Anatomy of an Expedition (Excerpts)

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Chapter One: fall 1965-march 1967 Genesis of an Expedition

CHAPTER ONE An oceanographic expedition is the crystallization of an idea; it begins and ends in the mind with a little sailing around in between. The idea that became the *Nova* Expedition began to take form in the fall of 1965. At that time, I had not been to sea for fourteen months, which was my longest period on land since I became an oceanographer in 1949, and I started to think about another expedition. It is now March 1967 and the expedition is about to begin, and at this point I cannot remember exactly when or where the subject first came under discussion. After fifteen expeditions the preliminaries get rather vague. Most expeditions, and I suppose this was no exception, are conceived late in the evening after enough oceanographers have become sufficiently relaxed to be optimistic about work at sea. The reason for this is that everyone knows that the eventual output is going to require a lot of hard labor and discomfort. The conversation can be generalized as follows:

Ed, why don't we go drill an atoll and find out why it sank?"

"Say that's great. I need a sequence of reef limestones to test this new idea...."

"Which atoll? What about Fakarava?"

"Where's Fakarava?"

"You remember. Where we stopped on the Downwind Expedition in the central Tuamotus."

"Yeah, that was nice, and we can get fuel and plane service at Tahiti. When?"

"Well, there is a gap in the ship schedule for Argo next July because Sam canceled out. He can't get delivery on his equipment. We could take some students along."

"I was going to visit Cambridge that month on my way back from the meeting in Stockholm, but I could go there before the meeting."

"We'll need to know the crustal structure in order to pick the best spot to drill. I wonder if George would be interested. Where is George? Hey, George how would you like to go to Fakarava next July?"

Perhaps one out of five of these evening expeditions eventually puts to sea, and then it is at Majuro because the Navy will provide free air service and in August because the ship needs an overhaul in July.

Considering that it is the taxpayer's money, it may sound a little casual, but oceanographers constantly get new ideas to test, and they have to go to sea at one time or another, and there is no virtue in going to an unpleasant atoll if a beautiful one has the same geology. In any event, there is stringent product control. If you don't produce results, you can't get money for your next expedition. You may not get the results you expected—this is research, not bridge building—but you have to get significant results of some sort.

An expedition needs an idea, scientists who are interested in it, a ship capable of the work, and money. Bringing them together takes a lot of talking, adjustment, writing, modification, time, flexibility, and work. The steps have not changed in the last 150 years. The first United States Exploring Expedition in 1838 and the first great oceanographic expedition on the Challenger in 1873 followed the same path of organization as the Nova Expedition in 1967. Indeed, these and the intervening expeditions had similar problems, used about the same size ships, and cost similar amounts. The great changes have been in the ideas under investigation and the equipment on the ships. All this historical precedent, however, is little consolation as the painful process is repeated yet again.

The basic idea of *Nova* is to try to determine the development and geological history of the peculiar Melanesian region

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in the southwestern Pacific where the sea floor seems to be part continent and part ocean basin. I shall elaborate on this shortly. Because the region is on the opposite side of the Pacific from Scripps Institution of Oceanography, the ships will incidently be available for other work coming and going. Several scientists at Scripps were immediately interested in the idea, and we decided to try to get a commitment for some time on one or more of our oceanographic ships. The ships belong to Scripps, and a committee of our oceanographers schedules their use. Even so, ships are hard to come by because so many scientists want them. On my first inquiry in the fall of 1965, I found that the next long period definitely available on one of our ships was in August of 1968, and then only if I had any plans for the area between Mauritius and Cape Town in the Indian Ocean. Fortunately some of my interested colleagues had time allotted in 1967, and by juggling the schedule with other scientists we put several months of ship time into one continuous block and still had everyone as happy as before. The next step was to look for money.

It may be useful at this point to consider how oceanographic expeditions are financed and what they cost. Captain Cook's first voyage in 1768-1770 was supported by King George III with a grant plus a bark, *Endeavour*. The total cost for the ship and outfitting exceeded £8235, and it appears that the voyage cost at least £10,000 or in round terms about $28,000. This sounds very modest, but for comparison with modern expeditions we must allow for the decreasing value of money. At that time, for example, a man with an income of $1200 a year was able to "keep five or six servants indoors and out, to look well after his relations, to travel freely, and to exercise a generous hospitality to rich and poor."

A reasonable approach is to assume that the purchasing power of a given amount of money has decreased by two percent per year. Accordingly, it takes seven times as much money to buy something at the end of a century as it does at the start. Consequently, it cost King George about 1,400,000 modern dollars for Cook's voyage. In addition, the scientist, Banks, whose tax-free income in modern terms was about $1,000,000 per year, put in a considerable amount of his own

money. The expedition lasted almost three years and in modern money cost a minimum of $1300 per day.

The cost of the first United States Exploring Expedition (Wilkes Expedition) in 1838–1842 is buried beyond recall in government records. However, in 1836 Congress appropriated $300,000 for the expedition, and the Navy reported that it was all spent in 1837, a year before the ships put to sea. The expedition involved four ships for four years and cost perhaps $10,000,000 in modern money. Each ship cost more than $2000 per day even though all were very small and some did not last for four years. A century and a quarter later, during *Nova*, we expect to land on the Exploring Islands, named after and surveyed by this expedition.

The value of the *Challenger* Expedition of 1873–1876 was enormous and so was its cost. The British government apparently paid out a sum equivalent to the estimated expense for the recent Mohole Project to drill a hole in the bottom of the sea. The modern project would have been so expensive that the American Congress in 1966 decided that the richest country in the history of the world could not afford it.

So, on historical precedent we needed to locate some rich and generous patrons if the *Nova* Expedition were ever going to sea. Fortunately for the success of American oceanography some far-sighted men established just such funding organizations fifteen to twenty years ago. No longer must appeals for support for each expedition be directed to king or Congress. Indeed, it would be quite impossible to have 50 to 100 American oceanographic expeditions per year without the existence and support of federal agencies. The most important for the past two decades has been the Office of Naval Research, which produced modern oceanography by supporting the expansion of marine institutions and fleets of research vessels. Somewhat later the National Science Foundation entered the picture and now provides about half the support.

The main cost of oceanography is for ship time. A research ship without support for operating funds is an albatross around the neck of an oceanographic institution. Not too long ago the cost of a ship had to be justified on the scientific merits

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of each expedition. Consequently, oceanographic institutions were in about the position of a business which rents out ships but cannot make a profit to cover the expenses while the ship is idle. The daily cost of a ship was determined at the end of a year by dividing the annual cost by the number of days of use. It was a gigantic gamble to use an unpopular ship for a week at the start of the fiscal year. An oceanographer might find that he was expected to find funds for fifty-one idle weeks. This insane system seemed necessary because of the federal system of appropriating money for only one year. Finally, a few years ago officials in the Navy and the National Science Foundation figured out how to fund ships on an annual basis, and life became much simpler.

The total cost of Nova was initially estimated as follows:

ARGO, ship time at $3000/day	$ 720,000
HORIZON, ship time at $2500/day	400,000
Direct support of expedition	270,000
Indirect costs of research	100,000
Scientific and technical salaries	200,000
	$1,690,000

This is certainly a staggering amount, but at $4000 per day per ship it is roughly what king or Parliament or Congress has always paid for marine expeditions during the last 200 years. It would have been much cheaper not too long ago. On our first Scripps deep sea expedition in 1950, *Horizon* cost only $450 per day, but that was before we loaded it with special equipment.

Due to the recent advances in federal money handling we did not need to ask for money to support the ships (which includes wages for the crew). Moreover, most of the salaries for scientists and technicians are paid by the University of California or by long-term state and federal research contracts. The indirect costs of research are those connected with this expedition but which are paid out of existing contracts with some broad objective. Thus the National Science Foundation supports a global study of carbon dioxide in the atmosphere, and some of this money will be used to make measurements

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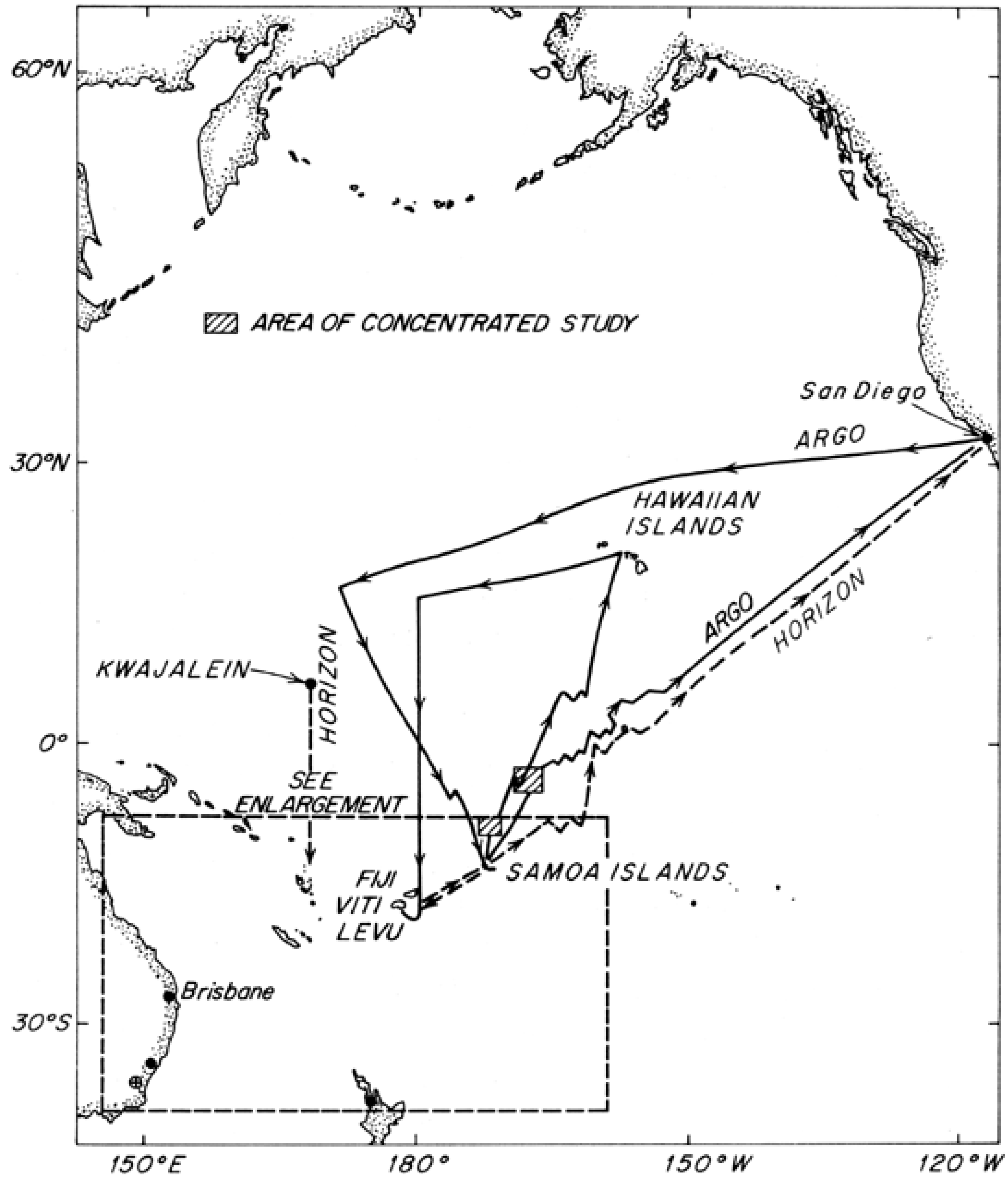
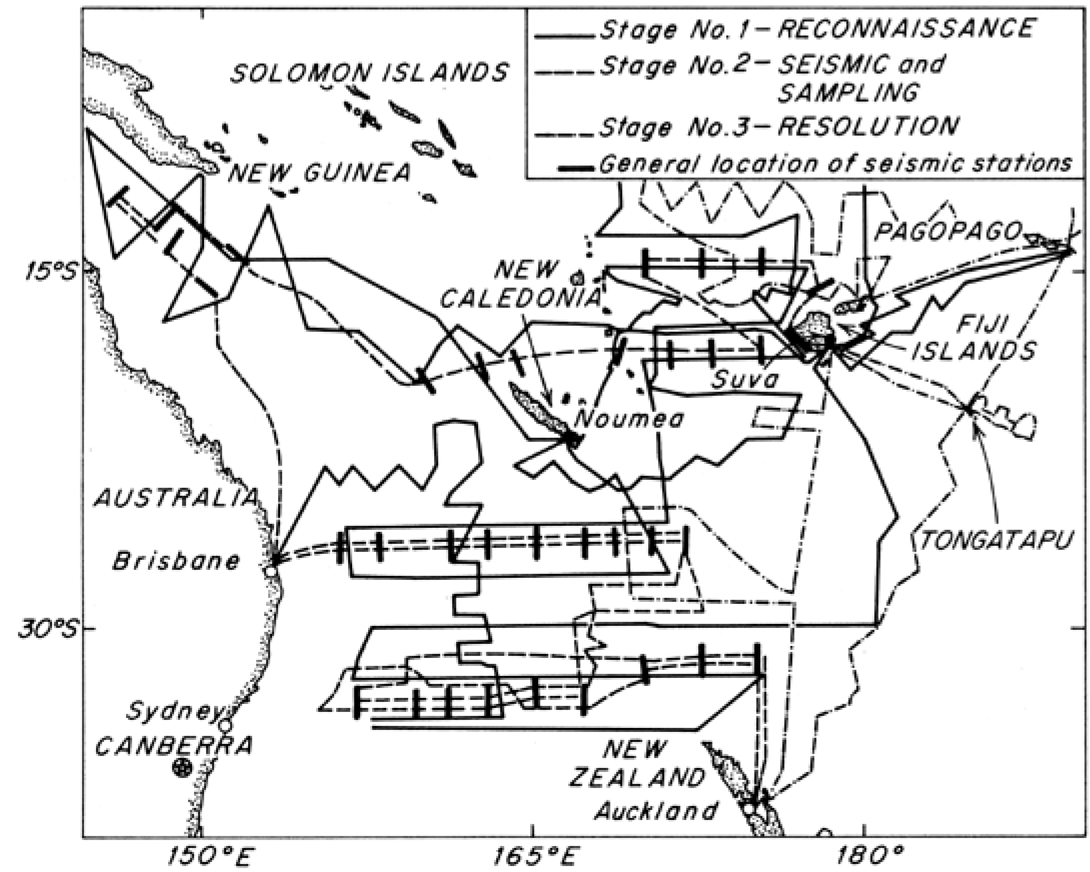
during this particular expedition. In sum, in order to put this costly enterprise under way, we estimated that we needed $270,000, or about 15 percent of the total.

My colleagues, Harmon Craig, Edward Goldberg, Edward Winterer, Robert Fisher, and Manuel Bass, plus Charles Helsley of the Graduate Center of the Southwest, joined me in submitting a request to the National Science Foundation for this amount on 13 May 1966. As in all such proposals, we said what we want to do and why it is worth doing, who we are,

The general plan of *Nova.*



The general plan of *Nova.*



The detailed plan of ***Nova*** **in the Melanesian region.**

The detailed plan of *Nova* in the Melanesian region.

what we have done that makes it likely that we can do what we are asking money to do, the capabilities of our university to carry out the proposed work, and what it will cost. This proposal went through the customary elaborate review. It was sent to scientists doing related work at other institutions, and they graded it according to the quality of the scientific proposal and the abilities of the people and university. Then a geophysical review panel of government and academic scientists compared our evaluated proposal with those from other scientists and universities and arranged them in order of merit with due consideration of the cost. It is easy to identify one of these panel members if you ride on the airplane he is taking to a meeting. He has with him three brown-covered volumes of proposals with a total thickness of about eight inches, and he is busily reading the last one while other people watch the movie. He does this three times a year and thus evaluates about two feet of proposals requesting perhaps $20,000,000. It is hard

work. I was on the panel for three years and for the same reason as the other members. I never met anyone who likes it, but we all agree that if scientists won't evaluate the merits of scientific proposals then it will have to be done by nonscientists who may have to judge proposals by criteria other than scientific merit.

This evaluation process takes three to six months, and meanwhile planning for an expedition has to continue with the hope that it will not be wasted. My colleagues and I talked to other scientists who might be interested in problems in the southwestern Pacific. Gradually more of them decided to participate in *Nova*, and technicians and special equipment were organized and integrated into as comprehensive and efficient a whole as possible. We did not need to know which ports we would use for fueling when we submitted the request for funds. However, we did need such information before sailing so we examined various permutations of ports, distances, ship speeds, fuel consumption, water supplies, explosive handling, airplane connections, and general logistics. Some matters require a long lead time. My hair is very short because when I was a poor graduate student my wife cut my hair and she liked it short. However, like all blonds, I have a skin sensitive to the sun and my hair is getting thin. I usually wear a hat in the tropics, but it seemed prudent to have some natural covering between the top of my head and the tropic sun. Consequently, seven months before the anticipated departure time I started to let my hair grow.

Early in August I got a phone call from Joe Creager, an oceanographer from the University of Washington who was doing his public service by working for a year with the National Science Foundation. He said that our proposal was approved on merit but that it cost more money than was available. Could we get along with less? I said we could, although with less I did not think that we could do everything we planned. How much less? Joe said "Half," and another time of troubles began.

What to cut? The categories in our proposal included salaries, assistantships for graduate students, supplies, equipment, travel, and a 42 percent markup to cover University

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overhead on salaries and wages. Almost all graduate students in oceanography receive substantial support, and most of it comes from federal contracts. This is not unreasonable, because the students help collect and analyze the results. On average they are the cheapest, highest-quality, and most eager workers in the world. We cut support for four students out of the budget, but we knew we would have to find the money somewhere else.

The next item was supplies and expenses. I quote from Sir Wyville Thomson concerning the outfitting of the *Challenger* Expedition in 1873:

It is almost inconceivable how difficult it is to keep instruments, particularly those which are necessarily made of steel, in working order on board a ship; or how rapidly even with the greatest care they become destroyed or lost. For this reason it is necessary to have an almost unlimited supply of those in most frequent use, such as scissors, forceps, and scalpels of all sizes.

We have the same problems and must constantly restock everything from pencils to expendable equipment which is used only once but is cheaper than the ship time necessary to recover it. We chopped our request for supplies mercilessly but again with the certain knowledge that we would have to ask for more for the next expedition. We did have one wind-fall. George Shor, who had decided to measure crustal thickness in the southwestern Pacific, needed a large supply of explosives for the purpose. This was in the *Nova* budget, but meanwhile a long-standing request for explosives from the Navy was at last granted and we did not need more from the National Science Foundation. That item alone saved $12,000.

Next we came to heavier equipment which we also wear out or lose in the normal course of oceanographic work. We are not the only ones. A Russian oceanographer, Gleb Udintsev, once learned that we were planning to dredge in the world's deepest water in the Marianas Trench. He told me that if we dredged up a deep sea camera it was his because he had lost one there. The camera cost about $10,000 and we did not find it—in fact we lost one ourselves. What can you expect when you attach a camera to six miles of wire and try to photograph

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the rocky sea floor from a drifting ship? We eliminated the least urgent items and hoped for good fortune at sea and future replacements even though they would be of no use on this voyage.

Finally we came to a whopping request for $76,400 for travel by air to various ports that the ships would touch. We got our Marine Facilities department which operates the ships to agree to pay for rotating the crew, and that cut the request in half. Of course, it also increased the ship operating cost, but that was covered by another budget. We could have eliminated all this air travel by merely returning the ships to San Diego every few months, but that would have been a waste of money—which is a very different thing from tailoring requests for money to fit the amounts available in different contracts.

After many a painful session we cut the budget request in half and resubmitted the proposal on 10 August. Creager knew how short the planning time was growing and soon phoned that the money was on the way. He also said that he had hoped that we might reduce the amount of ship time required because the Science Foundation was running short of money for ships. After our juggling this seemed a little ironical.

One problem we face at Scripps is naming an expedition after we create it. This does not arise at every oceanographic institution nor was it ever necessary in the past. If the Royal Society sent the *Challenger* around the world, the voyage was called the "*Challenger* Expedition," which was entirely adequate because no expectation existed that the ship would ever go on another expedition. Likewise the "*Galathea* Expedition" and the "Swedish Deep-Sea Expedition" clearly were conceived as unique efforts. When such ships as *Atlantis* and the *E. W. Scripps* began to be used for repeated cruises in the 1930s, the problem of identifying the results arose and two solutions were used. A sample is uniquely designated by the ship name, cruise number, and sample number, thus: *Atlantis*, Cruise 6, Sample 4, or by specifying the cruise location, thus: *Scripps*, Gulf of California, Sample 4. However, the second system may produce confusion if the ship returns to the Gulf of California, and it is not very useful if the expedition is to a

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nameless place such as the region where a certain ocean current exists, e.g., the Gulf Stream. Lamont Geological Observatory and the Russian Institute of Oceanology, among others, give each cruise a number, but initially by chance and later by design we name ours.

The first large Scripps expedition in the deep sea was focused on the mountains of the central Pacific and was named *Mid-Pac*. It was followed by *Northern Holiday*, which derived its name from the nautical term "holiday," meaning a place where no information exists. The name caused a little muttering among the financing agencies in Washington. When it was proposed that *Northern Holiday* be followed by *Southern Holiday*, the muttering became audible in La Jolla. Warren Wooster, who was in charge of the expeditions, decided the Navy was not ready for nautical terminology and changed the name. Then we had expeditions with a variety of names, such as *Capricorn* after the Tropic of Capricorn, which was the southern limit of the region explored, and *Cascadia*, which was the name of a continent thought to have once existed off Oregon where we were working. We had a great series of wind names typical of the region of operations: *Chubasco, Chinook, Zephyrus, Downwind, Monsoon*. We had momentary aberrations like *Six-Pac* and inspirations like *Blue Flash*, which was one of the first cruises using an arcer—which emits such a flash. We began to have so many similar names such as *Dolphin* and *Dorado* which means the same thing but in Spanish that we were in danger of confusing the samples. We all agreed not to name an expedition without the prior approval of Bill Riedel, curator of geological samples, who would ensure that the abbreviation of a suggested name would not be the same as one already used.

What to name the new expedition to Melanesia? Usually several weeks or even months pass before the natural name of an expedition emerges. It becomes increasingly inconvenient to organize a nameless expedition or one which different people call by different names. Finally Jerry Winterer noted that the region we would study contained New Caledonia, New Zealand, New Guinea, and the New Hebrides and suggested the name *Nova*. It was short, pertinent, and distinctive,

and Riedel was enthusiastic because we had few "N" expeditions. Now we had a name.

Next, or meanwhile, we needed to make base maps for our work. If you want a map to do something like hike in the Sierras or navigate a ship into Suva harbor you just buy one from the appropriate government agency. Not so in oceanography, where the maps have to be changed after each expedition and commonly are made on board in order to plan the next day's work. We needed to know about every unpublished geophysical observation anyone had taken in the southwestern Pacific, in order to avoid duplication and for making detailed plans. The only way to get such information is to write to or visit every person who might have it. There are both formal and informal worldwide information networks whereby oceanographers keep track of their colleagues' work. I had already acquired a large amount of information by making the usual carefully respected promises not to publish other people's unpublished data and by agreeing to make our own observations similarly available. There remained the problem of the most recent data collected by oceanographers in Australia, New Zealand, and New Caledonia. A lot is always known which has not even been processed to the point where it can be sent to anyone else. I needed that information but getting it would require work by other people whose time I did not want to waste. The only practical solution was to visit the laboratories and explain exactly what we hoped to do and what information we needed. The trip would also provide an opportunity to interest oceanographers in these countries in participating in *Nova* and thereby getting more scientific results from the same effort at sea. In any event, it is not very courteous to carry out a lengthy expedition in waters near other oceanographic laboratories without asking the scientists there if they would like to join in. Finally, the trip would give me a chance to inspect prospective ports once again and see whether the surrounding countryside would be suitable for geological field trips for the students.

My wife had never been in this region so we went through the necessary arrangements to leave our children. A month later we had talked to geologists and oceanographers and

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naval hydrographers in Wellington, Sydney, Noumea, and Suva; scanned the geology and logistic support available in the surrounding regions; given talks about the expedition plans and found a number of bright young oceanographers who wanted to participate; and had a load of unpublished charts and manuscripts and a painting of a mining scene by Pro Hart. We had had a delightful time thanks to the hospitality and generosity of many people we knew and others we had not met before. Many should reappear during the course of the expedition.

Back in La Jolla my associates Stuart Smith, Tom Chase, and Isabel Taylor plunged into the compilation of the new data for our base charts. Tom and Stu were each to lead one of the early legs of the expedition and had a vital need for this information. I also had the assistance of several graduate students who would join in the work at sea and might eventually use some of the results for Ph.D. theses.

My own effort was concentrated on further planning. The possible permutations in an enterprise like this are beyond belief. Somehow all the senior scientists have to be satisfied that their work can be done, equipment and supplies have to be directed to the right places at the right times, and the budget has to be balanced. It always reminds me of planning an amphibious assault—as I did in the same region many years ago—except that money is important.

I have mentioned the explosives before; the further problems connected with this item alone may illustrate something of what is involved. George Shor had about 12 tons of TNT stockpiled in the Navy ammunition depot in Pearl Harbor. Another similar amount would provide all we needed, and it was available in Southern California. However, *Horizon* could not carry its half of the explosives because its magazine had to take supplies needed for other work before we took it over in Kwajalein in April, 1967. It could have returned to Pearl Harbor, but that would have reduced time in the southwestern Pacific. We hit upon an elaborate scheme. Argo was scheduled to conduct oceanographic work from San Diego to Pago Pago to Pearl Harbor and then back south to Suva, where it would be joined by *Horizon*. We could load the local TNT on

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Argo, unload and store it at Pago Pago, and then reload the ship with the Pearl Harbor TNT when it reached there. *Horizon*, which by then would be half empty, would make a short trip to Pago Pago and back to Suva collecting useful although less important data on the way. But could we store explosives at Pago Pago? Appropriate inquiries indicated that we could. Could both ships enter Suva harbor with explosives? We knew from previous expeditions that we had done so, but had the port regulations changed? Our ship agent in Suva said that they had and we would have to unload everything onto a barge in midstream before coming to the dock for fuel. So a little time might be lost.

At about this time a new development entered the explosives problem. Charles Helsley, of the Graduate Center of the Southwest, would also measure crustal thickness and structure on *Nova*. He flew to La Jolla, and he and George Shor developed some intricate negotiations whereby the TNT in California became an equal quantity of Nitramon, which is commercial fertilizer except when it is triggered by an explosive fuse. This safe stuff could be stored on *Argo* outside the magazine and would not have to be off-loaded at Pago Pago before the magazine was filled with TNT at Pearl Harbor. Consequently, *Horizon* need not go to Pago Pago and would be available for more important work. Moreover, Helsley would shoot off all the TNT between Suva and Brisbane, and we should have no problem entering the latter port, which had not yet replied to our inquiries about bringing in explosives. Then Brisbane said it would not let us enter even with the quantity of explosive fuses needed to set off the Nitramon.

So why were we going to Brisbane? Originally we planned to stop at that time in Noumea because it is more centrally located in the southwestern Pacific and it costs less to fly there from California than to Brisbane. However, Helsley had to ship some of his gear on to South Africa to join another oceanographic expedition. To meet this other sailing date he had to have the gear in a port with frequent sailings instead of Noumea, and so we put all other considerations aside. With the news from Brisbane about explosives, the port question was raised again. It turned out that a ship sails on an irregular

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schedule from Noumea to Sydney about the time we should arrive. By radioing from sea for a final sailing date we could be sure to arrive in time to load Helsley's gear. I phoned him in Texas to ask if this port change was agreeable to him considering the new information about freighter sailings. He agreed and said, moreover, the expedition off Africa was now looking questionable and he was no longer in such a hurry about sending his equipment there. We decided to stop at Noumea, where explosives are brought in frequently for the mines.

The above is a simplified version of the explosives problem. Helsley also shipped some Nitramon from Texas which was supposed to go by commercial truck. A trucking strike stopped that. The Nitramon came by truck driven by a graduate student and his wife. The truck broke down but was repaired and arrived just before sailing time.

There were also the mysterious and complicated affairs of the different air fares to Pago Pago, the missing magnetometers, the carbon dioxide program, and the perplexing problem of who would be working in the Coral Sea.

Finally, in April 1967, Horizon sailed south from Kwajalein and Argo sailed west from San Diego. The Nova Expedition was under way, seventeen months after it was conceived. Not a bad record, really; the Challenger Expedition took eighteen months and the United States Exploring Expedition took more than ten years to do the same thing.

Chapter Three: fall 1965-march 1967 Scientific Exploration of the Pacific Ocean

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CHAPTER THREE One of my most distinguished colleagues at Scripps once designed a research ship. It was clear from the plans that none of the equipment would work when the ship was rolling in a normal swell. When queried, he explained that one could hardly expect a *real* scientist to work on something that pitched and rolled, and that the ship was a floating laboratory which would go to scientifically interesting places where the researchers would join it by airplane. The ship is a success.

Nevertheless, a very great scientist once sailed the Pacific and welcomed the opportunity even though he was wretched during every roll. Charles Darwin received a B.A. in theology, Euclid, and the Classics from Cambridge. By the time he was twenty-two, he had received some scientific training as an assistant to some very able men and knew how to stuff birds and collect fossils. As a result, on 24 August 1831, he received a letter from J. S. Henslow recommending that he take a position as unpaid naturalist on the circumnavigating voyage of *H.M.S. Beagle* with the words, "I have stated that I consider you to be the best qualified person I know of who is likely to undertake such a situation," a modest but realistic evaluation. Darwin first refused because his father had doubts about the merits of a position offered just before sailing. These doubts were resolved by family discussion, and young Darwin prepared to

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spend five years at sea on a small and overcrowded surveying ship. His accommodations consisted of a hammock and a few drawers in the chart room, although he did dine in the cabin with Captain Fitz-Roy, which gave him a "halo of sanctity" which caused the midshipmen to call him "Sir." He was ill during most of his later life and it is suspected that prolonged seasickness was at least a contributing cause. Oceanographers sometimes lie sick on the soaking deck of a ship until the moment required to make observations, then rise up, work, and lie down again. So it was with Darwin. On 25 April 1883, when he was about to be buried in Westminster Abbey, Admiral Lord Stokes wrote to the *Times* as follows:

We worked together for several years at the same table in the poop cabin of the Beagle during her celebrated voyage, he with his microscope and myself at the charts. It was often a very lively end of the little craft, and distressingly so to my old friend, who suffered greatly from sea-sickness. After perhaps an hour's work, he would say to me, "Old fellow, I must take the horizontal for it," that being the best relief position from ship motion; a stretch out on one side of the table for some time would enable him to resume his labours for a while, when he had again to lie down.

On 9 June 1834, the *Beagle* entered the Pacific and remained on the Chile coast until September 1835. During that time, Darwin was much ashore observing the uplifted ancient shorelines, collecting fossils, and otherwise working as a young naturalist. By Christmas Day 1835, he was in New Zealand, having spent a few weeks in the Galapagos Islands, about ten days at Tahiti, and the remainder of the time at sea. In that brief period, he made the observations that led him gradually to the *Origin of Species* and the theory of evolution. He also began to formulate ideas on the origin of coral reefs and the development of volcanic islands which were to influence greatly the future understanding of the geological history of the ocean basins. Darwin visited only one atoll and that was in the Indian Ocean in 1836, but he saw many volcanic islands in various stages of development from active eruption to almost total destruction by wave and stream erosion. He published *The Structure and Distribution of Coral Reefs* in 1842 and therein set forth his celebrated theory of the origin of

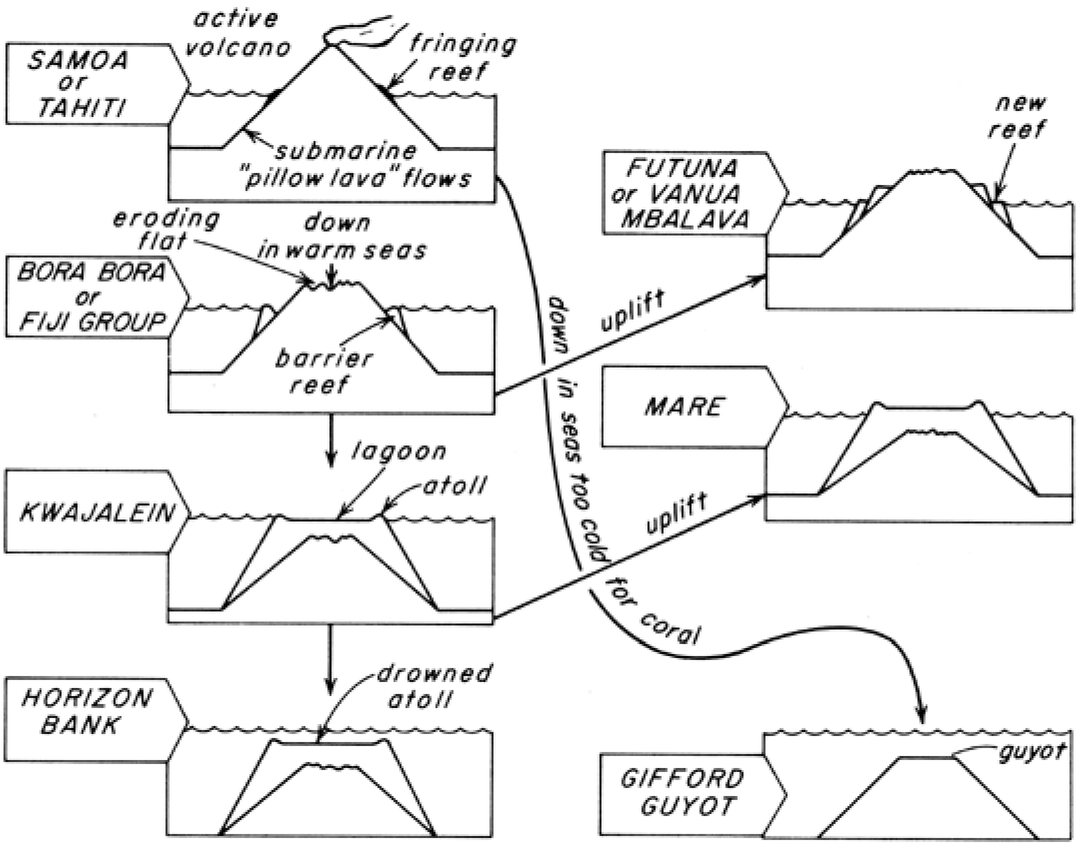

Figure
atolls. What he proposed was that some volcanoes grow up through several miles of water and become islands. When they become inactive, they are fringed by coral reefs if the ocean is warm enough. Then, he said, the volcanoes slowly sink and the reef grows upward so that a lagoon forms between reef and island. Finally, the volcano disappears, leaving a roughly circular lagoon within an almost continuous ring of coral reef. Previously, the prevailing idea about the origin of atolls had been that of the great geologist Lyell, who thought that the rings of coral marked the rims of submerged volcanic craters. Darwin had a copy of Lyell's Principles of Geology with him as a guide for his work on the Beagle. However, when Darwin returned, the master quickly accepted the ideas of the pupil, and Darwin's theory was off to a great start.

Not many scientists had either the grasp or vision of Lyell, and Darwin's ideas on coral reefs were hotly disputed. By the 1890s the Royal Society decided to resolve the matter by drilling an atoll to see whether it was a volcano capped by the thick coral demanded by Darwin's theory. I have not attempted

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to reconstruct the sequence of events whereby this drilling was brought about, but they cannot have been very different from the more familiar ones which occurred in the 1940s when the experiment was repeated by American scientists. Someone got the idea; a few friends and associates thought it was worth pursuing; the Royal Society appointed a committee to consider the matter; a recommendation was made to Her Majesty's Government to provide support; and another committee picked a drilling site after the most careful evaluation of available knowledge about atolls and the expectable results of drilling in various places. The site selected was the atoll of Funifuti and several holes were drilled in 1896–1898, the deepest to 1114½ feet with coral limestone at various places from top to bottom. It might seem that Darwin was confirmed, but critics quickly pointed out that below 300 feet all the corals were fragments which clearly had been rolled about. This meant that the hole might have been drilled in a capping of coral which had merely rolled down the side of a volcano. Submergence below 300 feet was still in doubt.

Most of Darwin's critics were concerned with the origin of the circular shape of atolls, whereas Darwin had been chiefly interested in whether large areas of the sea floor move up and down. Consequently, the scientific discussion in a remarkably large number of books was clouded by semantics. The matter was at last resolved when the atom bomb tests in the atolls of the Marshall Islands after World War II provided enough money for a thorough geological study. By this time the thickness of coral could be measured indirectly by geophysics and directly by the advanced drilling techniques developed to produce oil. Everything proved that Darwin was right. At Eniwetok Atoll the coral was almost a mile thick, and finally the drill brought up some basalt—the top of a volcano which had been near the sea surface more than 40 million years ago. With Darwin's theory of atoll formation confirmed, it was possible to turn to the, to me, more interesting question of movements of the sea floor. This will be one of the problems investigated during the *Nova* Expedition, so it is still not wholly resolved.

Between the *Beagle* and *Nova* the broad outlines of the

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geology of the Pacific Basin were discovered. The earliest efforts to study the deep ocean began about the time that the last island was discovered in 1859. Much of this was in the North Atlantic, which was near the major scientific centers of Europe and where there was a great financial incentive to learn whether it was possible to lay a submarine telegraph cable to the United States. It was not too long before popular and political interest in oceanography was widespread on both sides of the ocean. Once, a line of deep sea soundings was made across the Atlantic on an American cruise which had the primary purpose of teaching a useful trade to the indigent orphans of Philadelphia.

The Pacific was more remote, but some oceanography was being done—notably in the preparation of charts of surface currents and the distribution of whales—by Matthew Fontaine Maury, the Superintendent of the Depot of Charts and Instruments of the U.S. Navy from 1842 to 1861. Maury set himself the task of making order out of the countless entries in American ships' logs. He compared the course a ship appeared to be steering according to its compass with the course it proved to have steered according to the next position determined by star sights. The difference indicated the drift of ocean currents at that time and place. By plotting all the sightings of whales, he mapped migration paths which took great schools across half the ocean at different seasons. His objectives were the support of American commerce and the Navy, but his methods earned him international esteem and cooperation. From 1842 to 1847, his office processed 26 million observations specially collected for him in the world oceans. In addition to this work, he made the broadest studies of the ocean from the data then available. By 1859, his *Physical*

Geography of the Sea was in its sixth edition, and he knew everything there was to know on the subject. Except for the North Atlantic, all he could say at that time was:

The first specimens have been received from the coral sea of the Indian Archipelago and from the North Pacific. They were collected by the surveying expedition employed in those seas. A few soundings have been made in the South Atlantic but not enough to justify deduction as to its depths or the shape of its floor.

This lack of knowledge led the Royal Society to send the *Challenger* Expedition around the world in 1872–1876. The ship was specially fitted out to sound and dredge in very deep water and was blessed with a captain who was a Fellow of the Royal Society. The ship displaced 2306 tons, which is far larger than most oceanographic ships now in use, but, of course, it was due to be away from its home port for a very long time. There were twenty-three officers and a large crew but only six scientists even though the objectives were wholly scientific. The chief scientist was Sir C. Wyville Thomson, who only reluctantly accepted such a lengthy assignment. The results of this expedition came out in a vast array of scientific volumes, logs, reminiscences, and popular accounts. Its scientific impact was enormous. The samples collected are still being used today. I have collected some fragments of the geological samples for study myself while in London. They are protected in an underground civil defense center, left over from the blitz, which is situated on the grounds of the British Museum, Natural History.

The ship departed England at 11:30 A.M. on Saturday, 21 December, which I find rather amusing because these days we hardly dare antagonize our crews by leaving or even arriving home on a weekend. Our ship *Argo* is due to return from the *Nova* Expedition on 18 December for no special reason except to have everyone home for Christmas. The attitudes of pioneers are understandably different from those of present oceanographers who go to sea again and again and again.

One of the junior scientists on the *Challenger* was Mr. John Murray. By 1908, he had become Sir John Murray K.C.B., L.L.D., F.R.S., and anyone who collected a deep sea bottom sample or even measured the depth, took care to send it to him at the Challenger Society Office in Edinburgh. Consequently, when we look at his maps of that date stating "compiled from the latest sources," we can be confident that they are complete. His chart of the Pacific shows about 500 soundings except in the southwestern Pacific to which I shall return. Of those in the deep open Pacific, almost all came from two sources: oceanographic expeditions and surveys for submarine telegraph cable routes. The chief oceanographic expeditions

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were the *Challenger* itself which wandered from Australia to New Zealand to Japan to Hawaii to Tahiti and on to Chile; and various cruises of the *Albatross*. This later vessel belonged to the U.S. Fish Commission and was under the scientific direction of Alexander Agassiz, son of Louis Agassiz, who founded the Museum of Comparative Zoology at Harvard which published the records of this work. When the *Challenger* took the first deep soundings in the southeastern Pacific, a broad elevation was discovered under the ocean. Inasmuch as no land was nearby, this elevation was named the Albatross Plateau after the only birds which frequent the region. When we were once incidently following the approximate track of the *Challenger* from Tahiti to Valparaiso in 1958, I realized to my surprise that we were in the most remote spot from land on earth and once again there was no other life but the albatross. It seemed particularly felicitous to Agassiz that he had available a ship called the *Albatross* with which to study the Albatross Plateau. The two ships took most of the soundings available in the region until after World War II when the U.S. Navy began to provide logistic support for exploration of the Antarctic and ran many sounding lines from the Panama Canal to New Zealand.

The situation in the north and central Pacific was entirely different. There bands of soundings are shown on Murray's map along lines from the United States to Hawaii and thence to Fiji and to Guam and the Philippines. Another line went directly from Japan to Seattle. It will be apparent enough that these were the major political and commercial centers in the Pacific and that the routes were surveyed in order to pick locations for submarine cables. The surveys, seeking to avoid great depths which would destroy the cables, found the deep trenches that border the western Pacific. In its broad outlines, the Pacific known to Murray was about the same as we know sixty years later, but the "details" discovered since include numerous mountain ranges as big as the Alps.

In a second map, Murray shows the distribution of the curious sediments which he was first to discover in the Pacific. These included volcanic and coral mud around islands which were not very different from the muds and sedimentary rocks

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found on continents. However, almost all of the deep sea floor was covered with very fine red clay and oozes consisting of the remains of microorganisms which live in the sea. These sediments were exceedingly rare on the continents and from this evidence alone, it seemed that the deep sea floor was never elevated above sea level and perhaps was eternally quiet and still. How different are the results and interpretations of modern research.

In an earlier report, Murray had described the sea floor relief and sediments of Melanesia which is to be the focus of the *Nova* Expedition. The cumulative effort of British surveys, chiefly along cable routes, is really astonishing for 1906. At that time, the oceanic soundings were so numerous that the 2000 plotted were only a small fraction of those available. There were 1019 measurements of the sea water temperature directly above the bottom and over 700 samples of bottom sediments. Murray was able to describe the general geography as:

extremely diversified, ridges and valleys running approximately in a north and south direction alternating with each other, the valley nearest Australia being the deepest.... This deep valley ... is broken up by several elevations which do not reach the surface of the sea, the latest additions to these elevations being the "Britannia Hills," discovered by Mr. Peake in 1903.

Elucidating the origin of these very ridges and troughs and elevations is a major aim of *Nova*, and we hope to dredge the first samples of the Britannia Hills.

For almost forty years after Murray and Agassiz, little more was learned about the Pacific sea floor. Some samples of sediment were collected by the nonmagnetic ship *Carnegie* before she blew up in Apia harbor in 1929, but the invention of the echo sounder eliminated the opportunity to sample whenever a sounding was taken. Indeed, a large number of the new echo soundings were erroneous and, if anything, the oceanic charts of the Pacific in 1940 were worse than those of 1910. This was because measurement of depth with a wire was replaced by measuring the time it took an echo to bounce off the sea floor. This was tricky even when done by skilled

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— 37 —

scientists and almost beyond the capabilities of the sailors who made many of the measurements as an extra duty while standing watch. A friend of mine once met such a sailor in a bar in San Diego and was told that his normal practice was to ignore the sounder and just record the same depth as the sailor who had been on duty for the previous four hours. Even more conscientious men could rarely do much with mysterious beeps and flashing red lights which were supposed to tell the depth.

The marvels of technology developed by the Navy to look for submarines in the 1940s changed everything, and ships, instruments, and money became available for modern oceanography. The first work in the Pacific, however, was done as a part of three world-circling expeditions by British, Swedish, and Danish scientists, some of whom had used their time during the war to make preparations. The first was the Swedish *Albatross* (once again that wonderful name) Expedition of 1947–1948 headed by Hans Pettersson. Superficially, it seemed a little like the *Challenger* Expedition in that a small number of scientists were on a large and carefully outfitted ship. However, the problems of interest and the equipment to study them were very different indeed. The ship had a special winch with a great tapered wire capable of reaching the deepest parts of the sea. It had a newly invented corer which could plunge a steel pipe 30 to 60 feet into the sea floor, which previously had only been scraped. It had devices to measure the thickness of sediment between the sea floor and the hard igneous rock of the oceanic crust, and the amount of heat flowing through the sea floor because of the decay of radioactive minerals in the interior of the earth. And, wonder of wonders, it had an echo sounder which recorded a continuous profile of depths and thus gave a real picture of the shape of the sea floor. I can still remember the excitement when Robert Dietz and I first projected microfilms of these precious records in 1950. Now just my own collection of such depth recordings would form a continuous strip more than 100 miles long, and our Scripps fleet sometimes records 10 million soundings in a month.

With all these new devices, the Swedish scientists discovered undersea mountains and traced the fluctuations in oceanic

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— 38 —

sedimentation produced by variations in ocean currents during the last million years. They also found that the sediment in ocean basins was by no means as thick as was expected, considering that rivers had emptied into them for billions of years. Moreover, their measurements suggested that the same amount of heat was flowing up through the oceanic crust as through the continents. This was completely contrary to all expectations because the radioactive minerals which produce the heat appear to be concentrated in continents. All in all, the results were extremely exciting for geologists and geophysicists and stimulated work which still continues.

The Danish *Galathea* Expedition of 1950–1952 was headed by another famous scientist, Anton Bruun, whose name now graces a ship dedicated to international oceanographic research and cooperation. The expedition was partially financed by the Carlsberg Brewery in Copenhagen, which had supported research at sea on previous occasions. The objectives were primarily biological and were spectacularly successful. *Galathea* dredged animals from the deepest trenches and off Central America found a "living fossil," an animal of a type which was previously known only in rocks tens of millions of years old.

The last of these three circumnavigating expeditions was British and named *Challenger II*, but unlike its predecessor its primary aim was surveying possible hazards to navigation. One can only shudder at the story that the ship had been presented with a complete set of the great volumes of scientific results of the *Challenger* Expedition and that one day someone threw the lot overboard because the old books were taking up space. Perhaps this was apocryphal; in any event, very valuable measurements were made of the thickness of the crust of various ocean basins, which turned out to be much thinner than continents.

About this time, the general complexion of oceanography in the Pacific, and elsewhere, began to change. No longer was it assumed that each expedition would be followed by years of study of samples before planning began for the next one. Russia and the United States almost simultaneously began a

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truly massive assault on the ocean basins which continues to intensify. From 1949 through 1958, the Russian Institute of Oceanology ship *Vityaz* participated in twenty-five major voyages and the Scripps ship *Horizon* must have done about the same. Considering all the ships belonging to the two institutions, the total number of Pacific expeditions surely exceeded one hundred in a decade, and many other institutions in several nations were also active. Now there are always half a dozen oceanographic ships at sea in the Pacific. In 1960, we visited what we looked upon as a very remote island indeed, taking care to bring magazines and newspapers for the few scientists stationed there. It turned out that we were the third oceanographic ship to visit the island in a week.

Chapter Four: fall 1965-march 1967 Objectives of the Expedition

CHAPTER FOUR The *Nova* Expedition includes many independent scientific experiments on land, sea, and air. Some of the studies are only very broadly related, and describing the reasons for and expectations from each would take an encyclopedia. These programs are part of the expedition only because ships are expensive and we try to make our operations as varied and efficient as possible. However, most of the investigations are parts of an integrated study of a general problem, namely, the origin and geological history of continents and ocean basins. This includes such questions as: Do continents drift about on the surface of the earth? Do continents grow and shrink? Are they torn into fragments by the convective flow of rocks within the interior of the earth? Does the sea floor move from one place to another? And so on. These ideas may seem a little bizarre even to people who know that the elements are transmutable and are prepared to believe that the universe is expanding. I shall have to review some recent geological discoveries in order to explain why they are taken seriously and thus how we hope to test them.

To begin with, we may consider the origin of the continents and ocean basins. In attempting to answer this question, scientists have used their imaginations to the fullest to make up for a paucity of facts. It is known that continents and ocean basins are very different but they float on a common material

called the mantle. The top of the mantle is called the Mohorovičić discontinuity or more usually just the Moho. The deep structure of the crust and mantle is measured by the way shock waves move through it. For example, a hydrogen bomb test can be detected on seismographs all over the earth. The types of waves detected and their time of arrival indicates that in the upper part of the mantle the pressure wave velocity is about eight kilometers per second in most places. The bottom of the continental or oceanic crust is defined as the depth at which this velocity is detected. The thickness of the continents is about twenty miles and the oceanic crust is only about three miles. This is the reason that the Mohole Project planned to drill to the mantle through the ocean floor instead of through a continent. Not only are the thicknesses very different but so are most other things. The rocks of the continents are light-colored and the oceanic ones are dark—a fact which reflects their very different composition. The concentration of heat-generating radioactive minerals in continental rocks is much higher than in oceanic ones, for example. Even the age of the rocks is different. The continents contain rocks more than 3 billion years old, but the oldest rocks found in ocean basins are no older than 200 million years, or less than a tenth as much.

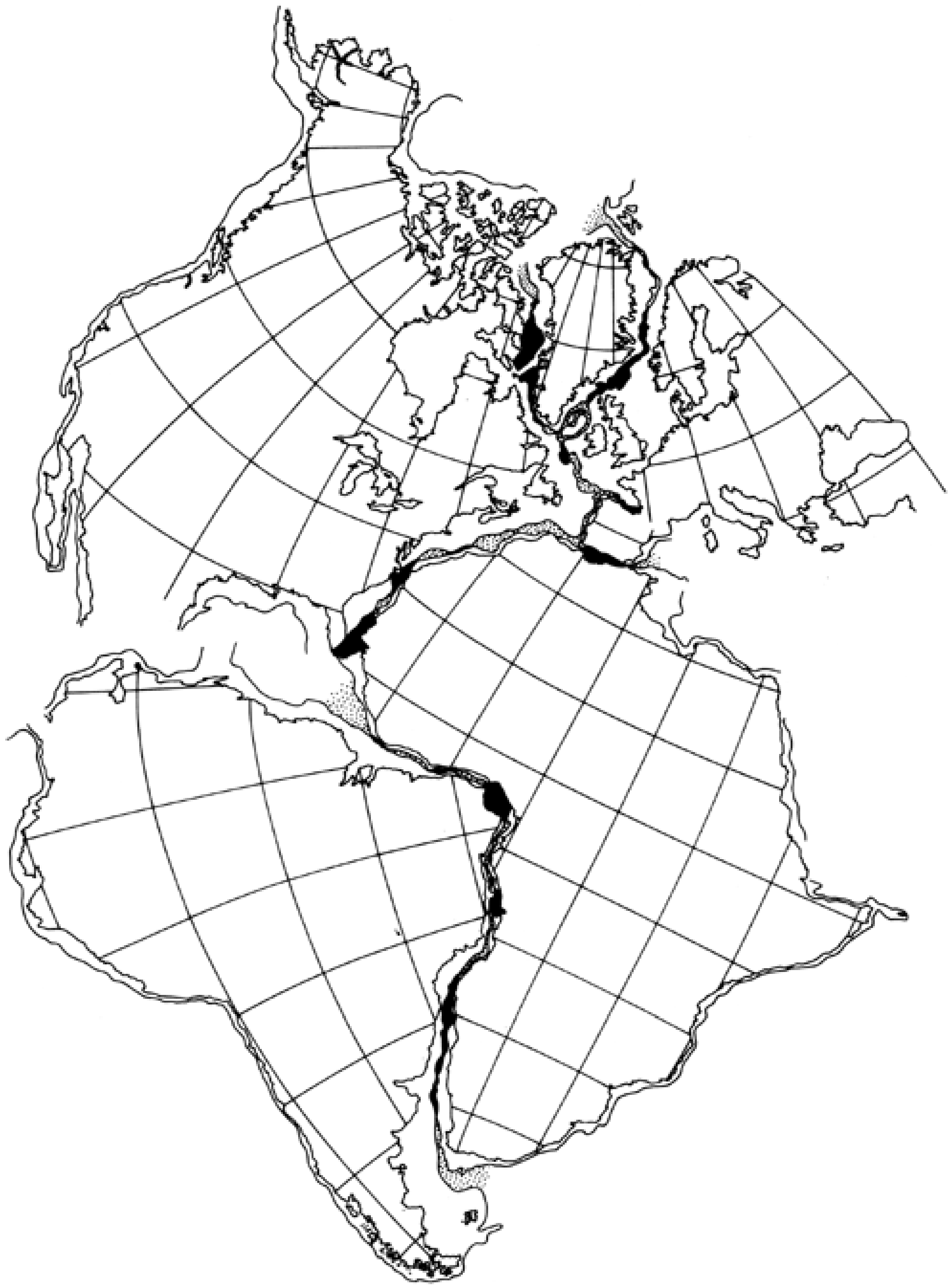
How did these two very different types of material come to be on the surface of one planet? One group of ideas involves celestial catastrophes. It has been proposed that the continents are asteroids which hit an earth which was once all ocean basin. A diametrically opposed view is that the earth was once completely covered with continent and that part of it was pulled away to form the moon or that several parts of it were blasted away by the explosive impact of asteroids. Quite a different type of idea is that the earth was once covered entirely by continent but has been, and is, expanding and that the ocean basins are the opening cracks between continental fragments. Finally, there is the hypothesis that everything on the surface of the earth is the froth produced by gradual migration of low-density material from the interior. This idea has the great virtue that it accounts not only for continental rocks but also for sea water, which is another light-weight

material. Having extracted the materials it is still necessary to sort them into continents and oceanic crust and sea water, but all of geological time is available for the purpose.

At present, no one is sure which of these ideas is correct. All may have had some effect, but which have been important? This is the sort of problem which perhaps will be answered only when we have more information about other planets and the moon.

On a more modest scale we may wonder what has happened to the continents and oceanic crust since they were formed. Many geologists think that the continents have grown. Lava comes from the mantle and volcanoes spread it on the surface of continents. Consequently, new continental material is accumulating even now. Similar volcanic rocks have flowed out for several billion years. If the rate in the past was the same as during historical times, this vulcanism alone could have produced the whole volume of the continental crust and probably sea water as well. There is also a suggestion that the area of continents has increased because the oldest rocks, in North America at least, are in the center and younger ones lie closer to the edge. As we might expect, considering the speculation involved, the alternative idea that the continents are now shrinking has also been suggested. This is one of the possible interpretations of the fact that many mountain ranges simply disappear when they come to the edge of a continent and that many sedimentary rocks seem to have been eroded from mountains located where ocean basins now exist.

Another way to explain this same evidence is to say that continents which are now widely separated were once joined together and thus that the continuations of the mountain ranges, for example, still exist but are now separated by an oceanic gap. This is the famous theory of continental drift proposed in its best-known form by Alfred Wegener fifty years ago. Wegener started with the fact that the Atlantic sides of Africa and South America can be fitted together with hardly any gaps or overlap. This can be seen on any world map. He also noted that very peculiar and distinctive plants flourished in Africa, southern South America, and India at about the same time in the geological past. Likewise, evidence of more



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or less simultaneous glaciation occurred in these places and Australia at about the same time. He argued that a vast southern continent once existed and had broken up and spread apart about 100 million to 200 million years ago. This idea was greeted with an understandable skepticism, especially when it developed that the mechanisms which he proposed for producing

the drift were quite improbable. For forty years his ideas were generally accepted in the Southern Hemisphere, where geologists could look at the evidence he advanced. Acceptance in Europe was mixed. It was minimal in the United States.

Then a new approach developed, based on the discovery that the position of the ancient magnetic pole was preserved in different types of rocks regardless of age. Although the origin of the earth's magnetic field is still somewhat obscure, for the last 60 million years the magnetic poles have corresponded roughly with the poles of rotation. Thus, the magnetic pole positions evident in the rocks probably indicate the approximate position of the geographic pole. The new magnetic data suggest widespread drift of continents and that the various components of Wegener's great southern continent, "Gondwanaland," were very near the southern pole at the time of the glaciation. Many ambiguities were discovered, and doubtful geologists had reason for continuing skepticism, but it was quite striking that the new type of observations strongly supported continental drift as originally conceived on quite different evidence.

In the late 1950s it was discovered that an enormous mountain range lies in the center of most ocean basins and particularly in those basins which separate the supposed fragments of Gondwanaland. The suggestion was rapidly advanced that this mountain range, or midocean ridge system, marked the initial crack along which Gondwanaland had split and from which the fragments had spread. The ultimate confirmation of this idea required large-scale investigations of the sea floor and a gradual understanding of how the midocean ridge system develops. To a certain extent, marine geology during the past two decades has only accidentally provided evidence bearing on this question, which is hardly surprising because the question itself is not that old. In more recent years, however, considerable effort has been spent in trying to answer this question with the happy result that the answer is now known.

How this came about involves a brief history of the course of oceanographic exploration and the development of

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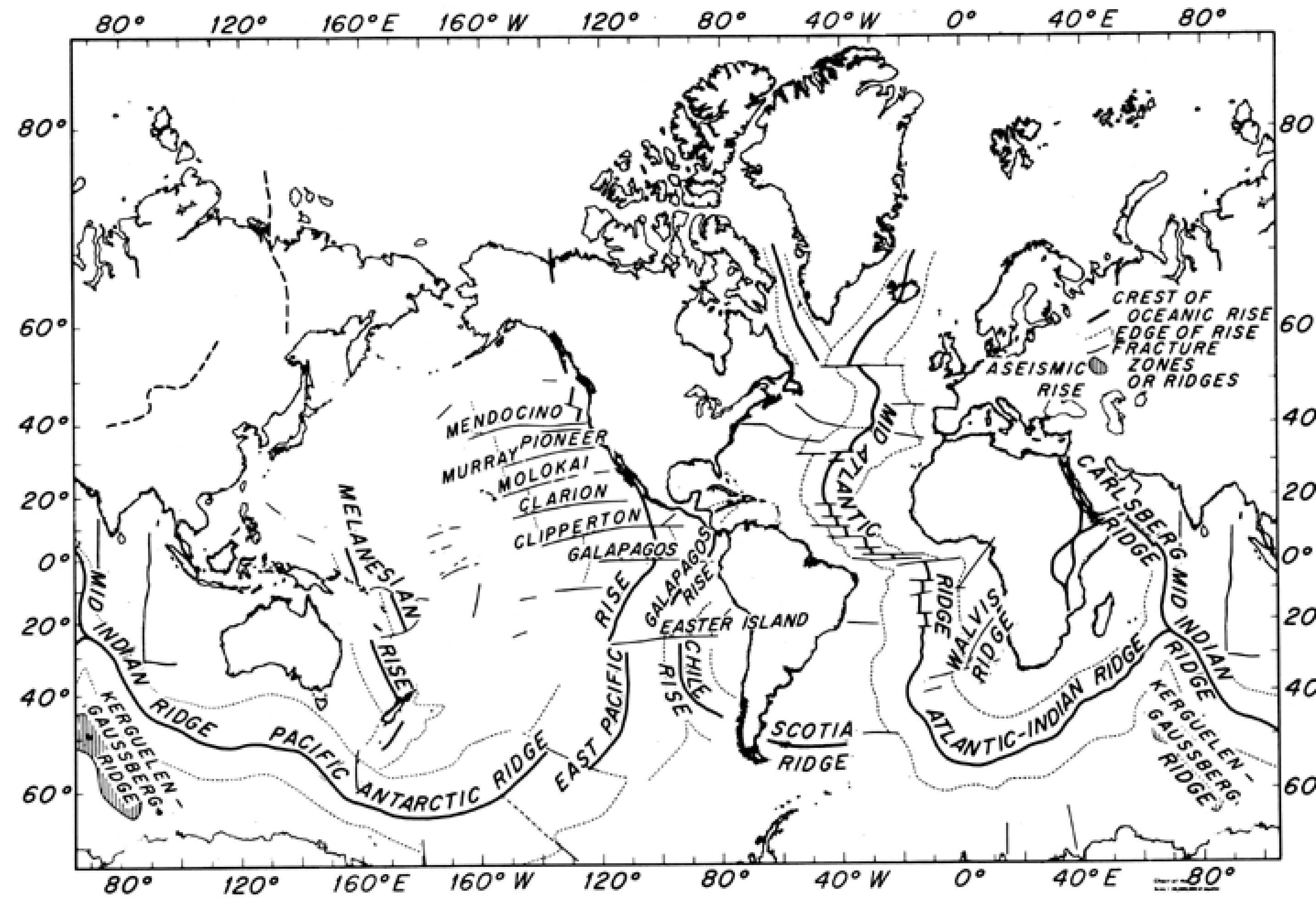
ideas and hypotheses to explain the observations. The first hundred years of oceanographic research contributed hardly anything that is of interest except the discovery of deep sea trenches and parts of the midocean ridge system. In 1940, it appeared that the sea floor was a relatively quiet place with minimal relief and that all mountain building and other important geological processes occurred on continents or at their margins. During World War II came the first breakthrough because Professor Harry Hess of Princeton happened to be navigator and later captain of the *U.S.S. Cape Johnson*, a Navy supply ship plying the western Pacific. He kept the recording echo sounder on his ship operating in deep water where most ships turned them off. The result was the discovery of deeply submerged volcanoes with flat tops which he called "guyots" after Guyot Hall where his office is situated at Princeton. He immediately made the correct interpretation that guyots are truncated volcanoes which had been eroded flat when they formerly were islands, even though they are now at depths as great as a mile below sea level. It was clear that the sea floor was far more active than anyone had believed.

Shattered beliefs soon became a commonplace as it developed that almost everything supposed about ocean basins was wrong. The crust under ocean basins is not as thick as continental crust but in fact is much thinner and also astonishingly uniform. The sediment in the basins is not a mile or more thick as anticipated from the known rate of erosion of continents. Instead it is only a thousand feet thick, which is the amount of sediment that would accumulate in only a few hundred million years rather than in the total age of the earth. Moreover, no really ancient rocks can be found in the basins comparable to the three-billion-year-old rocks of the continents. At first, this was attributed to inadequate sampling but as the number of bottom samples continued to mount, the suspicion grew that ocean basin rocks are all relatively young. The midocean ridge system was found to be the locus of many earthquakes and a great trough was discovered running along the crest of the Mid-Atlantic Ridge. Soon a belt of unprecedentedly high heat flow was found along the crest of the midocean ridges.



Figure

All of these features were accepted as evidence of modern mountain building and the supposedly quiet ocean basins began to emerge as the very focus of global deformation. The largest faults on the surface of the earth were discovered in the Pacific, features thousands of miles long which follow straight lines even when plotted on a globe. Similar faults were soon found extending across the Atlantic ocean basin and indeed everywhere on the flanks of the midocean system.

— 50 —

Patterns of magnetic anomalies were discovered in the northeastern Pacific which have the peculiar properties that they are perpendicular to the great faults and are offset along them. That is, a distinctive anomaly can be traced to a fault and then is found again on the other side but displaced by a great distance. The maximum displacement is 700 miles. A comparable discovery on land would be that a road in California would stop at a great crack and that when this crack was traced to the east, a continuation of the road would appear in Utah. No such offset has been found anywhere on the continents and, once again, the sea floor assumed more importance in geological thinking.

As oceanographers made these discoveries, they proposed explanations but they were very specific—the number of explanations was at least equal to the types of facts and no common unifying hypotheses were proposed. After a while broader syntheses were attempted and they succeeded in explaining large groups of facts. One such idea, that the earth is rapidly expanding, could explain young ocean basins, thin sediment, and the position, topography, and earthquakes of the midocean ridge system. Another idea, mantle convection rising and spreading from the centers of the midocean ridge system, offered an explanation for most of the same features and also for the orientation and length of great oceanic faults, the surprising equality of average continental and ocean heat flow, the high heat flow near the crest of rises and ridges, and the tendency for large regions of the sea floor to sink.

However, it remained for the same Harry Hess who discovered guyots to propose a truly comprehensive hypothesis which explained just about everything known, and as it turned out, was capable of explaining many things discovered later. Hess rarely goes to sea on an oceanographic ship any more and thus this is a classic example of a broad synthesis created by someone who is not involved with the trivia which blind people who are involved with day-to-day collection of data. The example is highly exceptional, however, because the synthesizer was also a pioneer data collector in the same field more than two decades earlier.

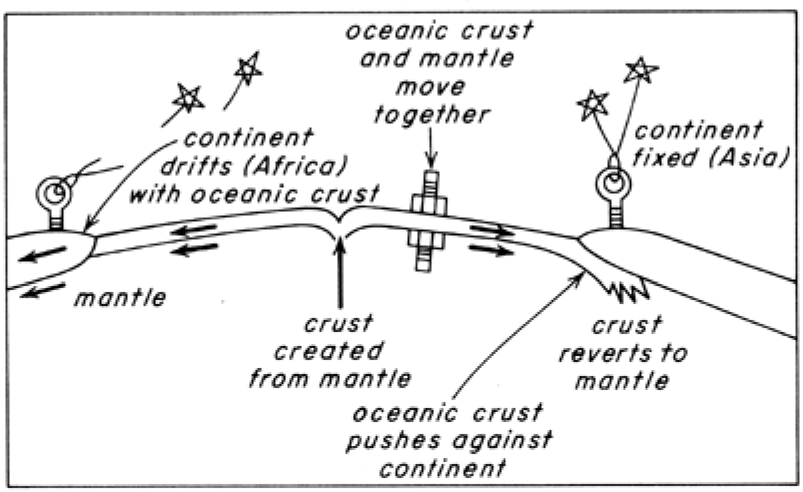
Hess proposes that convection currents in the mantle rise

— 51 —

under the midocean ridge system and spread toward the margins. This is a concept that he and others have been developing for several years and which could explain a variety of major features of the ocean basins. The inspiration that he added was that the oceanic crust is not merely stretched over midocean ridges but is actually created there. Hess is interested not only in marine geology but also in the origin and composition of the little-known rocks of the mantle.

His investigations have led him to the conclusion that the upper mantle is probably made of peridotite, a dense rock rich in magnesium. It is commonly found at the surface of continents in places which were once deep in the crust and which have been elevated and eroded. It also occurs as fragments in lava flows which have come from the mantle. Thus, it appears that it is an important constituent of the interior of the earth. This peridotite has the interesting property that in some circumstances if it combines with water at a temperature below 500°C, it forms a new rock, serpentine, and that if reheated the water is driven out and it again is peridotite.

Hess conceived that peridotite and water in the mantle convect upward along lines under the ocean basins and elevate the midocean ridge system. As the mantle moves upward, it cools and passes through the level which is at 500°C, and the water and peridotite combine and form serpentine. Laboratory tests indicate that whereas peridotite transmits earthquakes with the velocity of the mantle, serpentine transmits them like the oceanic crust. Consequently, Hess had a mechanism for producing crust from mantle as a consequence of the convection which was already suggested by many types of evidence. But now he could explain many new things. The oceanic crust has its astonishingly uniform thickness because it is formed above a certain temperature which occurs at a uniform depth. The crust can move very large distances without leaving central gaps because new crust forms at the center of spreading. It does not pile up in great thicknesses at the edges of the ocean basins because as it is pushed down below the 500°C level, it is reconverted to peridotitic mantle. Continents do not drift through the mantle but merely float on it as it moves away from rises and ridges.



The sea floor spreading hypothesis.

The sea floor spreading hypothesis.

The theory can also explain many other things provided the duration of the most recent mantle convection is known. The evidence available is rather tenuous but it suggests that most of the sea floor has been produced in this way during the last few hundred million years. For example, the Atlantic Ocean apparently was opened by continental drift during that time. If so, the oceanic sediment is thin because it is sitting on a young crust and has not had much time to accumulate.

This does not mean that all ocean *basins* are necessarily very young but merely that older sediment has been skimmed off and plastered against the continents while older crust has been reconverted into mantle. The elimination of older crust likewise explains why no old rocks are found in ocean basins no matter how many times they are sampled. The older rocks have been moved out of ocean basins and either pushed into continents or pulled down into the mantle where they cannot be dredged.

Although an impressive display of imagination, this idea, soon named the "sea floor spreading hypothesis" by Robert Dietz, did not quickly gain acceptance. Geologists and geophysicists on average are as conservative in their own fields as any other group. It is hard enough adjusting to a growing flood of new work and ideas which surely are proven without also trying to understand the implications of everything which is not proven. The sea floor spreading hypothesis was not the

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only explanation for each of the things it explained, and it did not seem to agree with the details of many observations. Acceptance required some precise confirmation which was incompatible with all other ideas about the sea floor. An even slightly ambiguous confirmation would not do. When I was a student, Professor Kirk Bryan at Harvard once told me, "Sometime you may be lucky enough to get a revolutionary idea and prove it is right, but don't expect that it will be immediately acclaimed. Students may accept it but you will have to wait until the older geologists die off before everyone agrees it is right."

The confirmation of sea floor spreading came from the proof of a really outlandish corollary. Fred Vine and Drummond Matthews, respectively a graduate student and young instructor at Cambridge University, suggested that the spreading sea floor is a tape recorder. About the time Hess published his hypothesis, it was being established that the magnetic field of the earth is periodically reversed. Certain iron-rich minerals in solidifying volcanic rocks become oriented in the direction of the earth's magnetic field. When the rocks cool through the Curie point the orientation of the magnetic minerals is fixed and cannot readily be altered without reheating. Thus by measuring the direction of magnetization of the minerals in an ancient rock it is possible to determine the location of the magnetic north pole at the time the rock cooled. When the position of the pole was studied in detail in a given locality, it appeared that the north and south magnetic poles occasionally reversed. This was rapidly explained in several different ways and the significance of the observations was obscure. The field measurements continued and more effort was made to date the time of reversals by newly refined methods of measuring rock ages by the decay of radioactive elements. The origin of the reversals was established in an elegant way when it was gradually proved that the reversals occurred at the same time in widely separated places. That is, there were several distinct and worldwide times of reversal which could only mean that the whole magnetic field had reversed during a period of a few thousand years. It is interesting to speculate how navigation would have been affected if the field had been reversing,

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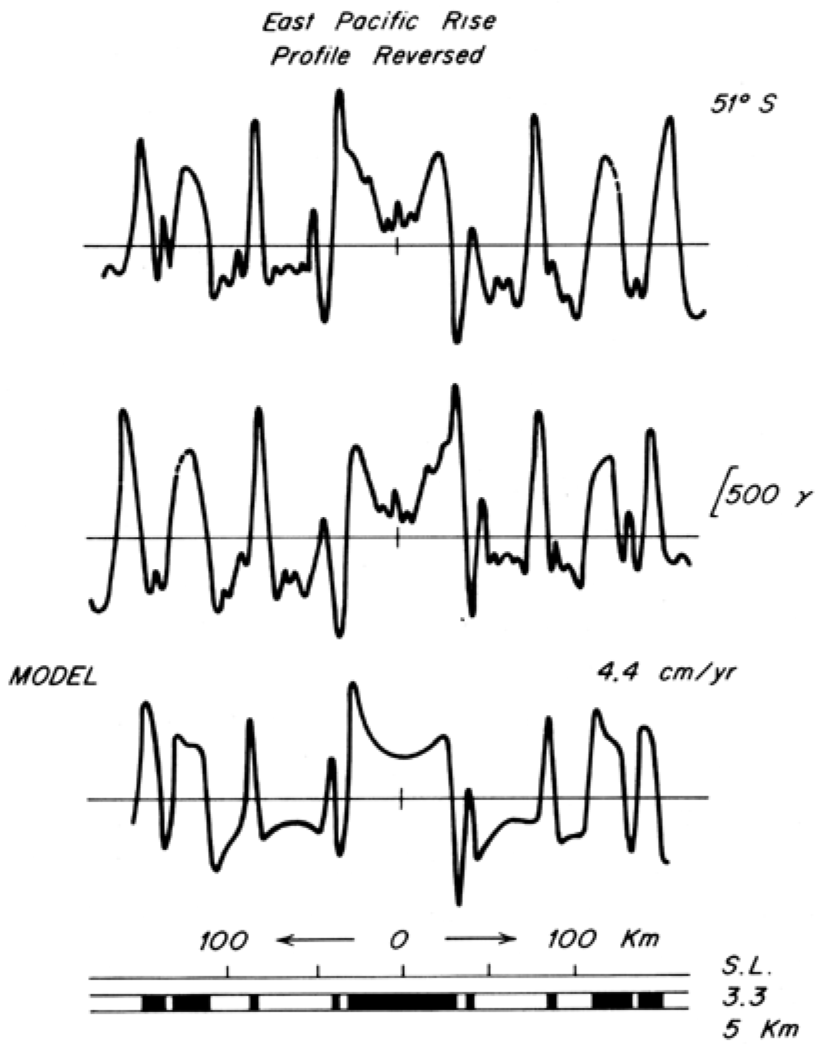
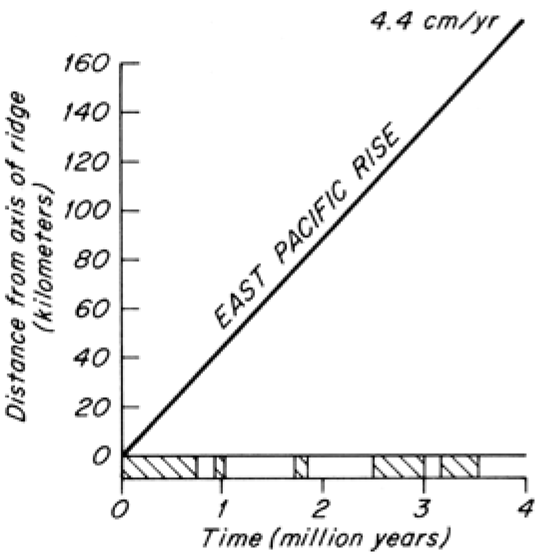
as it will again, during the last 1000 years. If the reversal occurs in jumps, or if it starts and stops, there would have been a lot of lost sailors and much more actuarial and scientific interest in magnetism.

Vine and Matthews reasoned that the long parallel magnetic anomalies characteristic of the oceanic rises and ridges are produced by reversals of the magnetic field combined with sea floor spreading. The lava that pours out and cools in the center of a rise preserves a record of the orientation of the magnetic field at that time. Spreading splits the solidified lava and moves it to the flanks of the rise. Meanwhile, the widening gap is again filled with cooling lava. If the magnetic field reverses during this period, the newer rocks preserve a record of a reversed field. Field measurements of the present total intensity of the field would show a strong field where the present one is reinforced by an ancient field with the same polarity and a weakened field where the modern and ancient field are in opposite directions. A tape recorder works in the same way, preserving the intensity of a varying magnetic field to which the moving tape is exposed.

Within a few years two new facts about the long parallel magnetic anomalies were discovered and both agreed exactly with the tape recorder hypothesis. First, it was found that the anomalies, which have quite distinctive shapes, are symmetrical around the centers of rises and ridges; in short, the sea floor is a *stereo* tape recorder with almost identical volcanic rock "tapes" spreading out in enormous sheets from the center. This was beyond the fondest expectations of Vine and Matthews because no one ever conceived of the enormous mountain-building deformation of the earth behaving in such a remarkably orderly manner. In any event, the idea of the moving crust preserving a history of reversals of the earth's magnetic field is the only one that offers a reasonable explanation for the symmetry. The other discovery was that the distance between the sea floor anomalies is generally proportional to the spacing between the times of reversals of the magnetic field measured on land during the last 3 million years. Consequently, particular sea floor anomalies can be paired with particular reversals. This not only reconfirms that the anomalies


East Pacific Rise
Profile Reversed
51° S
500 γ
MODEL
4.4 cm/yr
100 ← 0 → 100 Km
S.L.
3.3
5 Km

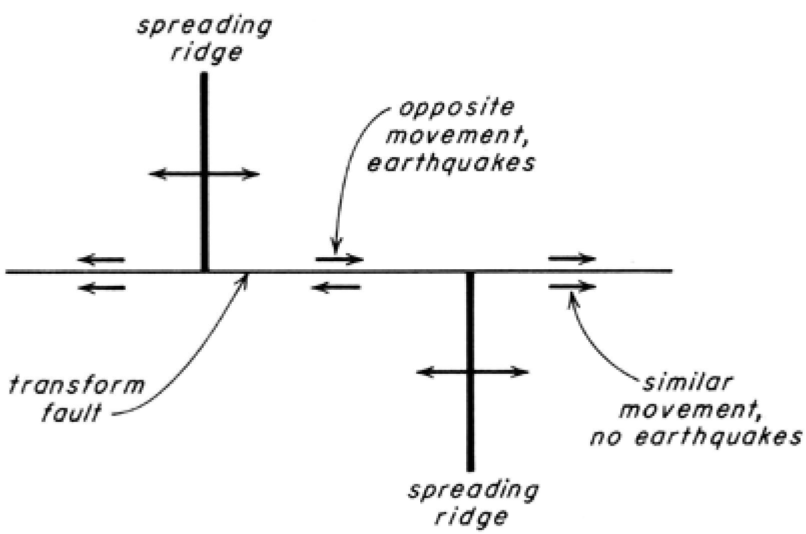

4.4 cm/yr
EAST PACIFIC RISE
Distance from axis of ridge (kilometers)
160
140
120
100
80
60
40
20
0
0
1
2
3
4
Time (million years)

Figure

— 56 —

are produced by reversals but gives the speed at which the "tape" is moving and shows that it moves at a relatively constant speed for long periods. The sea floor is spreading away from the center at about one-half inch per year in the North Atlantic and almost two inches per year in the South Pacific. These are about the right rates to open up the whole Atlantic Basin in the time since Africa and South America were apparently joined together.

The origin of these magnetic anomalies confirmed Hess's hypothesis of sea floor spreading and numerous other confirmations followed in short order. J. Tuzo Wilson of the University of Toronto observed that the large earthquakes along the great sea floor faults which offset the center of midocean ridges have a very peculiar distribution. The faults are thousands of miles long, but the earthquakes generally occur only between the offset centers of the rises. He reasoned that if the sea floor is spreading from each of the offset centers, this distribution can be explained. Between the centers, the oceanic crust on the two sides of the fault is moving in opposite directions, and this causes earthquakes. Beyond the centers, however, the crust on both sides of the fault is moving in the same direction and no earthquakes occur. If this is correct, it means that the motion on the faults is in the opposite direction from the indicated offset of the centers of a rise. He assumed that the faults are very old and that the younger rises and ridges were offset



Figure

— 57 —

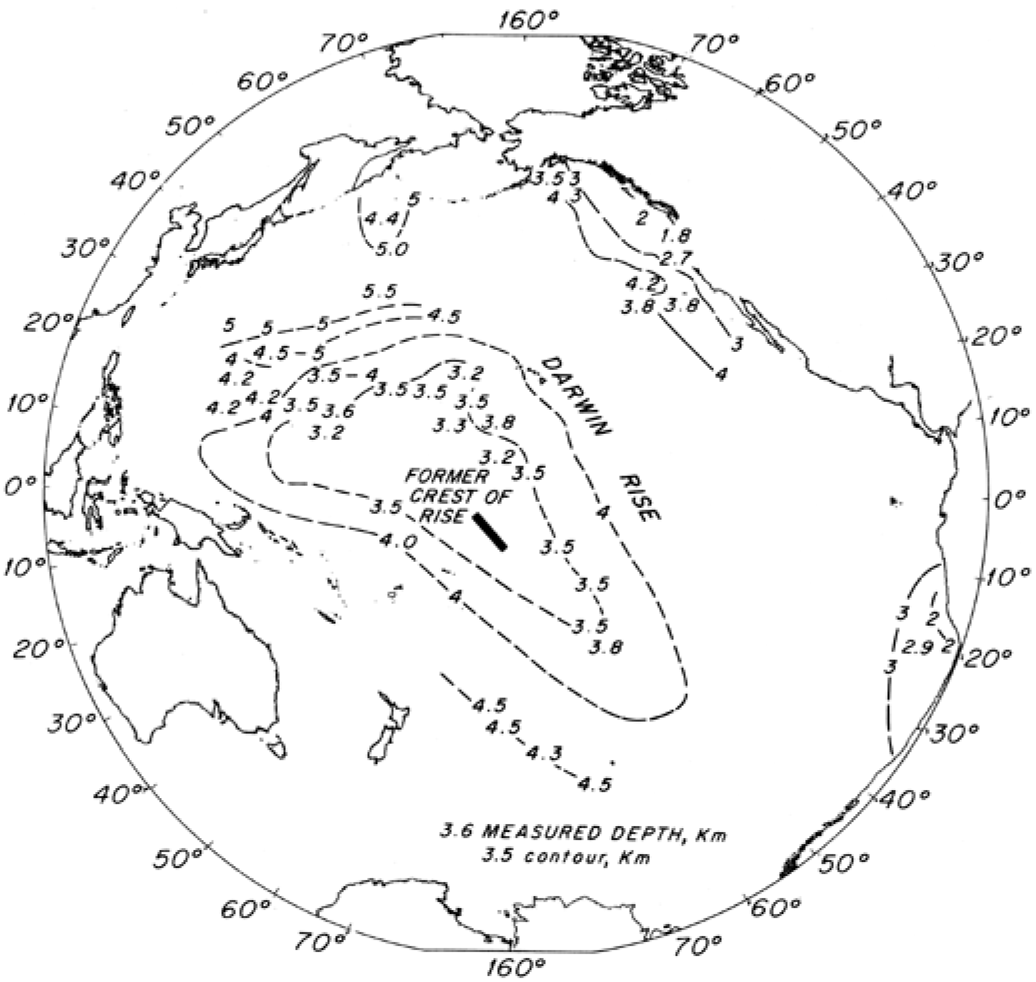
when they were created by convection. He did not attempt to explain the origin of the faults or the offsets, merely the earthquakes. His prediction of the direction of motion of the crust was subject to testing because the first motion on earthquakes can be measured by studying the motion they produce in instruments on land. Lynn Sykes of Lamont Geological Observatory quickly proved that Wilson was right and, since the prediction assumed sea floor spreading, also proved that Hess was right.

Shortly everyone got into the act. Vast quantities of oceanographic observations were available to evaluate the reality of sea floor spreading. The thickness of sediment increases at greater distances from the centers of spreading. Although no fossils in ocean basins are very old by geological standards, the oldest ones occur at the margins of parts of the sea floor that have spread, and younger and younger fossils are found as the centers of spreading are approached. There is even a correlation with the rate of spreading. The solidified lavas are thick where spreading is slow and they are thin where it is fast. Likewise, mountainous ridges and a central rift are associated with slow spreading and low rolling hills with fast spreading. Most of the topography of the sea floor, like the magnetic anomalies and crust, is formed at the center of a rise or ridge and then spreads to one side or the other.

These correlations and confirmations of sea floor spreading have appeared very rapidly and are still coming. As I write this in preparation for the *Nova* Expedition, many of these discoveries have yet to be published in the scientific literature. Thus we go to sea in a time of great intellectual ferment and excitement. What remains to be done? For one thing, we have before us the opportunity to prove that more than one period of convection has occurred during the history of the earth. This is a particularly important point because our present knowledge about sea floor spreading is concerned with only the last small fraction of geological time. We need quite different information to tell whether the same process has occurred again and again, sweeping the sea floor clean and causing continents to drift about during the last several billion years.

The most favorable place to look for such evidence is in the region between Hawaii and the southwestern Pacific which we shall be traversing repeatedly during *Nova*. In this region it appears that about 100 million years ago the sea floor was elevated into a broad oceanic rise which has since slowly subsided. Charles Darwin first observed that this region had subsided, and consequently this broad feature is called the Darwin Rise in his honor. Since Darwin's time, Hess and various following oceanographers have found and dredged numerous drowned ancient islands in this region. The few that are sampled were islands at about the same time.

The load of these ancient islands cracked the surrounding crust and enormous volumes of volcanic rock poured out of the cracks. Then the rise sank. But was it really a rise like the existing ones, and will the Mid-Atlantic Ridge and the East Pacific Rise sink like the Darwin Rise? It is now clear that one



Figure

of the most powerful geological tests for the former existence of a rise like modern ones is the discovery of long parallel magnetic anomalies distributed symmetrically around the supposed ancient center of the rise. A few magnetic profiles across the Darwin Rise indicate anomalies with the right shapes and sizes. In one small area they are at least slightly linear. Our ship tracks are laid out to fill in gaps and to try to see whether there is an integrated, symmetrical anomaly pattern. If found it will be very important because almost inevitably it will be older than the patterns associated with the sea floor spreading of the last 100 million years. This is not certain because we do not know for sure that the present period of spreading has existed for only 100 million years—but it seems probable. Since the Darwin Rise started to die and sink at about that time, its magnetic anomalies would be older. If we are very lucky we shall find that the magnetic anomalies are symmetrical and that the ones in the center of the Darwin Rise are the same as those on the outer flanks of the existing rises and ridges. Then we can be sure that the ancient and modern rises have the same development and see how they are related. We shall also know whether rises are ephemeral and may have existed at different times in the past.

Of course, we cannot expect too much of six profiles measuring magnetic anomalies, and we may not be much further along toward solving these problems next fall than we are now. If so, we shall have to return. One of the complications making it difficult to map magnetic anomalies is that they are so commonly offset along perpendicular faults. If the faults are not located, it is difficult to determine the trends of anomalies. Unfortunately, it is not easy to map faults and anomalies at the same time because they are mutually perpendicular. If we go in one direction, we cross anomalies but not faults; and if we go in the other, we cross only faults and run along the anomalies, which is not much help. Some faults are already mapped, however, and they will help us determine what has happened and where to explore. The faults themselves are not without interest because they are so long and straight, and in some places they have formed very deep troughs. One just east of the Phoenix Islands is almost 25,000 feet deep and the

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bottom is far down in the oceanic crust. By dredging the cliffs on each side of this trough, we hope to get samples which in most places would be obtainable only by very expensive drilling through the unfaulted crust. Thus, if we are successful and the wire does not break and fresh rock is exposed on the cliffs, we shall know the composition of the crust and see whether Hess is right in his prediction of serpentine, as he has been right in so many other things.

The sea floor spreading hypothesis will also provide a guide for our studies of the southwestern Pacific, and in that region we shall have information from islands to guide us. In fact, the great peculiarity of Melanesia is that it is a jumble of large continental islands and small ocean basins on a scale which exists nowhere else on earth. The Pacific edges of Melanesia generally are marked by "island arcs," which is the geological term for chains of volcanic and uplifted coral islands bordered by a very deep oceanic trench and with associated earthquakes. These island arcs are different from others around the margins of the Pacific. The Tonga-Kermadec "arc," for example, is straight except at the northern end where it curves sharply to the west. In other respects, it is relatively normal in that the trench is on the Pacific side of the islands and the earthquakes dip from near the surface below the trench to about 400 miles below the sea floor west of the islands. Dipping planes marked by earthquakes of this sort occur only below island arcs or continental equivalents, and they are usually intepreted as faults on which the islands are being pushed over the trenches. Most dip away from the Pacific, but in this respect, the island arcs of northwestern Melanesia are peculiar. The New Britain, Solomon, and New Hebrides arcs are linked in a wavy chain. The trenches next to these arcs are all on the side away from the Pacific Basin and the fault planes all dip toward the basin. Everything is backward, and the cause for this is one of the puzzles that besets us.

Many puzzles exist regarding the small faults at the top of the crust around island arcs, and these will be studied to see what they reveal about the existence of deep thrusting and the formation of trenches. The filling of trenches by sediment is also little understood and will be elucidated by mapping

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and sampling the small basins filled with sediment on the sides and in the bottom of the trench. We shall also measure the heat flow to see how it varies from place to place and take photographs to try to find bare rock to dredge. Very peculiar rocks, which appear to be typical of the lower parts of the crust, have been dredged in other trenches and may occur here exposed in the walls of the trench.

Between the island arcs and Australia are the small continents and ocean basins of prime interest to *Nova*. The islands of New Zealand, the Fijis, and New Caledonia have been mapped by geologists and are relatively well known compared to the sea floor. They are considered to be little continents because they are above sea level and because they consist of rocks which occur in typical continents but not in ocean basins. Most of the rocks are produced by weathering and redeposition and sometimes reheating of fragments derived from older volcanic outpourings and intrusions of igneous rock. What are the sources from which the sediments were eroded? The geologists who have mapped Fiji are inclined to believe that the sources were all local and that erosion and redeposition have merely shifted sediment from one place to another on the islands. All the rocks in Fiji seem to be relatively young, and shifting them about does not seem unreasonable. New Caledonia is much more puzzling. Many of the rocks are much older, and they were deposited in an elongate submarine trough which was at least as extensive as the present island. Where did the sediments come from to fill the trough, if the present island was all under water? New Caledonian geologists tend to call on a former bordering mountain range which has now disappeared. There are two possible positions for this former range. If New Caledonia has not moved, then the range has been changed into the deep ocean basin which is now west of the island. However, continental drift is another possibility. Perhaps New Caledonia was once next to Australia or perhaps next to a supposed continent midway between the present positions of Australia and New Caledonia. New Zealand is more like New Caledonia than Fiji, and many geologists think that it, too, derived sediment from a source which has sunk or drifted away. The concept of one or

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more sunken continents in Melanesia has been stated repeatedly and for convenience I shall merely refer to a continent "Melantis," which not only sounds similar to Atlantis but has some of the same connotations of doubtful existence.

In a few months we can hardly expect to do much productive work on these islands because it has taken many years to learn the little that is already known.

Mostly we shall try to solve the problems of the land by looking at the sea floor between islands. This may seem rather indirect, but it has been a revealing approach in other regions. We shall map the topography, measure the thickness of the layers of the crust, measure variations of the magnetic and gravity fields, determine heat flow, and obtain samples of sediment and rock. Hardly anything is known about the Melanesian ocean basin because of a peculiarity of oceanographic exploration. The general topography and sediment distribution were discovered early because of the necessity of laying submarine telegraph cables. Since the development of modern marine geology, however, the region has largely been skirted. It is extremely distant from the older Pacific oceanographic centers in the United States, Siberia, and Japan; and the newer centers in New Zealand, Australia, and New Caledonia are only beginning to develop the necessary technology. Fortunately, a special international effort was made to study the even more remote Indian Ocean a few years ago. Much was learned along the tracks of ships going to and from the Indian Ocean. Even so, Melanesia was almost completely neglected because the ships had to go either north or south of Australia and Melanesia is, so to speak, in a lee of the continent. All we have are a few intriguing clues regarding what we may find.

We know more about the bottom topography of Melanesia than anything else, but we do not know enough for our purposes. The older spot soundings simply do not give the same kind of information as modern, recorded echo soundings. Older methods yielded a generalized map. New ones produce a continuous picture of the sea floor under a moving ship. The difference in what can be learned is about the same as the difference between looking at the moon with the naked eye and taking pictures of it with a rocket which flies just above it. We have

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to know exactly what the bottom looks like in order to plan all our other work because everything depends on the depth and relief, whether mountainous or smooth. Mountains are interesting because they indicate the nature and orientation of the deforming forces which produced them. Mountains can be formed by vertical uplift or horizontal squeezing or stretching, and each type looks somewhat different. The smooth plains are also interesting because they result from burial of hills by sediment or very fluid lava flows. Near continents almost all smoothing is by sediment which flows out from rivers and beaches, moves along the sea floor, and comes to rest in deep basins and troughs. Such flat-floored basins occur east of Australia and New Guinea, south of Fiji, and in various directions around New Zealand. The movement of sediment described is not unreasonable because it is all downhill and thus aided by gravity. In fact, the deposition of sediment on the steep submarine slopes around continents might seem unlikely, but it has occurred in the Coral Sea southeast of New Guinea. For some reason sediment has collected in and filled an arm of a basin between New Guinea and the deep sea floor instead of continuing to move downhill. We hope to find out why. One possibility is that the deep basin has been created by continental drift since the arm was filled. That is, the area itself was the deepest place available when it was filled. We can get some indications of what happened in the past by studying what is happening now. For this purpose we shall collect sediment from the rivers and beaches of New Guinea. Different rivers drain different source regions and hence may transport quite different and distinctive minerals. We shall take sedimentary cores of the Coral Sea floor and compare the minerals obtained with those from the rivers. This may show from whence and how the abyssal sediments are deposited.

With the echo sounder we can also try to find flat-topped submarine volcanoes, guyots, like those first discovered by Harry Hess in the central Pacific. A few have already been found in the deep Tasman Basin east of Australia. They lie along a straight line about midway between the Australian coast and the Lord Howe Rise, and their history, can we but discover it, will tell us much about what has happened in this

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part of Melanesia. The Lord Howe Rise itself is strikingly flat on top in an area 60 miles wide and more than 100 miles long. Even though it is now a little more than half a mile below sea level, this great area may have been planed flat by waves and rivers if it was above sea level at some time in the past. Extensive truncation on this scale has occurred in the region east of New Zealand which is now at a considerable depth. Another possibility is that the Lord Howe Rise was once mountainous and that the valleys or troughs have been filled with sediment. Shallow areas of the sea floor in warm equatorial waters typically are covered with microscopic shells of Foraminifera, which are animals that are very abundant in surface waters. When they die, the shells fall to the sea floor but are dissolved in very deep water and preserved only on highs.

We shall have no difficulty distinguishing a blanket of sediment from truncated rock on the Lord Howe Rise because we shall make continuous profiles of the upper layers of the sea floor. This is done with instruments like echo sounders but more powerful. A loud noise can be generated in the sea in many ways: by explosives, by electrical sparks, by igniting blobs of natural gas, or by compressed air bursts. We use an electrical spark for the gear on *Horizon* and compressed air on *Argo*. This is because these sub-bottom profilers, now abbreviated to "profilers," are still in experimental development and one type is better than the other, depending on the detail desired. The chief problem with profilers is that the listening hydrophones are affected by the noise of water moving past them, and this means that ships cannot go at full speed. Considering that ideal speeds for profiling are only 4 to 6 knots, it costs a lot of ship time to obtain the records. We are now conducting experiments which may enable us to go faster and thus do more profiling. These newly developed profilers give records that oceanographers find almost miraculous. In the past, we had to deduce the thickness and structure of sediment in most places from the details of the surface topography as revealed by echo sounders. Now when everything is just right we can see the layers far below the bottom, watch them thicken and thin, and see whether they are flat or have been folded or faulted and then buried. With these marvelous instruments

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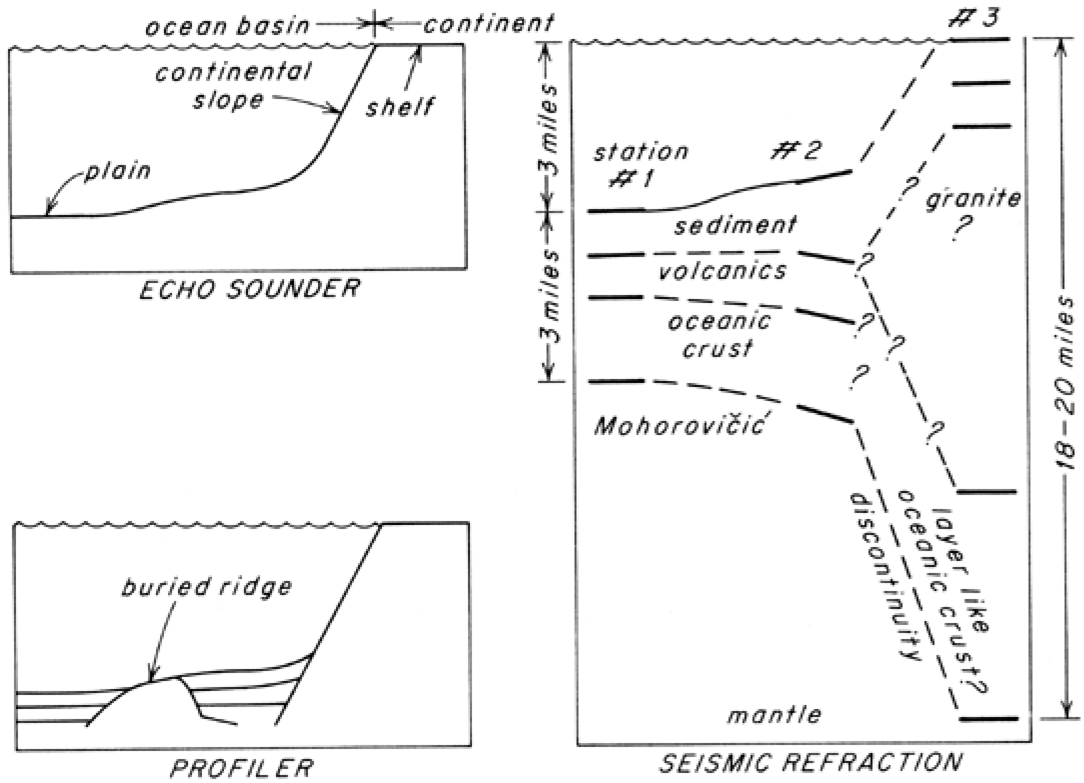
we shall be able to identify places where the sea floor in Melanesia has been deformed recently, where thick undeformed sediment is accumulating, and tell where it is coming from, because the layers usually thicken toward the source.

By measuring the thickness and layering of the crust we hope to establish whether its general character is that of a continent which is being destroyed or an ocean basin which is being built up into a thick continent. This is done by setting off explosions in the ocean and recording the times at which they are detected on a ship or at several floating buoys. There are few precedents to guide us, but that is one reason the region is interesting. The velocity at which explosive-generated waves move through typical continental crust is less than through typical oceanic crust. Consequently, unusually thin continent may not be the same as unusually thick ocean crust. The layering might also differ depending on the history of the upper crust. Thick mud on an old oceanic crust would not resemble the hard sedimentary and metamorphic rocks characteristic

The sea floor and the crust below as seen with instruments with different power and resolution.



The sea floor and the crust below as seen with instruments with different power and resolution.

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of the surface of continents. Unfortunately, as my colleague, Al Engel, remarks, "Setting off explosions is not a very sensitive way of analyzing rocks." At least, it will give us a general picture of the structure of the crust which cannot be obtained in any other way.

Variations in the intensity of gravity from place to place result from variations in the thickness and density of the underlying rocks. Geophysicists learn early that gravity observations can be interpreted in an infinite number of ways if nothing else is known. Given the discrete measurements of crustal structure by explosive seismology, however, the interpretation of gravity can be quite informative. The gravity interpretations can be adjusted to agree with the seismic data. This might seem rather pointless and would be if nothing else could be achieved. The value of gravity measurements is that they can be made continuously while the ship is underway between seismic stations, which are limited in number by time and the amount of explosives that the ships can carry. If all goes well and the weather is not too rough for sensitive instruments, we shall have continuous profiles of gravity which can be equated with some confidence to continuous profiles of the crust of Melanesia.

Profiles of magnetic intensity are obtained by towing a magnetometer behind each ship at a distance such that it is unaffected by the steel hull. We shall be seeking the distinctive local magnetic anomalies which have elsewhere demonstrated sea floor spreading. Pre-expedition observations have been made in Melanesia by the U.S. Navy in Project Magnet, using low-flying airplanes. These indicate local magnetic anomalies, which is promising, but flight lines are too few to show whether the anomalies have the characteristic elongate shapes produced by sea floor spreading. If the anomalies are elongate and parallel we shall look for symmetry and centers of spreading. We shall also try to identify the identical sequences of anomalies already found in other places where spreading has occurred. This will give some clues regarding the time of Melanesian spreading. We already have some ideas about where to look for spreading because of the possibility that the

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large continental islands have drifted. If New Caledonia drifted away from Australia, symmetrical magnetic anomalies may exist in the oceanic crust between them. They may be anticipated for the same reason between Australia and New Zealand. Another likely prospect is that the Lord Howe Rise and Norfolk Ridge are associated in some way with linear magnetic anomalies because they lie in a region with strikingly linear topography. Moreover, New Zealand oceanographers have already shown that the southern part of the Norfolk Ridge has a large positive magnetic anomaly. A last prospect is that the Fiji Plateau is part of a small oceanic rise which is actively spreading and will show typical magnetic anomalies. The evidence is unclear, but the plateau is relatively shallow and has most of the identifying characteristics of an active oceanic rise, so I have great hopes that we shall find it is one.

Heat comes to the surface of the sea floor from deep in the interior of the earth. I do not mean that the abyssal sea floor is warm; on the contrary, it is very close to freezing. But if we plunge a long probe into the bottom sediment and measure the temperature at different points along the probe, we find it is warmer the deeper we go. If we also measure the thermal conductivity of the sediment, we can calculate the flow of heat moving upward from the interior. Were there no complications and if all the heat came from great depth, these measurements would tell us where the mantle is hot and rising and where it is cold and sinking. Such information would be very helpful in trying to establish the origin of the small crustal blocks of Melanesia and whether they are moving. Unfortunately, there are complications. If sediment accumulates rapidly, the heat flow seems low. If a measurement is made on a hill, heat flow seems low and if in a valley, it seems high. Consequently, measurements must be numerous and at carefully selected sites for meaningful interpretation. Existing measurements suggest that heat flow is relatively normal in northwestern Melanesia and relatively high elsewhere in the region. It remains to be seen if these relationships are still true when we have a hundred measurements instead of twenty. If there are centers of sea floor spreading, we may expect to find that

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heat flow is high along the center line. If the Lord Howe Rise is a part of Melantis it should have normal continental heat flow; if it is an ancient oceanic rise, heat flow may be low.

Having examined the sea floor with every type of modern instrument, we can also fall back on the oldest method of doing geology, namely, taking samples. We do this in several ways: by photographs, cores of sediment, and dredge hauls of rock. Modern deep sea cameras can be located accurately a few meters above the bottom and can take several hundred pictures as the ship drifts along. The photos are developed immediately and provide a guide for other types of sampling. By themselves they show such things as whether ocean currents are intense at the bottom; whether animals and animal borings are abundant; and whether the bottom is paved with nodules of manganese oxide or phosphorite, which are minerals of potential economic importance. Cores are a few inches in diameter and three to thirty feet long. Heavy weights propel the coring pipes vertically into the bottom so the cores show not only the surface sediments but also the layers just below. Sediment accumulates slowly in the ocean, and may not accumulate at all on small hills. Consequently, even these short cores may recover sediment millions of years old. With our profilers we can follow deep layers to places where they reach the surface, on a cliff for example. When we core such a place, we are able to deliberately sample old sediment which we would be very lucky to hit if we did not have profiler records. This is also true for dredge hauls of rock. We used to try to dredge rock from cliffs which often turned out to be covered with mud. Now our chances are better. If our dredging is successful, we should have no difficulty in resolving the nature of the submarine rises and ridges in Melanesia. Continental rocks are strikingly different from those of the ocean basins. If they contain fossils or are fresh enough for radioactive mineral dating, we can also tell their age and a great deal about their history. If they are very old or otherwise unusual, we shall want to make more dredge hauls.

When the expedition was first planned we had the choice of trying to do all these things at once or doing them in stages. We elected the latter course because so many programs are

dependent on others. *Horizon* will spend three months sounding and profiling and measuring the magnetic field without ever stopping except twice for fuel. This will give us the information to plan all the subsequent work in following months. We expect to analyze all the data as rapidly as they accumulate, which will require a newly developed method of computer handling. If it works, all the preliminary analysis and much of the final analysis of all the millions of observations will be completed by the time the ships return to San Diego. I certainly hope so, not only because it is essential for planning the later parts of the expedition but also because I shall not go to sea again until it is done. American oceanographic laboratories are loaded with undigested observations, and I do not want to be guilty of adding to the pile.

The General Schedule of The Nova Expedition

Argo

LEG I E. Goldberg*

17 Apr., San Diego to

16 May, Pago Pago

LEG II W. G. Van Dorn*

19 May, Pago Pago to

9 June, Honolulu

LEG III H. B. Craig*

16 June, Honolulu to

8 July, Suva

LEG IV E. L. Winterer*

12 July, Suva to

8 Aug., Noumea

LEG V H. W. Menard*

(1st half)	11 Aug., Noumea to
	23 Aug., Brisbane
(2nd half)	25 Aug., Brisbane to
	12 Sept., Auckland

LEG VI H. B. Craig*

18 Sept., Auckland to

2 Oct., Suva

LEG VII R. L. Fisher*

(1st half)	7 Oct., Suva to
	16 Oct., Tonga Islands
(2nd half)	16 Oct., Tonga Islands to
	26 Oct., Pago Pago

LEG VIII V. Vacquier*

(1st half)	29 Oct., Pago Pago to
	4 Nov., Suva
(2nd half)	4 Nov., Suva to
	19 Nov., Pago Pago

LEG IX A. E. Engle*

22 Nov., Pago Pago to

11 Dec., Hilo

LEG X P. Crampton*

11 Dec., Hilo to

19 Dec., San Diego

* Scientist in charge (SIC)

Horizon

LEG I

(1st half)	**T. Chase***
	18 Apr., Kwajalein to
	3 May, Brisbane
(2nd half)	**S. M. Smith***
	6 May, Brisbane to
	29 May, Noumea

LEG II E. L. Winterer*

1 June, Noumea to

1 July, Suva

LEG III H. W. Menard*

4 July, Suva to

11 July, Suva

LEG IV H. W. Menard*

13 July, Suva to

7 Aug., Noumea

LEG V G. G. Shor, Jr.*

(1st half)	11 Aug., Noumea to
	23 Aug., Brisbane
(2nd half)	25 Aug., Brisbane to
	12 Sept., Auckland

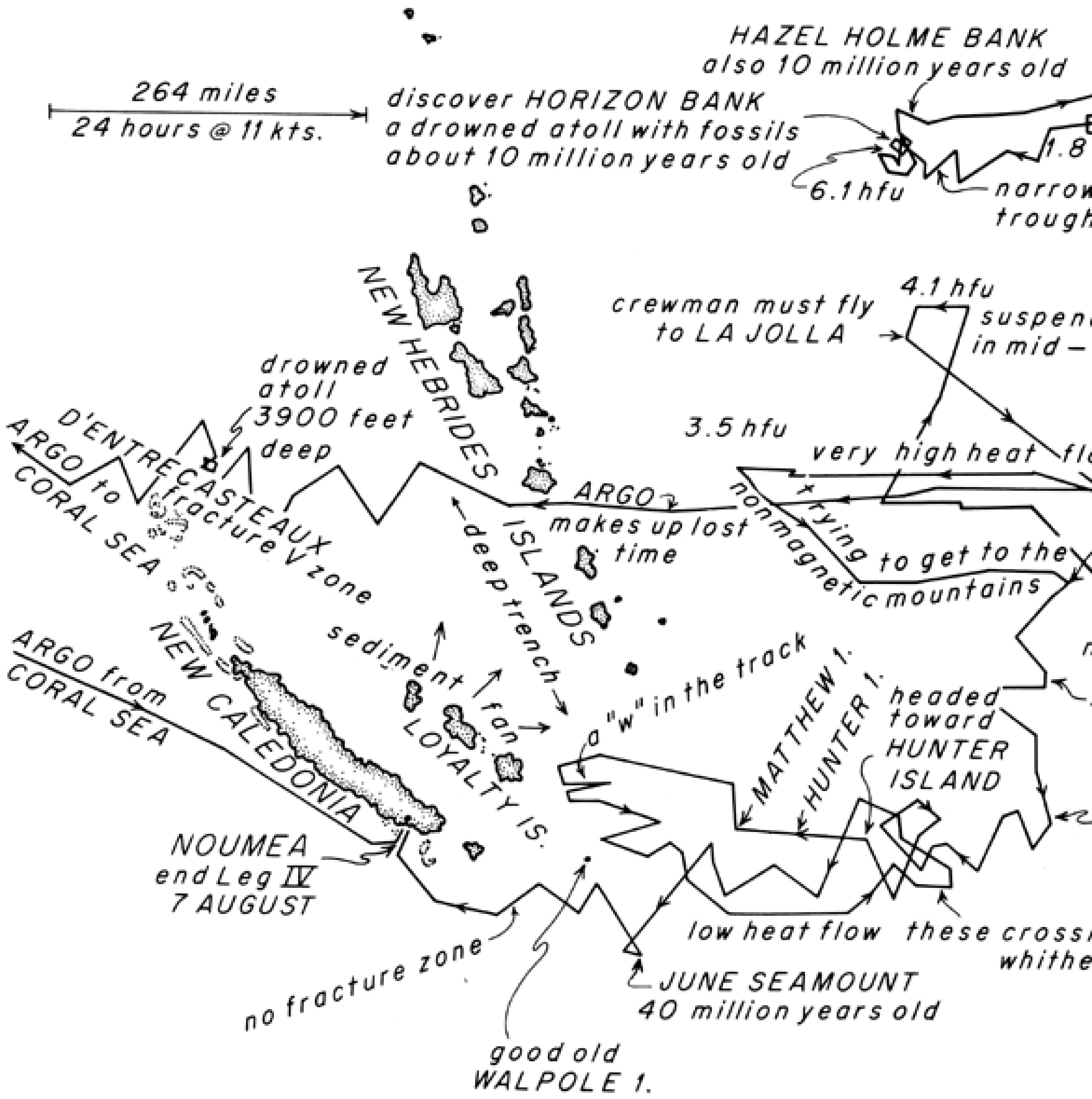
LEG VI D. E. Karig*

(1st half)	12 Sept., Auckland to
	26 Sept., Pago Pago
(2nd half)	26 Sept., Pago Pago to
	14 Oct., San Diego

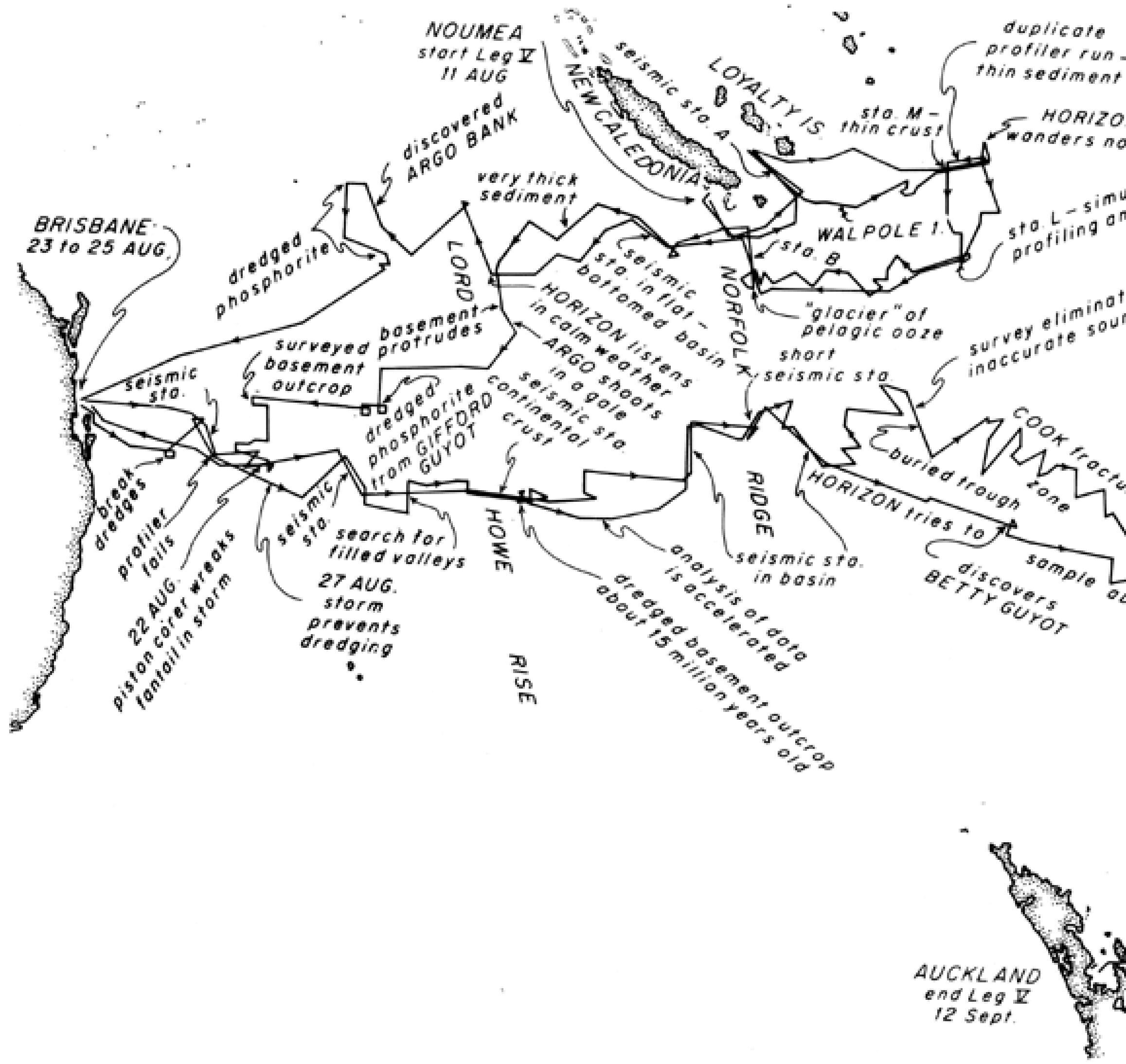
* Scientist in charge (SIC)

Track Charts

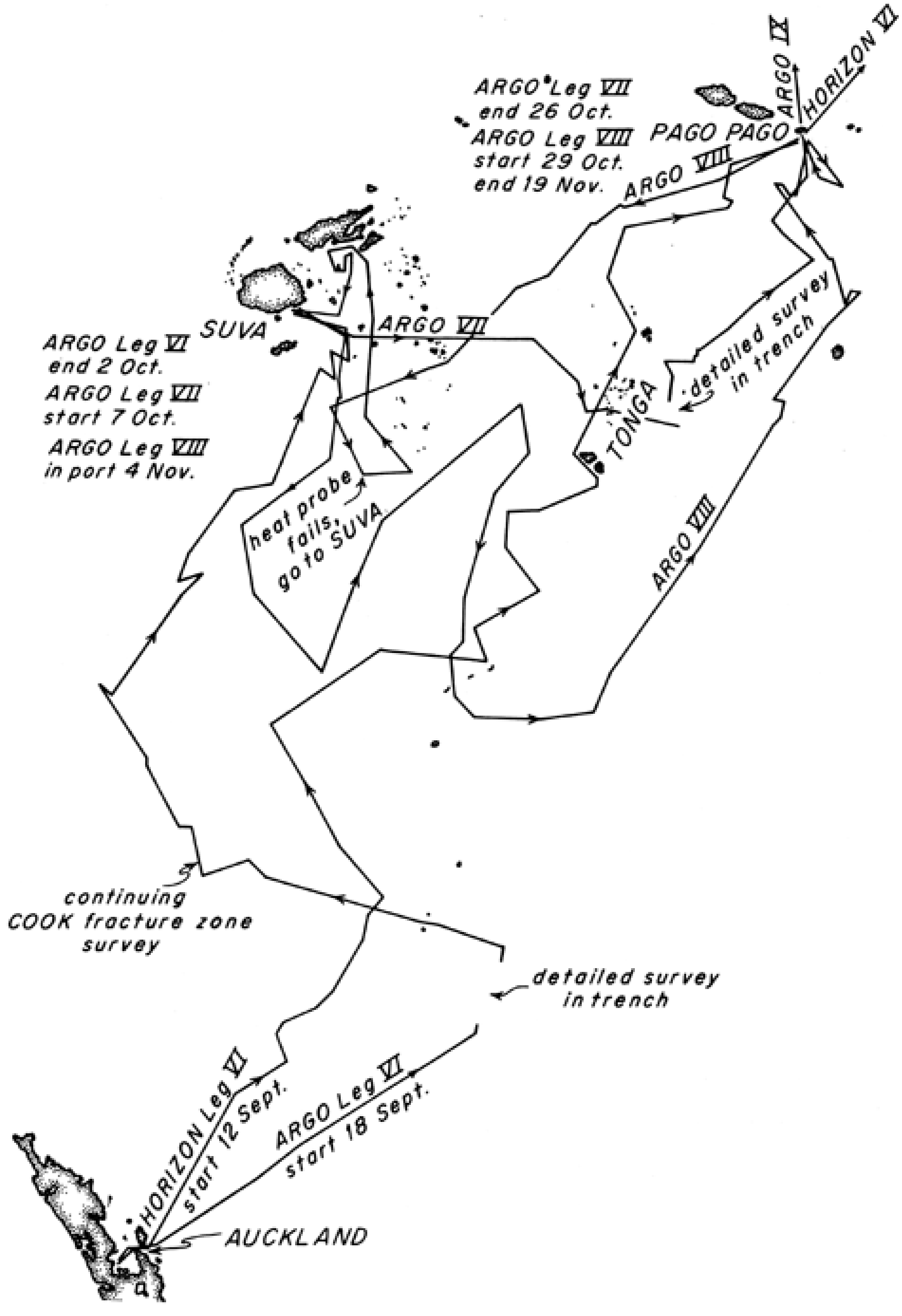
Excerpted track charts from Chapters 8, 9 and 14.



Track of R/V Argo Nova Expedtion Leg IV (July 12-August 8, 1967), Page 115



Track of R/V Horizon Nova Expedition Leg V (August 11 -September 12, 1967), Page 146



Tracks of R/V Argo Legs VI-IX and R/V Horizon Leg VI (October-November 1967), Page 230