. (Parenthetically, I might say that having named our daughter Aldabra, I had in mind that the son might be named Diego Garcia. It was pointed out to me with some force that if that happened he would never be able even to cash a check. So he is simply Michael—just as well given the gigantic military development Diego Garcia has become and the uses to which it has been put). The IABO Committee was vital to establishing the international credentials of the coral reef community. I chaired it for several years, and then it was taken over by

Bernard Salvat, who certainly realized its immense significance.

The immediate outcome of this was that it was now possible to pursue the idea of further international symposia with some authority. At Mandapam I had discussed the possibility of the Great Barrier Reef with Frank Talbot, who was then director of the Australian Museum in Sydney. The Great Barrier Reef Committee in Brisbane had recently taken on a new lease of life. I found it necessary to go to Australia on several occasions in connection with the detailed planning of the Royal Society and Universities of Queensland Expedition to the northern Great Barrier Reef, which occupied the second half of 1973. The University of Queensland at Brisbane was a leader in this, and its representative, Dick Orme, also came to Cambridge. A specific invitation was made for the Australians to host what was now being called the Second International Coral Reef Symposium. They agreed to do so, and in no half-hearted manner. They proposed to charter a cruise liner and sail from Brisbane north to Lizard Island and back, thus giving members the chance to see new reefs and islands every day. I was rather staggered by this because the financial implications were alarming. Such liners had to be chartered long in advance, and in this case before anyone had been asked for registration fees. My wife was alarmed that we might lose the house because of it. But the university was behind it and a substantial donation was received. Great credit must go to David Montgomery, long-time secretary of the Western Society of Naturalists (WSN) as well as a teacher at California Polytechnic State University at San Luis Obispo. At the outset he was able to guarantee a block booking of WSN members. David was himself an indefatigable meeting organizer, and many will remember his meetings on Indo-Pacific marine biology, based on Guam, and on marine biology and evolution in the Pacific, in New Zealand, and the really extraordinary pan-Pacific field trips (Fig. 8) he built into them. Sadly, he died in 1993.

The Great Barrier Reef Symposium was an amazing success; it was also astonishing that no one was drowned as hundreds of people leapt into the sea every day. On one occasion while Dick Orme was at our house in Cambridge I sent a letter asking Prince Philip if he would lend his name to the venture; his reply opens volume 1 of the proceedings. It was also remarkable to have on board two members of the 1928–29 Expedition: Sir Maurice Yonge and Alfred Steers. After that the International Symposia developed their own momentum. Bob Ginsburg organized the third in Miami in 1977, Ed Gomez the fourth in Manila in 1981, Bernard Salvat the fifth (Fig. 9) in Tahiti in 1985 (sadly the last to be attended by Ray Fosberg and Marie-Hélène Sacht), and these were followed by Townsville, Australia, in 1988, Guam in 1992, Panama in 1996, and Bali in 2000. The Panama meeting (the last that has published its proceedings) attracted nearly 350 papers. Mandapam now seems a very long time ago. But great credit should be given to the fundamental role played by S. Jones and Gopinadha Pillai for starting the ball rolling over 30 years ago.

The IABO Coral Reef Committee was instrumental in getting this series of meetings off the ground, but more needed to be done to structure coral reef science. During the 1970s a small group of British reef scientists met regularly at Churchill College, Cambridge, where I was a Fellow, to review how reef science could be encouraged. We convened a much larger meeting, also at Churchill, in 1979; it was



Figure 8. A seminar on one of Dave Montgomery's field trips in Fiji.



Figure 9. Dancing at the final dinner party of the Fifth International Coral Reef Congress in Tahiti, 1985.

attended by a number of people from Europe. Barbara Brown at the University of Newcastle-on-Tyne was a major driving force behind these developments (Fig. 10). We had the advantage too that her husband Richard Dunne was a naval lawyer with experience in drafting legal documents. A second meeting was held in Cambridge in December 1980, and at this the International Society for Reef Studies was founded. This

in the long term made the IABO Committee redundant, though both coexisted until 1996. I was the first president of ISRS.

The society had two main initiatives. One was to hold annual meetings at mainly European locations. After Cambridge, there was one in 1981 at York, in 1982 at Leiden, in 1983 at Juan Les Pins, in 1984 in Miami, in 1986 at Marburg, in 1989 at Marseille, in 1990 in Noumea, French Caledonia, in 1991 at Berkeley, California (I had moved there from Cambridge in 1988), in 1993 in Luxembourg, in 1994 in Austria, and in 1995 at Newcastle-on-Tyne. Of course, 1996 was the year of the Panama Symposium (at which my wallet was stolen, including all credit cards and the Green Card. Try entering



Figure 10. Engaged in field work with Barbara Brown, one of the founders of the International Society for Reef Studies.

the United States without that critical documentation. I called my wife and asked her to block all the credit cards. She attempted to do so, but a number of credit card operators

refused to accept her instructions: apparently it is a common trick in North America for disgruntled wives to call the company as soon as the husband is out of the door and cancel all his cards. So at vast expense I had to do it myself from Panama. One continuously learns life's strategies.)

The second initiative was to have a journal devoted to coral reef science. Frank Talbot started this process after discussions with Konrad Springer before the Miami meeting in 1977. It was felt at that time that the idea was somewhat premature, but by the time of Manila in 1981, the council felt more confident. Springer was again approached and agreed to publish a journal under the title of *Coral Reefs*, under what I thought were extraordinarily generous terms. They knew very well that it takes time to establish an interdisciplinary journal and were prepared to take a loss for the first decade. It began slowly: it constantly amazed me that ISRS members continued to give their papers to journals in their own special field rather than to *Coral Reefs*. It was also a major task to get such a journal going. Volume 1, number 1 appeared in 1982. I served as coordinating editor for a decade until Richard W. Grigg took over in 1992. Those early years were only made possible by the extraordinary efforts made by the subject editors and reviewers. It has taken 20 years, but *Coral Reefs* is now established and thriving. Its papers come primarily from the United States and Australia, and then a long way behind from Britain and France.

The purpose of *Coral Reefs* was different from that of the *Atoll Research Bulletin*. The latter had always been a journal of record—descriptive of reefs and cataloguing their plants and animals. This purpose it has served for 50 years, during which it has published almost 20,000 pages in 500 numbers. Ray Fosberg invited me to join the editorial board in 1969 and subsequently Ian Macintyre (1974), who is now the editor-in-chief. Many of my longer papers were published in it. *Coral Reefs* was to be more process oriented, quantitative, and experimental. It is amazing to see it so succeed. The other ISRS publication, which has likewise flourished, is *Reef Encounter*, the newsletter. I was at first not particularly enthusiastic about this, knowing the level of commitment required. And there were at that time other reef newsletters in circulation. Indeed in its early years its appearance has been termed sporadic. But now it is one of the most interesting newsletters there is.

I think I have now said enough. I am fully aware that coral reef science has moved on spectacularly since those early years. The International Coral Reef Initiative in 1995, the International Year of the Reef in 1997 (which Bob Ginsburg did so much to promote), and the Global Coral Reef Monitoring Network (with Clive Wilkinson as a major promoter) have truly transformed and internationalized the way that coral reef studies are done. I have no doubt that had I not been around, the International Coral Reef Symposia, the International Society for Reef Studies, and *Coral Reefs* would all have come to pass. Likewise, the *Atoll Research Bulletin* was thriving before I came along and will be long after I have gone. I simply happened to be around at a particularly fruitful time to make things happen, and also the testosterone was flowing freely over the years. It has been my privilege to have known some of the great spirits of coral reef science: Maurice Yonge, Frederick Russell, and Alfred Steers from the Great Barrier Reef Expedition; Seymour Sewell of the John Murray Expedition; Harry

Ladd and Josh Tracey from the drilling operations; Tom Goreau in Jamaica—the great names from the past.

I want, however, to enter one caveat. In 1960, in Belize, I was told by two elderly American doctors on South Water Cay that I faced a dismal future (given my coloring) if I continued with a life of cavorting on tropical beaches. Of course I disregarded them. And of course they were right. Ten years ago, I had my first substantial surgery for skin cancer, and this has continued every year ever since (Fig. 11). It is the case that at that time the lotions available were purely aesthetic; there were



Figure 11. Advice to those entering coral-reef studies: always, but **always**, wear a hat. Four surgical portraits out of many from the last decade.

no such things as sun protection factors. That is no longer the case. The fact is that once it begins it is too late to do much about it. Three years ago I had a virtual world record aggressive tumor on the top of my head. It took skin grafts and seven months of daily dressings to heal. We got fed up with it—I went to Harvard to work on the Agassiz archives and then we went to retrace Humboldt's steps in highland Ecuador (even staying at haciendas where the great man himself had stayed, as indeed had La Condamine). But it was all rather tedious. On the day I finally abandoned

the daily dressings we went down to the store to buy a chicken. It was customer appreciation week in that particular chain, when the staff were supposed to engage in meaningful conversation with their customers. We got to the checkout and the chap at the till said, "Have you been chased by a bear or something?" Then he said, "But you must be all right, then—when I'm not here I work as an undertaker, and everyone I've ever seen who looks like you has been dead." I am not normally lost for words and could only ask for my chicken.

These difficulties were compounded by the onset of senile diabetes. Predictably, this affected the lower extremities, which needed special care. Four years ago, I was at the University of California Research Station on Moorea (Fig. 12) and staying in a very elegant guest house. There were a lot of rats around after I had gone to bed, and I constantly kicked them away. But then I fell asleep. At three in the morning I got up to go to the washroom and when I put the light on was appalled to see blood all over the floor. It was the time of the O. J. Simpson trial and that gave me pause. Rats had eaten deeply into my right foot—deep holes into raw flesh. I dressed the wounds as well as I could and went back to sleep. Whereupon they did the other foot. The next night they had a go at my head, though there isn't much to eat on the scalp. When I finally got home, my wife said the top of my head was covered with tooth incisions. I had the great pleasure of faxing my physician who was tending the diabetes to announce, "Deeply regret, feet eaten by rats."

The following year there was a meeting in Honolulu I needed to go to, but I



Figure 12. Surveying the reefs with a Topcon Total Station, Moorea. 1994.

of this is, as the greatest of American geographers, Carl Sauer, said: “Do your fieldwork while you can, because otherwise it will be over before you know it.” and my doctor issued an edict forbidding me to ever be out of sight of a major hospital. One cannot change one’s life style just like that. A year ago, however, I was on Aldabra and went



Figure 13. The end of the road? The coral-reef section of my library.

chose to go to Midway for the albatross nesting (years before I had been on Kure when the plane taking me in had gone into a flight of albatross and disabled its engines; I have pictures of dismembered albatross all along the runway). I ran round Midway like a lunatic and inevitably got a deep foot infection. I managed to get back to California and had a week in the hospital where they got on top of it (my brother lost his leg in similar circumstances). Then the foot rotated irreversibly, under what was called Charcot’s Syndrome. It is amusing to ascertain that he described this as diagnostic of paralytic syphilis in Paris at the end of eighteenth century. Well, there aren’t too many of those around these days, even in Paris, but it evidently is the chief reason for diabetic amputation. My good students T. Spencer and S. Brooks were so intrigued by all this that they went to Paris to search out Charcot and indeed found his tomb in a cemetery on Montparnasse near the Sacré Coeur. The message of all ashore under circumstances that when I was in charge there I had forbidden. But now, most of the time I am in the library (Fig. 13) or the map room at home, piecing together the early history of coral reef science that I have outlined above and trying to complete literally dozens of papers that have never got beyond the revision stage. And I have already bought my tickets to continue the fortieth year resurvey of Rendezvous Cay in Belize.

I have to say that coral reef science has been extraordinarily good to me, and

also that there has been recognition of my work over the years in the Caribbean (first), the Indian Ocean (second), and the Pacific (third). For the Caribbean work, I had the Ness Award from the Royal Geographical Society in 1965 and the Prix Manley-Bendall from the Institut Oceanographique de Monaco and the Société Oceanographique de Paris in 1972. For the Indian Ocean work I had the Livingstone Gold Medal of the Royal Scottish Geographical Society in 1981. Then for the Pacific work there was the Herbert E. Gregory Medal of the Pacific Science Association in 1986 and the Davidson Medal of the American Geographical Society in 2000. I was appointed an Officer of the Order of the British Empire by the queen in 1979 (Fig. 14), and a Fellow of the American Association for the Advancement of Science in 2000. I particularly treasure the first award of the Darwin Medal of the International Society for Reef Studies in 1988, and indeed the Founder's Gold Medal of the Royal Geographical Society in 1979.

Elizabeth R

Elizabeth the Second, by the Grace of God, of the United Kingdom of Great Britain and Northern Ireland, and of Her other Realms and Territories Queen, Head of the Commonwealth, Defender of the Faith, and Sovereign of the Most Excellent Order of the British Empire, to Our trusty and well-beloved David Ross Stoddart, Esquire

Whereas We have thought fit, to nominate and appoint you to be an Ordinary Officer of the Civil Division of Our said Most Excellent Order of the British Empire

We do by these presents grant unto you the Dignity of an Ordinary Officer of Our said Order, and hereby authorise you to have hold and enjoy the said Dignity and Rank of an Ordinary Officer of Our aforesaid Order together with all and singular the privileges thereunto belonging or appertaining

Given at Our Court at Saint James's, under Our Sign Manual, and the Seal of Our said Order, this Sixteenth day of June 1979, in the Twenty-eighth year of Our Reign.

By the Sovereign's Command.
[Signature]
Grand Master.

Grant of the Dignity of an Ordinary Officer of the Civil Division of the Order of the British Empire to David Ross Stoddart, Esq.

Figure 14. My appointment as Officer of the Order of the British Empire (O.B.E.).

know why the stars shine. And I have tried to apprehend the Pythagorean power by which number holds sway above the flux.” And then he continues, quite amazingly for

Envoi: I would like to end these reminiscences with words from the prologue to volume 1 of the autobiography of Bertrand Russell in 1967 (I would like to quote the entire passage but would run into copyright problems, which I do not have time to sort out). It may seem impertinent of me to claim Russell's words for myself, but they are so true. “Three passions, simple but overwhelmingly strong, have governed my life: the longing for love, the search for knowledge, and unbearable pity for the suffering of mankind.” Russell describes the first of these, and then goes on: “With equal passion I have sought knowledge. I have wished to understand the hearts of men. I have wished to

the author of *Principia Mathematica* and so many other great philosophical works: “A little of this, but not much, I have achieved.” Every coral reef scientist—indeed every scientist—can only echo this. Little by little, you expand the frontier with the unknown, find out more about the natural world of the reefs, and try to make a difference. And the difference comes with one’s interactions with the closest colleagues and life-long friends, and with the extraordinary students one has been privileged to know. They know who they are and I cannot attempt to name them here.

Russell ended his prologue with these words: “This has been my life. I have found it worth living, and would gladly live it again if chance were offered me.” So would I though I am sure that all of us could do it better the next time around.

I would like here to publicly acknowledge the enormous debt I owe in all these procedures. First, to George Hemmen, David Griffin, and Len Mole at the Royal Society; second, to my school teachers who pointed my ambitions to the study of the tropical world; third, to my most extraordinary students; and finally, to all those folks from all walks of life who around the world over so many years have enabled me to follow my star. It has been my privilege to know so many who have been and are indeed becoming the salt of the earth. I am acutely aware that without many of them I would no longer be here. There could be no greater debt. So many expeditions, so many projects, so many papers, so much laughter. It has been fun.

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