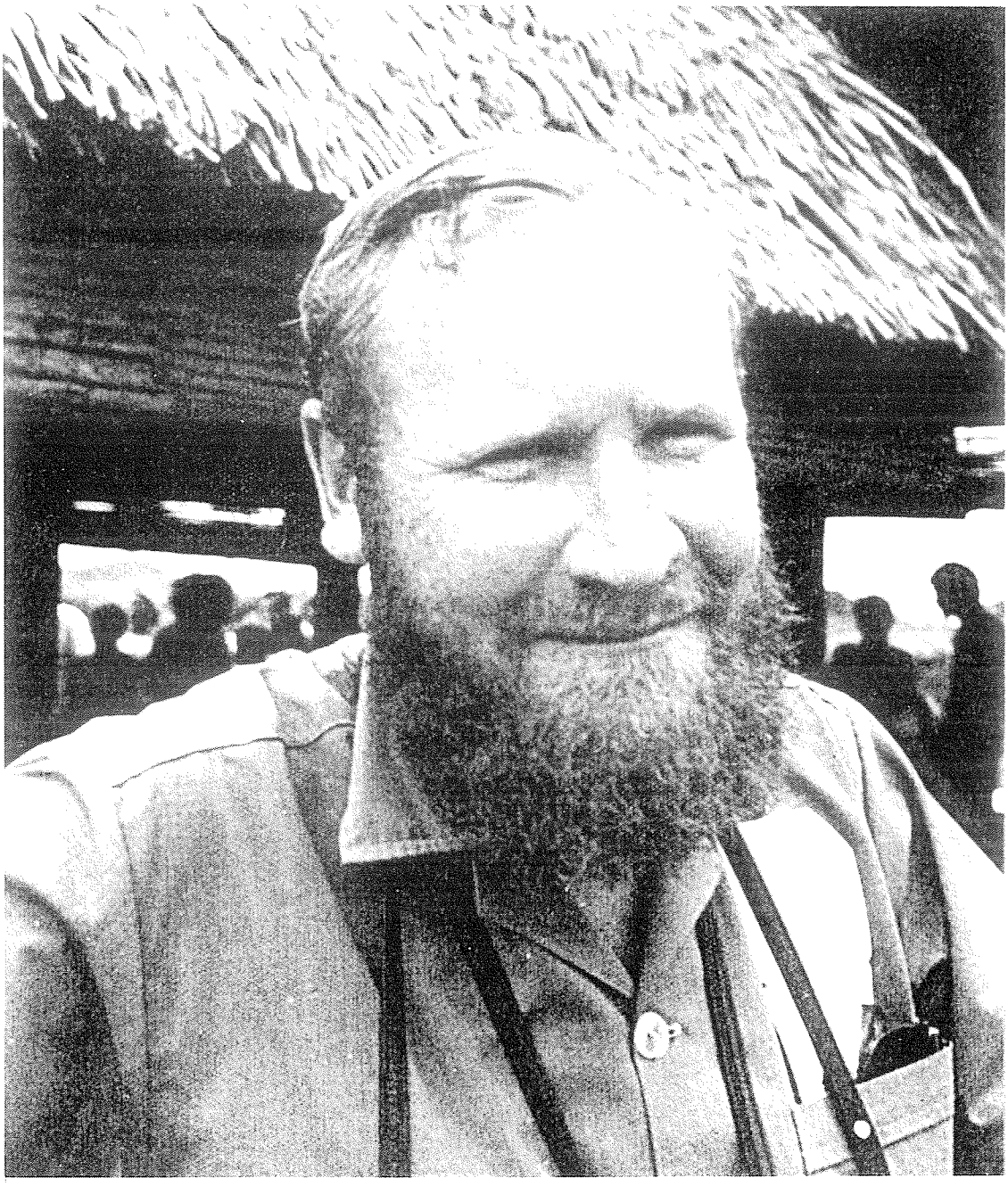

David R. Stoddart (Fiji, mid 1970s)

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.

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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.

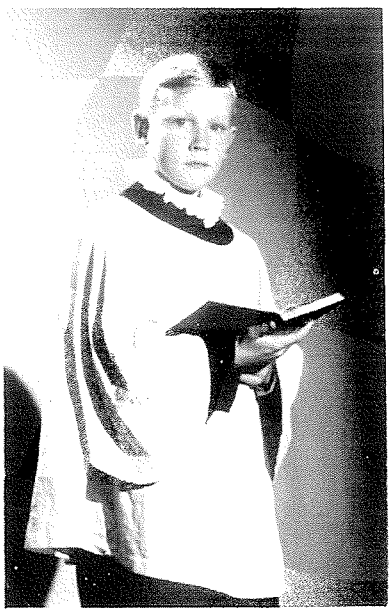


Figure 1. How it all began. I wish that I could say that the book I was holding was *The Coral Reef Problem* by Davis—it would have been much more interesting. 1950.

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

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 drive 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 form 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 form 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 Glagow 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 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.

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

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 my first meeting with Ray Fosberg.

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

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 ne 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 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

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.

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.

Then1969 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 Suwarrow. 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-maneuvered 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 form 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 form 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 Narturalists (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 Sachet), 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.

Figure 10. Engaged in field work with Barbara Brown, one of the founders of the International Society for Reef Studies.

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 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 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.

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.

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

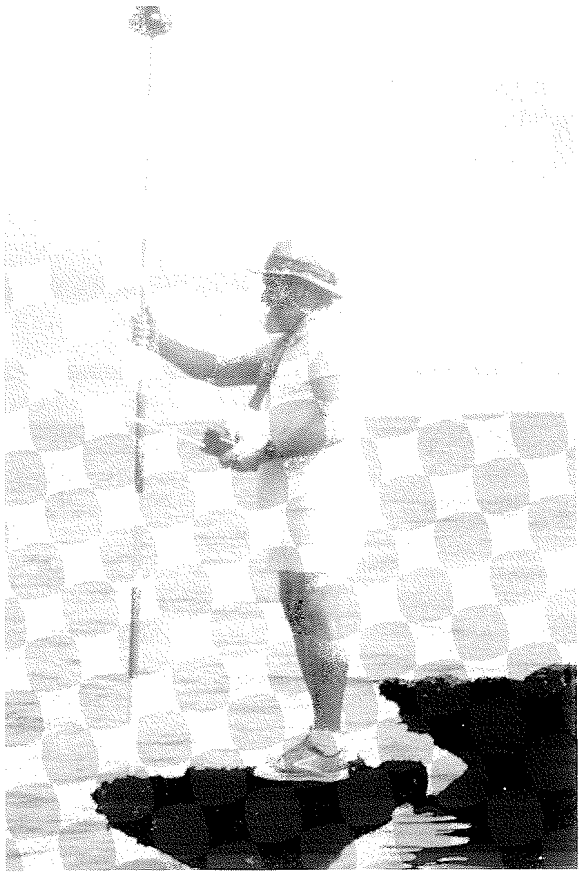


Figure 12. Surveying the reefs with a Topcon Total Station, Moorea. 1994.

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 syphilities 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 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 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.

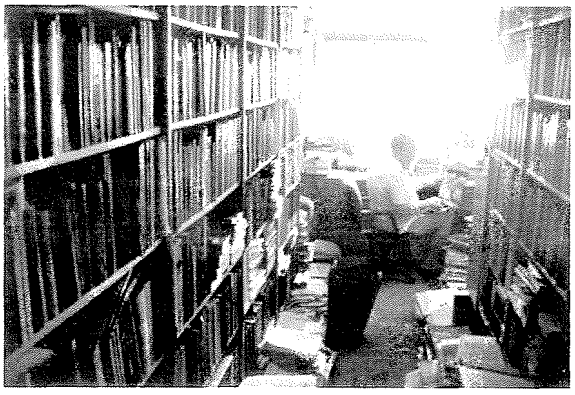
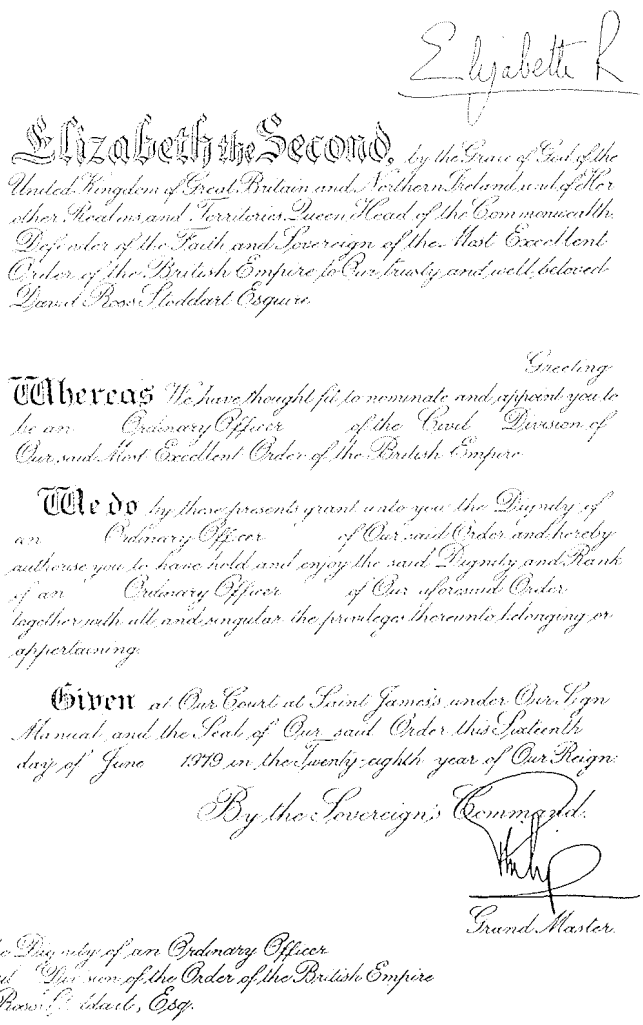


Figure 13. The end of the road? The coral-reef section of my library.

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 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 Daniel Ross Stoddart Esquire

Greeting

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 Fifteenth day of June 1979 in the Twenty-eighth year of Our Reign

By the Sovereign's Command

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.).

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 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

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.