



SEEKING
SIGNALS
SEA *in the*

Recollections of the
M A R I N E
P H Y S I C A L
L A B O R A T O R Y

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Layout by jo p. griffith

June 1997

Published by:
Marine Physical Laboratory
of the Scripps Institution of Oceanography
University of California, San Diego

We gratefully acknowledge the following
for use of their photographs in this publication:

Christine Baldwin
W. Robert Cherry
Defense Nuclear Agency
Fritz Goro
William S. Hodgkiss
Alan C. Jones
MPL Photo Archives
SIO Archives (UCSD)
Eric T. Slater

SIO Reference Series
97-5

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Introduction: How MPL Came To Be

Betty Shor

The Marine Physical Laboratory was established shortly after World War II ended, as a follow-up of wartime scientific programs. Roger Revelle summarized the wartime beginning:

*Prior to the entrance of the United States into World War II, German submarines were posing a disastrous threat to the Allied lifelines across the Atlantic and the Caribbean. This threat intensified for almost two years after 1941. To help meet it, the National Defense Research Committee (NDRC), one of the wartime scientific agencies created to mobilize all segments of American science in support of the war effort, established laboratories at San Diego and New London, directed almost exclusively to submarine and antisubmarine warfare. They also contracted with the Woods Hole Oceanographic Institution to transform itself for the duration into a military research and development laboratory for underwater sound and explosive phenomena. ("The Age of Innocence and War in Oceanography," *Oceans Magazine*, vol. 1, no. 3, Mar. 1969, pp. 6-16).*

The San Diego project was on Point Loma at the Navy Radio and Sound Laboratory and was named the University of California Division of War Research (UCDWR). Physicist Vern O. Knudsen of UCLA was the first director of UCDWR, and in 1942 was succeeded by physicist Gaylord P. Harnwell, on leave from the University of Pennsylvania. Half a dozen scientists from Scripps Institution of Oceanography, including its director Harald U. Sverdrup and Roger Revelle, were early participants in the laboratory. Also there was physicist Ernest Lawrence. Revelle said:

Lawrence and his friends, reasoning with some justification that the oceanographers were bumbling amateurs, quickly decided that underwater sound was a poor way to catch submarines and that optical methods should be used instead. They constructed an extremely powerful underwater searchlight and sewed together a huge black canvas cylinder which could be towed underwater to imitate a submarine. Unfortunately, it

turned out that when the searchlight was directed on this object, it could be detected out to a range of about 100 feet. Shortly thereafter many of the physicists disappeared from UCDWR. The rest of us did not learn until after the war that they had gone off together to design and build an atom bomb.

Revelle went to Navy duty in Washington, D.C., and the physicists who continued at UCDWR were ones who specialized in acoustics. The laboratory reached a staff of 600 people, which included, according to Harnwell in the final report: physicists, engineers, psychologists, artists, writers, draftsmen, illustrators, housewives, and high school students to help in the war effort. Revelle described the results:

Much was learned during these wartime years about sound in seawater: the variety of background noises made by shrimp, fish and breaking waves; the existence and diurnal migration of the deep scattering layer that reflects sound as if it were a false sea bottom; the fact that the ocean rings with echoes like a badly designed auditorium; the upward bending of part of a sound beam in the mixed surface layer and the downward bending of the rest of the beam in the thermocline; the sharp attenuation of high frequency sound with distance in the ocean; and the enormous distances over which low frequency sounds can be propagated if the acoustic energy is protected from absorption in the bottom muds or the bubbly surface waters.

The deep scattering layer, for instance, was noted specifically by UCDWR scientists Russell W. Raitt, R. J. Christensen, and Carl F. Eyring. It registered on the scope of the echo sounder as if it were the floor of the ocean itself. Where the bottom was known to be 12,000 feet deep, the phantom registered at 1500 feet. The peculiarity was first thought to be created within the instruments, but it was soon confirmed as real. Scripps Institution biologist Martin W. Johnson believed it to be a layer of living organisms, which would rise in the evening and then disperse and form again the next morning. He accompanied a trip at sea in June 1945 which confirmed his prediction.

UCDWR was scheduled to be dismantled in early 1946, but many of its people joined the Navy Electronics Laboratory that was being established on Point Loma. Its aim was "to effectuate the solution of any problem in the field of electronics, in connection with the design, procurement, testing, installation and maintenance of electronic equipment for the U.S. Navy." However, physicist Carl Eckart, who had joined UCDWR from the University of Chicago in 1942, wanted to continue his researches in a university environment.

Revelle recalled this time in his after-dinner remarks at the Navy symposium in honor of Russ Raitt and Vic Vacquier in 1986:

Before the end of the war, in 1945, the University of California Division of War Research and the other components of the wartime effort rather rapidly faded away. I was at that time with the Bureau of Ships in Washington in the Navy Department, and we were very much impressed by what good work the laboratory had done in underwater sound, which is really the way to look for submarines. And how much there was still to do, how much science there was still to do.

The moving spirit of this enterprise in Washington was an astronomer named Lyman Spitzer ... one of the great astronomers of our generation. ... Lyman and I together wrote a letter ... for the Chief of the Bureau of Ships [Vice Admiral Edward L. Cochrane] to sign. It was a revolutionary letter to President Robert Gordon Sproul of the University of California. It said to President Sproul that the Bureau of Ships of the Navy Department wanted the University of California to establish a laboratory under the direction of a particular man, named Carl Eckart, and if the University established this laboratory, the Bureau of Ships would give it tenure — which meant that we would support it indefinitely, without limit of time, as long as the Navy existed as a Navy and was concerned with submarines.

This was an unprecedented thing for anybody in the government to do. We operated on one-year or at the most two-year contracts, and the idea of support for an unlimited time was quite shocking to Admiral Cochrane. So he sat on this letter for seven or eight months.

We went to see him from time to time about the letter, and he said, "Well, I'm thinking about it." And finally in January of 1946, he actually signed the letter. ... Then it turned out that we had an equally difficult time persuading the University of California. President Sproul and [business manager] Bob Underhill and the other officers of the University were not at all certain that they wanted to cooperate with the Navy or that they wanted to do anything in underwater sound or that they wanted to do research that would be paid for by the federal



Roger Revelle and Elizabeth N. Shor, March 7, 1989.

government. It's hard to believe now, but that's the way it was in 1946.

It was not until the summer of 1946, six months after Admiral Cochrane signed this letter and sent it to Berkeley that President Sproul and the regents agreed that maybe they could do this. And all this time Carl Eckart was wanting to go back to the University of Chicago. I would have to come out and hold his hand every two weeks or so and tell him, "It's going to happen pretty soon now, Carl." And it finally did happen of course, and the regents and the President of the University did agree to accept this contract with the Navy. Carl Eckart did become the director of the laboratory and professor of marine physics in the University of California.

The Marine Physical Laboratory opened, adjacent to NEL, on 1 July 1946, with a staff of five people: Director Carl Eckart, Russell W. Raitt, Robert W. Young, William C. Kellogg, Jr., and Finn W. Outler. Leonard N. Liebermann joined the staff within the first year. On 25 November 1946, under Task 10 of Contract NObs-2074, MPL was assigned as research problems:

- (a) Theoretical and experimental investigations of the physical principles governing the generation and propagation of sound in the sea;
- (b) Studies of related phenomena as necessary to provide a broad scientific foundation for the above principles;
- (c) Investigations of the principles governing the recognition of signals, with special emphasis on underwater sound signals of all kinds.

The sources of funds through the years have changed, but the basic assignments have continued, thanks to the breadth of those early statements and the concern by MPL for "the deep ocean problems of the Navy and the basic understanding of the environment needed for it to operate intelligently."

Carl Eckart continued as director of MPL from 1946 until 1952. Within that time he was appointed director of Scripps Institution of Oceanography in 1948, and he arranged with the University of California for the laboratory to become part of that institution. His special concern was signal processing, but he took an interest in all of the laboratory's programs from its beginning until his death in 1973.

Canadian-born physicist Sir Charles S. Wright became director of MPL in 1952. A member of Robert F. Scott's antarctic expedition

from 1910 to 1913, he had served during World War II as director of scientific research for the British Admiralty, in underwater sound projects, and after the war as scientific advisor on the British naval staff in Washington, D.C. He was director of MPL until 1955.

Alfred B. Focke, a physicist at NEL, joined the MPL staff in 1954 and became its director in 1955. He was in charge of the nuclear depth-charge project Wigwam. He left MPL in 1958.

Physicist Fred Noel Spiess, who had joined the staff of MPL in 1952, was director from 1958 until 1980; he was also acting director of SIO from 1961 to 1963 and director of SIO from October 1964 to June 1965. Some of the role that he has played at MPL is given in this account in his own summaries.

Kenneth M. Watson, professor of physics at Lawrence Berkeley Laboratory at UC Berkeley, was appointed director of MPL in 1981. Following his retirement in 1991 he continued as acting director until 1993.

William A. Kuperman, who had been a research scientist in acoustical physics at the Navy Research Laboratory in Washington, D.C., became director of MPL in 1993.

Carl Eckart and the Marine Physical Laboratory

Leonard Liebermann

As a young man with a newly minted Ph.D. in 1925, Carl Eckart became fascinated with the then-emerging field of quantum wave mechanics. Highly mathematical and revolutionary in its concepts, it was ideally appropriate for a young physicist with mathematical inclinations. Not surprisingly, he was awarded a postdoctoral fellowship to study in Munich, Germany, with A. J. W. Sommerfeld and other leaders in the field. Shortly thereafter, working independently, he published an important fundamental result, unknowingly in competition with similar work by Werner Heisenberg. Later, another brilliant work established his name permanently in the foundations of quantum mechanics: the Eckart-Wigner theorem.

Scientists who are initially successful in their chosen field ordinarily stay with it; why tamper with success? Not Carl Eckart. Having become famous as a young man in the emerging field of quantum mechanics, he promptly shifted to well established classical physics subjects: electrodynamics and thermodynamics. Perhaps he, like Einstein, was troubled by the weird duality of particles and waves of quantum mechanics. More recently, even Richard Feynman complained about his “uncomfortable feeling” with the measurement vagueness codified by the Uncertainty Principle of quantum mechanics.

By the 1940s, Eckart was the leading theoretical physicist at the University of Chicago. At that time the attention of the entire physics department was focused on an exciting discovery: a nuclear-fission chain reaction occurred in a uranium pile constructed on campus by newly arrived Enrico Fermi. Shortly thereafter, our country entered into World War II, and it was inevitable that the Chicago physicists (as well as German physicists) would promptly mount a research program to investigate whether a uranium chain reaction could be made to explode a bomb. Arthur Compton, Sam Allison, Enrico Fermi, and practically the whole physics faculty decided to become immediately engaged in the project. But not Carl Eckart!

It is interesting to speculate on Eckart's lack of interest in the bomb

project. If his choice was influenced by moral reasons, he did not disclose that to his colleagues. Perhaps he had reason to believe that a fission bomb, even if practical, would never be used and hence would not influence the war's outcome. It is rumored that Heisenberg did not encourage Germany's bomb effort because his calculation of the bomb's size predicted it to be impossibly huge. Could Eckart have made a similar error?

In any case, we shall never know why in the summer of 1942 Carl Eckart decided to join the war research group in San Diego to improve submarine detection. At that time our east-coast shipping was suffering severe losses by German submarines, and our subchasers and destroyers equipped with sonar were not detecting submarines as expected. For example, the U.S. Navy described a problem known as the "Afternoon Effect," referring to the phenomenon that submarine detection was always less effective in the afternoon. Another problem was acoustic "reverberation," which consistently masked the returned sonar signal. In response to these and related submarine-detection problems, the University of California in 1941 had assembled a scientific group termed "University of California Division of War Research" (UCDWR), with headquarters in San Diego. Carl Eckart was appointed director of research in 1942. The work of this group culminated in a publication, *Principles of Underwater Sound*, of which he was both editor and principal author.

It was clear from the wartime acoustic work in San Diego, as well as at Columbia University and Woods Hole Oceanographic Institution, that there were many fascinating unanswered physics problems concerned with the ocean. Consequently, scientists from all three groups urged the Navy to establish a permanent center for continuing such research after the war. This would be the new field of marine physics, outside the mainstream, but of scientific as well as practical importance. Thus the Marine Physical Laboratory was founded and initially funded by the Bureau of Ships. Carl Eckart was asked if he would leave his professorship at the University of Chicago to become the first director of the new laboratory, and he accepted.

As Director of MPL, Eckart was an outstanding success. Although he was the consummate mathematical physicist, he took a personal and intense interest in the seagoing and laboratory experiments ongoing at



Carl Eckart circa 1945.

MPL. Nearly every noon, while munching on bag lunches, bought or brought, the scientific staff would hold lively informal meetings, always enthusiastically led by Eckart. Every experimental result was reviewed and subject to close scrutiny by the group. Eckart's interest in underwater acoustics guided him to investigate fundamental properties of sound in fluids. In particular, he developed a new theory of nonlinear acoustical phenomena in liquids. In addition to theory, he also indulged in experimental work. Long before computers were commercial, he suggested a computer. His design was analog, rather than digital, and was intended specifically to analyze statistically large amounts of data, particularly to extract the auto-correlation function.

Eckart's immediate and personal interest in the work at MPL ended all too soon. In 1948 he was asked to assume the directorship of Scripps Institution, and he accepted. Although he also remained as head of MPL until 1952, he was stationed in La Jolla and was unable to devote time to his prior MPL involvement and daily stimulating discussions. But he had made an indelible mark on the laboratory, which continued to serve as a high standard and inspiration for decades thereafter.

Close Encounter of the Worst Kind

Fred Fisher and Christine Baldwin

Carl Eckart, the founding Director of MPL, had many interactions with the Navy during World War II and thereafter, with all of them to the mutual benefit of the Navy and the University of California — except one.

On a day in 1958 he was driving on old U.S. Highway 395 southbound just east of the Miramar Naval Air Station in his 1958 Mercury Turnpike Cruiser. Navy fighter planes were doing touch-and-go landing practice maneuvers, circling the area time and time again. As he was approaching Miramar, Carl noticed that one of the fighters was in obvious trouble. Scientist that he was, he was calculating whether he would be better off slowing down or speeding up to avoid danger. He elected to slow down, but not quite soon enough. A 50-caliber machine gun from the plane dropped and penetrated his engine block. Although his car was severely damaged, he was not actually hurt. But the equipment that was impaled into the block of his car was classified. The military officers refused to release his car until they had thoroughly investigated the incident. He was unable to get his car back from them for some months to get it repaired.

Early Days of Seismic and Magnetic Programs at MPL

Arthur D. Raff

After getting out of the army, I went out looking for some career type of work. I had gone to some of the aircraft factories in the Los Angeles area, and they were interested in me, but not immediately. So I went over to UCLA, and I was talking to somebody there in the physics department who introduced me to Carl Eckart, who happened to be there on some business. He said, "We have an activity starting up in San Diego. Why don't you come on down and talk to us?" A few days later I drove to San Diego and found MPL, which was over where the Navy submarine training is now. I was introduced to Dr. Russell Raitt, who said he could use me, and we agreed on a monthly salary of \$220 a month. I came aboard at MPL in February 1947.

My first work was to analyze some records from a fathometer that Raitt had been using. And I'll tell you this was some fathometer. Instead of the records being on anything that we're aware of now — sort of a continuous profile record — they were individual echo returns on a continuously running photographic negative strip. The 35-mm negative strip ran fast enough to resolve the actual motion of a very small particle in the water — not an envelope curve. The fast-moving negative recorded the outgoing ping and the ping echo coming back from the seafloor. By measuring the distance from the outgoing ping to the echo return, one could determine the ocean depth at that point. By noticing the shape or signature or the envelope of the returning echo, one could tell something of the seafloor character — smooth sediment, rocky, or whatever. It all was an evolving science — the relationship of the outgoing signal to the characteristic of the echo as determined by the character and/or morphology of the seafloor. They were quite laborious to read, and my first job as I remember it was to start going through those things and catch up to date. Now these had been obtained on this very very special fathometer — one of a kind and just about a first — that was aboard the ship that we called the *Stranger* (when it was used by UCDWR it went under the name *Jasper*). From these records, Raitt, with R. J. Christensen and Carl F. Eyring, had found the deep scattering layer during the war.

Raitt needed quiet hydrophones for listening, for two types of work, reflection shooting and refraction shooting. For reflection shooting a charge would be set off, the sound would go down to the bottom into the various layers and come back up to a receiving hydrophone. It would be nice if one could keep the ship moving along at 8 or 10 or maybe 12 knots, but towing any hydrophone that fast, our experience proved, was out of the question. I tried to devise ways in which we could tow a receiving hydrophone at those speeds and still not have too much noise. The best method was Maurice Ewing's way; to have a very long line on the receiving hydrophone with most of the long cable piled on deck, and then let it sink down freely at the time the reflections were due back. Raitt lost interest in reflection shooting, so our efforts went entirely to refraction shooting, which usually used two ships. For that we needed to make the hydrophones very quiet for receiving. In refraction shooting the listening ship did not have to move through the water at all except to hold position if there was a current. It was just a matter of getting the hydrophones very quiet. So the problem was much more simple. The ship that was dropping the explosives could move as fast as it wished, simply tossing the explosive charges into the ocean.

For the listening ship I carefully developed a method of having a quiet receiving hydrophone. I reasoned that I needed something to keep the ocean water at the surface from yanking the line to the hydrophone up and down and creating noise, and then something down where the hydrophone was that would not generate noise from water flowing by it. After several tries of various things, I tried a spar buoy. The electric cable of the hydrophone went out to a spar buoy (made by Archie Dunlap in his shop) about 5 feet long, and 5 or 6 inches in diameter. The cable went out to the lower end of that, and then it would go straight down. I figured the spar buoy would not transmit so much wave noise down. Where the cable got down to hydrophone depth, there was a streamlined weight and then more of the hydrophone cable. About every two or three feet I had a little float that was just enough to float the cable. There were six or eight of those leading to the hydrophone, which was floated by means of a piece of aluminum pipe, with the front end round and the tail end tapered for a streamlined shape. The hydrophone was clamped to that long float. The idea was for all the horizontal cable and hydrophone assembly to be neutrally buoyant. This was all worked out by trial and error. That device was surprisingly good. We could not go fast but, at slow enough speeds to hold position against the ocean current, it had very low noise. We were able to operate out to many miles of distance, using large charges up to 300 and 400 pounds.

When we had only one ship available for refraction shooting, I was put into a lifeboat to set off the explosions. The receiving ship would come to a stop before I set off the explosion; Raitt would put the gear in the water and be listening; and then I would detonate the charge. The ship would receive it just fine. Then Russ had to pull the gear back aboard and proceed to the next station for listening, stop, put the gear out, tell me they were ready, and I would fire another charge. That was very time-consuming.

The first time that Raitt put me in a lifeboat with a lot of explosives, all alone, he steamed away in the large ship. We had radio communications. He was to let me know when he wanted a charge dropped and how large. None of them were really big charges, because we could not put a lot of weight of explosives in a lifeboat. The day was warm, and it was a dead calm. The ship was over the horizon, and I could not see it. I got to thinking that if our radio communications broke down, that would be one lonely place out there. I tell you it gave me an eerie, lonely feeling. Luckily, the communications did not fail, and after working there for a good share of the day, Russ's ship showed up over the horizon. After a few more charges were detonated, the lifeboat was pulled aboard.

Another time, we were off the east of Guadalupe Island. The ship was the 855 or 857. The Navy ship was to be the listening sound-recording vessel and would have to stop at each listening station, stream the hydrophone, and notify me to fire the charge. The sea was fairly calm. The deck hands of the Navy ship readied a powered lifeboat, helped me load it with my radio communications equipment, some sandwiches and water, explosives for the day, and fuses to provide time for me to lift a charge over the side of the boat and pull away a safe distance. The skipper provided an experienced lifeboat man to operate the engine and safely handle the boat in both shallow and deep water. The deck crew lowered us to the water. I was to drop the charges in water about twenty feet deep where they would explode on the bottom. I noted rocks and trees in several directions that I could line up so as to drop the charges in the same place.

Everything was going fine until about noon when suddenly a very strong wind came from the north. Almost immediately there was chop about two feet high. I quickly secured the gear as best I could. The boatman pointed the boat into the chop to minimize water coming into the boat and at the same time worked the boat away from the beach out into deep water where there was less chop. I had radioed the skipper and Russ to come rescue us from the worsening situation. Out in deeper water high steep waves were developing. By the time the ship

arrived the boatman, by keeping the boat pointed into the steep waves, was just barely keeping us afloat. The Navy ship arrived for rescue. There was no chance that in such rough seas we could attach the pulley block hooks fore and aft to our boat.

With the bullhorn the skipper told us to hang on and ready the slings to quickly attach to the pulley hooks. With full power he circled us twice at the smallest radius possible. That knocked down the waves to a slick. He stopped the ship downwind of us and yelled for us to hook up quickly while the surface was slick. We did so and were hoisted up to deck level and tied fast to the rail or whatever was there to lash to. We were safe and a little wet. With the ship moving ahead slowly for least roll, we unloaded and got the gear inside. The deck crew brought the boat over onto its rests and made it fast. No more shooting that day.

In doing refraction shooting there always seemed to be plenty of intensity with the water wave and the higher frequencies coming through that were refracted, but the very low frequency from the deepest layer in the ocean was weak. This frequency was down around 3, 4, 5, maybe 10 cycles per second, and we always wanted more of that intensity. So I started thinking of ways to get more intensity to this low frequency, at the expense of the higher frequencies. I was going on my own pretty much, and Russ just let me do it. I thought that if an explosive charge were set off in a bell of some sort — maybe two feet across and three feet vertical — and the bell filled with air and held down by weights to 10 or 15 feet below the surface, the explosive set off in there in the air would not generate so much of the high frequencies but might generate a lot more of the low frequencies. I decided to try that down by the NEL pier. I got a big trash can and put a bail on the open end and a wire down to a lot of sash weights. I rigged an explosive charge in the trash can's air space, only a quarter-pound stick. I used electrical detonation so I would have good control. I lowered the whole assembly carefully with the help of a cherry-picker. There was enough weight and tether length to pull the inverted trash can under about three feet below the surface. I had the listening recording hydrophone down the bay a short distance. I pushed the button, and the charge went off. It broke the bail and the wire down to the sash weights. This didn't affect the experiment at all. The trash can came to the surface, but I had lost our sash weights. I borrowed a grappling hook, fished around down there, and luckily snagged onto part of the



Assembled charge ready to hoist over the side, August 1949.

wire with the sash weights. The cherry-picker pulled them all up. I had gotten a signal over the receiving hydrophone. It was not really a satisfactory signal. It had a spike, which was necessary to generate higher frequencies in the water, and the estimated intensity of the low frequency was not increased much, if any. I decided it was not worth pursuing further in view of the fact that out in the open ocean putting down such a rig would be difficult.

At one time Russ told me that he would like to find out how much sound was transmitted straight up in the air from a charge that was set off, say, 25 feet below the surface, and vice versa, how much sound from a charge set off in the air is transmitted down into the water. I did not know what he had in mind that he was going to do, but anyway, he asked me if I would find an answer to that. That meant setting off some explosions up in the air and having a listening hydrophone below. So I thought of various ways I might do this. I decided that if I had a charge with a fuse on it and tied to a weather balloon, it would lift this charge up several hundred feet above the ocean surface, and when it went off the sound would come down through the air to my listening hydrophone below in the water. So I got some weather balloons and some explosives and a lot of fuse that would give the delay times that we needed. I used one of the picket boats from NEL. I waited for a calm day. The sailors were astounded when they saw me putting all this aboard. Anyway, we got it aboard and we proceeded on out just beyond the mouth of the harbor and about a thousand feet to the west.

With the help of a sailor I filled a weather balloon with hydrogen from a metal pressure bottle. With about three feet of string I hung a one-pound charge below the balloon and attached a fuse of estimated length to let the balloon rise about a hundred feet before detonation. We held the balloon over the side, lit the fuse, and let go. The charge settled down to the water! I should have had sense enough to test on deck before lighting the fuse, if the balloon could lift the charge, but I did not. There it was in the water, and the fuse going. Well, I never saw a skipper of a small boat get things started so fast in all my life. He rushed to the wheelhouse, hit the starter button of the diesel engine, threw it in gear, revved it up, no warming at all, and pulled away from there. He got it out at a safe distance, and all we could do was just wait to see it explode. When the charge exploded a beautiful orange-red ball of burning hydrogen rose up about thirty feet. The burned rubber balloon made a little smoke, and there was a dirty spot on the water.

I turned to the skipper saying, "Thank goodness you got away from

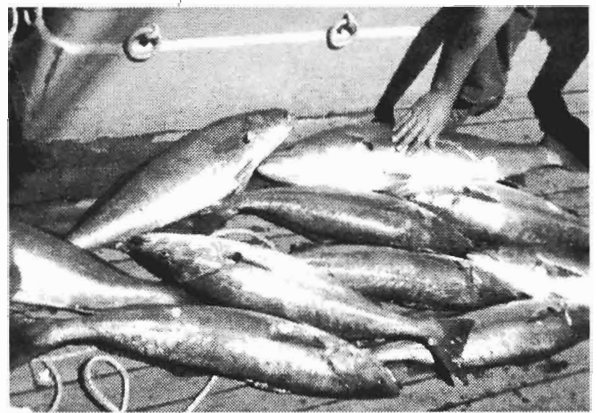
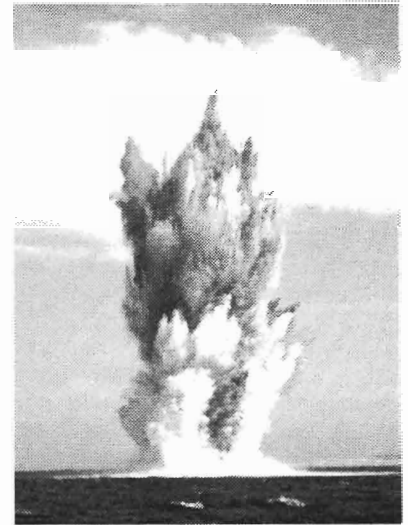
there. Let's try again. We know what to expect now." So we rigged up another one-pound charge and a lot longer fuse. Weather balloons are made to swell a lot before bursting. I could keep adding hydrogen till I got the lifting force I wanted. We added more and more hydrogen till we were satisfied that the balloon definitely lifted the charge. I lit the fuse and let go. It slowly rose straight up — no wind at all. It rose up to about two hundred feet and suddenly moved south. There was a wind current up there unbeknown to us. I was not very happy about that. I wanted it to explode right over our boat where I had the listening hydrophones in the water. Well, we looked south, and there was a large Navy carrier steaming slowly into port. The balloon rose higher and getting closer to being over the carrier, and when it was right over the carrier: Kerbang! A huge orange flame from the hydrogen curled up. Well, we all stood there wondering who was going to get in trouble now. I'll bet the guys out there on the carrier deck got a surprise when they looked up and saw that ball of orange flame rising in the air and then dissipating. We never heard a thing from that. Apparently, they were not worried or had no idea where it came from.

After thinking it over, I decided that using weather balloons was not the way to go. If using this idea at sea, we would have poor control. We would not know what the wind currents were above. The situation could be very dangerous if we were using it off one of the Scripps ships. Anyway, Russ Raitt for some reason decided he did not care to do anything more about the amount of sound from an explosion that could be transmitted down into the water, or vice versa. So that was discontinued.

[Raff continued to work for Raitt until 1952, then quit MPL for a year.]

In December of 1953 I went out to Scripps to see if they could use me. Roger Revelle had contacted Ronald G. Mason to do some work with magnetics, and he put us in touch with each other. Mason was impressed with the work that I had done for Raitt, and so I was employed. Then he immediately went back to Imperial College of London, where he had a teaching commitment. During World War II, Gulf and Western Electric developed special fluxgate magnetometers for aerial hunting of submarines. After the war the Western Electric versions were declared surplus and given to scientists. From Van Allen's schematics R. J. Smith converted two of the electronic parts to use in conjunction with the towed fluxgate part as a magnetometer.

My first job was to take the electronics part that R. J. Smith had built



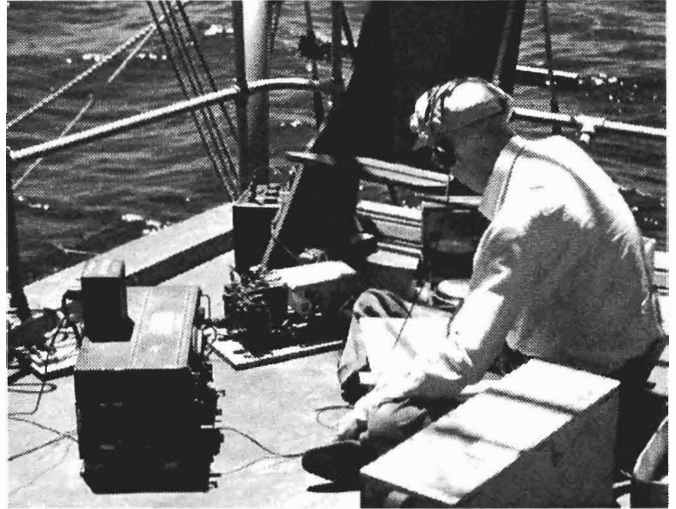
Top: Geyser of water and gas displaced by explosion. Bottom: White sea bass killed by explosion, August 1949.

from some schematics and some hardware provided to Mason and build a magnetometer for Ronald Mason's use. Magnetism was something that I had never given a second thought to up to that time and it hit me cold. I read one book Mason had given me, something about fluxgates and the earth's magnetic field and the gamma, the geophysicist's unit of magnetic field intensity. I wasn't doing very well, and Mason was not there to coach me at all. So things rather slid along until Mason came back. Using some materials that Van Allen gave us and some materials I purchased and with the specifications by Van Allen, I went down to Archie Dunlap's machine shop. Archie was a very competent machinist with many years of experience in shop techniques and the physics of treatment of materials. We carefully went over all the specifications and discussed what he would have to do to achieve our goals and build the fluxgate and all associated hardware.

This is not the place to tell how to build a fluxgate magnetometer, so I will only touch on interesting high points. Using a special kind of glass, Archie had to make a spool about 3/8 inch wide at the flanges and 1-1/2 inches long, with a hole in the center to take the heat-treated magnetic foil Van Allen gave us. Archie made the glass spool, using grinding techniques. Then the spool had to be wound with very fine insulated copper wire, neat as the thread on a spool of sewing thread. Rolling the foil and inserting it in the spool hole with no damage to the foil was quite a feat. Mason had ordered from the cabinet shop a control box of wood about 20 by 20 by 20 inches. It was to contain a very sensitive expensive galvanometer, a light beam and mirror method of reading the galvanometer, six very accurate detent decade rotary resistors, and a standard volt cell which could not tolerate any current drawn. The purpose of the control box was to measure the value of the earth's magnetic field in terms of the current in the glass spool winding that exactly canceled the earth's magnetic field.

The fluxgate and associated hardware and electric motors had to be towed 500 feet astern of the ship to minimize the magnetic field produced by the steel ship. This required some sort of a streamlined watertight case which we always referred to as the fish. Mason reasoned that the shape of the old bathythermograph would have been well tested and ideal for the shape of our fish, bathythermograph shape scaled up to fish size. The fish, to contain the fluxgate, the gimbals, and the little electric motors that were about three feet away from the gimbals, had to be about 12 plus inches in diameter and five feet long, and with the round nose and tail making for a total length of about eight feet. The watertight case was about an inch thick and made of a very special plastic. It had to withstand a water depth of about a

hundred feet in case it accidentally sank a ways while being handled over the side. There was an inch and a half thick plastic cap on each end machined to fit with "O" rings for a watertight seal. We had a local builder of fiberglass and plastic boats construct the round nose and finned tail with instructions to use no metal, especially iron. When the pieces were delivered to us, I could see an iron paper staple in one place. I had to have it all X-rayed to find all the staples and then dig them out and patch up the holes. For any later fish, I did my own glass and plastic work. The fish had to have enough pitch and roll stability to not exceed the rate at which the electric motors could turn the gimbals to keep the fluxgate aligned with the magnetic field. This was accomplished with a lead weighted keel. The 500-foot plus towing cable had to also have electric conductors. Vector of Texas custom built this. It was a 1-1/4 inch steam hose, one piece, about 510 feet long. Inside was a stainless steel multistrand wire, tested to 2000 pounds stress. Also in the hose were about nine electric conductors with some shielded. The fish was held by a heavy brass harness with two clamps around the fish body and a hinged piece reaching up to the stress terminating piece about four feet above the hinge. A heavy, hard to handle kludge.



Mr. Rooney with receiving hydrophones aboard the Paolina T, August 1949.

Russ Raitt and George Shor had worked up a refraction shooting trip off southern Mexico, and Mason wanted to take along his newly constructed magnetometer for both testing it out and obtaining some magnetic data. He was to arrive in San Diego just in time to help load gear on the ship and with me sail south to Raitt's working area. This was a two-ship refraction shooting operation with my ship the shooter.

When Mason and I were well clear of the harbor, we streamed the fish astern for towing and recording the magnetics. I knew a lot more than Mason did about handling gear over the side and towing things, so I supervised all this with Mason helping. We hooked up all the components and turned on the switches for operation. There was no signal! I really knew nothing about the magnetometer's electronics, and Mason knew a little. We had a complete set of schematics aboard, and Mason started meticulously checking the circuitry wiring. He found nothing obviously or grossly wrong. He kept working away almost day and night. I stayed close by to help as best I could. Mason was getting very discouraged, thinking he would be taking a long trip with no returns for his time and effort.

Finally he found one place where R. J. Smith had made an incorrect circuit connection. Mason changed the connection to that shown on the schematics. He turned on the power switches, and lo and behold he got a good magnetic signal. He let out a shout of joy! I too was quite happy because that got me off the hook, so to speak. We proceeded on south to the seismic work area getting a good magnetic intensity profile all the way.

Up to this time the magnetometer at sea had been simply used to run long lines. Maurice Ewing had obtained one in the Atlantic, and Mason had borrowed Ewing's magnetometer and in 1952 had run one east along the equatorial Pacific. Seamount anomalies had shown up, and there were anomalies that were not caused by seamounts, something not understood.

The Navy became concerned that nuclear subs could run very deep and might run into seamounts. The Navy arranged with the U.S. Coast & Geodetic Survey to survey the ocean on the west coast from nearshore out to about 300 miles, using the ship *Pioneer*, about 310 feet long. The survey was to start down by Guadalupe Island off Mexico, and work north to Queen Charlotte Island off British Columbia. It would operate a good fathometer to measure ocean depth on lines run east and west at 5 miles apart. Navigation was based on three mobile shore stations, all of which was called Electronic Position Indicator, which gave a position accuracy of about plus or minus 150 feet — very good at that time. Where things looked interesting or they needed more detail on depth of seamounts, the lines were to be split to two and a half miles, or even less.

Probably by way of Roger Revelle, who learned about this project — he was good at picking up things going on in Washington — a deal was worked out whereby Mason could tow a magnetometer on a non-interference basis. The *Pioneer* would go out for three weeks and come back in for rest, recreation, fuel, and outfitting and then go out for another three weeks plus, for trip after trip. They first operated out of San Diego, then San Francisco, Portland, and finally Seattle. They started before Mason could put a magnetometer aboard, so he missed the very southern portion of the survey. He did have his magnetometer aboard on the second and third cruises. With that he had a lot of data to work up to see what he had. At that point I volunteered to go on the *Pioneer* so we would continue to obtain those interesting data. When he plotted these data, he saw a most unusual pattern; there were magnetic stripes running north and south, which at that time made no sense at all.

As soon as we realized that this instrument was going to be used a lot, we immediately started to build several more in case one was lost. We ordered another towing cable from Vector of Houston. I built a second control box. We had a second set of the electronics. I ordered material to make another watertight fish, and we had three or four of the air-borne submarine-hunting fluxgate elements, surplus from the Navy. With Archie Dunlap's help I built several more of the converted fluxgate elements, so that we ended up with at least three complete towed units.

The harness that held the fish was a big, heavy, very clumsy kludge, difficult to launch and retrieve. I first made a much lighter-weight harness to hold the fish, then soon realized that we should tow it directly from the forward end of the fish. I built a long rubber snout, about five feet long, attached solidly to the front end of the fish, and tapered on down to just barely larger in diameter than the towing cable. This would bend with a large radius as it was towed and not fatigue the steel towing cable. If the fish were towed without any weighted keel, it would rotate over and over as the stress changed on the towing cable, intolerable to the fluxgate's stabilization mechanisms.

In the final version of the fish I lengthened the vertical dimensions of the vertical tail fins to about 24 inches and had an about 20-pound lead weight at the lower end of the lower fin. With that, we had a fish consisting of the long rubber snout, the cylindrical body, and the tail fins with a lead weight for roll stability. Our fish was really quite simple, and it worked well.

Mason could not continuously stay at Scripps to continue with the *Pioneer* farther north. As previously stated, I said I would do it. I went to sea several times, and I sent the data back to Mason to work up and plot. We were learning more and more about the magnetic lineations that ran approximately north and south, and had places where there were discontinuities. I became more and more interested as data came in and realized that we had something worth pursuing to the limit. I was away from home a lot, and my wife, Carolyn, was unhappy with my being away so much. Max Silverman made one or two trips and Alan Jones helped out a trip or two. Nothing was missed. The *Pioneer* did not operate during the winter months. They figured the weather would be bad, and they needed time to take extensive care of the ship, so they put into San Francisco for several winter months. I hired a brilliant young fellow named Gregory J. Nicholas, whom I trained, and he made the remaining trips. He was helped by Donn Lindbergh, whom I also hired. I always met the ship when it put into

port for its week's stay, which involved a lot of traveling, using an MPL truck. I was criticized by the MPL director for running up so many miles. The *Pioneer* finally got all the way up to Queen Charlotte Island.

By then Mason had worked up the data as far north as the Mendocino Escarpment, and I had the data from there to the north. So I handled that part of the job. Mason plotted data at a scale of four inches per degree of longitude. He was near-sighted and could plot data at this small scale. I certainly could not make the magnetic maps at that four inch per degree scale and did not even attempt to, even using a magnifying glass. I figured that it would be very easy when photographing my maps for publication to photoreduce them to the scale or size which the publisher desired. I chose a scale of fourteen inches per degree of longitude. The only problem with that scale was that my map would be more than a dozen feet from top to bottom. I solved that problem by cutting out rectangular pieces of paper for each degree of longitude, with a two-inch border all around the edge. On this rectangular sheet of paper I drew a heavy black line to indicate the longitude and latitude parallel lines. When contouring I would hold two sheets together so that I could carry the contour lines smoothly from one rectangular degree to the next. When the whole map area was contoured I had a photographic facility photograph each rectangular degree down to four inches per degree of longitude and glue the individual photo-prints to a large sheet of cardboard. That was my working master map for showing to people. The scheme all worked very well.

Using the EPI navigational information furnished to me by the Coast Survey, I plotted all the magnetic intensity values on the large rectangular sheets. To get all the contouring finished in a reasonable time for publication, I needed help. I employed Norman Head's wife. She was absolutely brilliant and hard working. She could soon outperform me, although when it came to interpreting geology and magnetic theory, I would help her. We did the contouring in pencil allowing for erasing as necessary. To put the contouring in ink for photographing to a smaller scale, I employed a draftsman experienced in ink work. He overlaid our final pencil contouring with semitransparent mylar and inked on the mylar.

Finally the contouring of the total measured magnetic field intensity was completed, and his inked contour work was sent to the photographic facility for photoreduction as told of earlier. Now came the job of removing the earth's theoretical smooth field leaving the anomaly only. I observed, as had Mason earlier, that the theoretical

field of a dipolar earth did not describe very large areas such as the extent of the *Pioneer* survey. Therefore, I had to determine the local smooth field of the surveyed area. Mason used one method for his map, and I used another for my northern map. I looked at each rectangular degree of the total measured field and noticed the value where there was no anomalously high or low value, an eyeball value. I wrote that number in the center of the rectangle. Having done this for the whole area of my map, I contoured those numbers with contours that were gentle or smooth curves. That was then my working smooth field. Using a trick fast method of subtracting the smooth field, I made a map showing only the magnetic anomalies. This was what was published. Where my map joined Mason's, I moved my anomaly contours a little one way or another to make a smooth transition. Some scientists who were observing our work objected to obtaining a smooth field as we each did; they said we should use smooth fields published in the literature. I had looked at these, and they were terrible — based on very sparse data that were sloppy measurements to begin with. Time, with more knowledge about the earth, has justified our choice of a smooth field.

Mason liked to do thorough work with his data and with no urgency to publish as we do. He finally published an article in the *Geophysical Journal of the Royal Astronomical Society* (volume I, 1958, pages 320-329), about the southern third of his anomaly chart. Later, he and I published two very extensive reports in *Bulletin of the Geological Society of America* (volume 72, August 1961, pages 1259-1266 and pages 1267-1270). The first by Mason and Raff on the southern half was titled "Magnetic Survey off the West Coast of North America, 32 Degrees North Latitude to 42 Degrees North Latitude." The second by Raff and Mason on the northern half was titled "Magnetic Survey off the West Coast of North America, 40 Degrees North Latitude to 52 Degrees North Latitude." Mason's part took care of a lot of the theoretical aspects of what might be causing the strange pattern, and I stuck more to the configurations revealed on my chart. These articles were accepted well by the scientific community, and brought about much further research.

During most of the latter part of our use of the fluxgate magnetometer, people at Varian Associates in Palo Alto were developing a proton-precession magnetometer which was more accurate than the fluxgate we had and could be made much smaller. I worked with them somewhat as a consultant, and then MPL purchased the proton-precession element to go inside a small towed fish. We built a cylindrical case for it in San Diego, about two-and-a-half feet long and about six inches in diameter. It had a much smaller tapered rubber snout to take care of

the towing-cable fatigue. It used a much smaller and lighter towing cable and required no fins or stabilization because the sensing devices would operate even when it was tumbling. It was towed 500 feet aft of a ship and worked beautifully. Several of these were constructed and used by MPL.

One time I was towing one of these proton-precession magnetometers as we approached Honolulu, Hawaii. When we were sailing along just east of Waikiki beach, sharks attacked the fish. I was not aware of the attacking sharks at the immediate time, but when I pulled the fish aboard just before entering the harbor at Honolulu, I grabbed the fish to heave it aboard and felt something cutting the palms of my hands. I looked at the fish case and saw dozens of sharks' teeth sticking out all over the case. The teeth had broken sharp ragged edges. I treated my hands and then examined the case. None of the teeth had penetrated the case, but were firmly embedded. Using a sharp edged steel tool, I scraped them all off down to where there were no cutting edges to cut bare hands. I continued to use that fish for years. I never again had that happen to a towed magnetometer fish.

Recollections of Work at the Marine Physical Laboratory: A Non-Academic Point of View

Dan Gibson

The Laboratory Staff Was Family

When I was first employed at MPL in August of 1948, the lab was located in Building 3W at the Fleet Sonar School off Harbor Drive (later to become the Admiral Kidd Officers Club). The staff at that time was quite small: I recall Dr. Carl Eckart, the Director, Dr. Russell Raitt and Dr. Leonard Liebermann, Frances Sparks and Chris Baldwin in the office, Finn Outler, Bill Grimley, Earl Squier, Stan Lai, and Archie Dunlap. My memory is not clear concerning Dr. Philip Rudnick, Arthur Raff and some others who go way back to about that time. I know that they, as well as Vic Anderson and Dan Andrews, were on board before 1950. To me they were all extended family. A couple of examples of why I felt like this come to mind.

At the time I started work at MPL, the Navy contract providing support had expired on the previous June 30th, at the end of the fiscal year. It was two months or more after I arrived before the contract was signed by the Regents and the Navy. During that period the University could not legally pay our salaries. Just going a month between pay-days is a struggle at first when one is budgeted for weekly pay periods, but going a couple of months or more without income can be a near-catastrophe when one has not been home from military service and gainfully employed long enough to accumulate much of a rainy-day fund. Without any complaint on my part, Finn Outler called me into his office and asked if I had any financial problems as a result of our not being paid. He told me that some of the senior staff would be willing to make me a cash advance if needed until such time as we would get our pay from the University. I accepted, and, believe me, it was a lifesaver. There was no note to sign and no interest to pay. It was never quite clear to me exactly who put up their own personal funds to do this, but I was under the impression that Dr. Eckart, Dr. Raitt, and Finn were involved. It was also clear that I was not the only staff member to take advantage of this offer. This procedure came to

be repeated at least a couple of times, if not more, during years to follow whenever there was a delay in the contract. I believe that, on a much later occasion, Dr. Roger Revelle was known to advance money under similar circumstances.

Another example of the extended family was a custom that was followed for a number of years in the early life of the laboratory. If we heard that a fellow staff member was making a local change of residence, we would suggest that the person moving should rent a truck or trailer, a dolly, and pads. A number of us would then form a work party (we called it a “goon squad”) and work all day Saturday or all weekend as needed to complete the move. The mover was expected to have a modest supply of cold beer on hand.

All of the few directors we had were caring people who were genuinely concerned for the welfare of the entire family. Finn Outler and Chris Baldwin were invaluable to those of us engaged in the nitty gritty technical support of the scientific investigations. In addition to getting out reports, papers, proposals, budgets and helping with personal matters, Chris served as our design draftsman and illustrator. Finn had served on a gunboat of the “Sand Pebbles” variety on Yangtze River patrol in China during the 1930s. It seems that he earned his master’s facility in cumshaw while there. With his many contacts at NEL and the Navy in general, Finn was always able to provide promptly the instruments, machines, components, books, manuals and materials, whenever there was a need. Given our project budgets at the time, much of the good work that we accomplished would have been virtually impossible without Finn’s expertise. He seemed to have powerful connections in many places that worked sometimes with and sometimes without paperwork. We also were frequent shoppers at military surplus stores and marine salvage yards where electrical, electronic and mechanical components and materials could be found at five to ten cents on the dollar.

In the machine shop we had Archie Dunlap, a toolmaker, instrument maker, machinist and mechanical designer, with outstanding talents, uniquely suited to our experimental work. As time passed, other machinists joined the team, such as Arnie Force, Gus Witfoht, Sam Web and Fred Uhlman. All were creative, inventive types, so suited to our endeavors. Arnie Force loved the laboratory as I do. He once said to me, “Dan, I’d rather you wouldn’t spread it around, but I would work here for nothing if it took that to stay on.”

Gloria Slack and Gwen Roy in a small office in Building 106 spent day after day, month after month, for years, reading multi-channel

paper oscillograph records from Dr. Raitt's seismic studies. They tabulated the readings of data, then crunched the numbers on mechanical or early electric (not electronic) calculators — and found it exciting! Bea Young and Mildred Rogers each typed over a hundred words a minute on a manual or early electric typewriter, correcting our spelling and punctuation all the while — and with five, six, or seven carbon copies! When more copies were needed, office staff cut a stencil and ran them off on a mimeograph machine. Phil Rapp's magnificent graphics made the presentation of an idea, concept or body of data so much more effective. Marge Toomath frequently performed near miracles in the procurement of desperately needed supplies. She used her powers of persuasion in such a way that people would agree to produce and/or ship something immediately just to get her off the phone.

From the time Dr. Vic Anderson came on board as a graduate student until long after I retired, he was a mentor, advisor, and counselor to me. I learned much from him and will forever be in his debt. Working with Dr. Raitt in my earliest years at MPL was a real joy. He was a tireless worker, patient and never angry, no matter how adverse the conditions. I recall one time during the early 50's while working on a project for Dr. Liebermann, I made a really stupid blunder of a design error which caused the destruction of an irreplaceable experimental cathode-ray tube. I was devastated. When I told Dr. Liebermann what I had done, he put his hand on my shoulder and said, "Don't let it bother you, Dan. It's only the people who don't do anything who never make mistakes." When Vic went back to the east coast (I believe Hunt Laboratory at Harvard) on a post-doc fellowship, he left me a project to pursue while he was away. This was probably the 1953-54 year. He instructed me to contact Dr. Rudnick if I had any problems or questions. I worked on it whenever I could find time, finally completed it, and set it up for a lab demonstration. I called Dr. Rudnick and he came right down to witness the demonstration. Upon completion of the demo, I said, "OK, it works, so what do I do now?" Dr. Rudnick said that Vic had told him nothing about the project, that he had no idea what it was for, but it looked good to him and I should keep up the good work.

So this was my extended family, and I loved working with all of them.

A Seagoing Laboratory

Finn Outler told me when I first started at the laboratory that the "Marine" in Marine Physical Laboratory means we go to sea. "You

must go to sea to study the physics of the ocean,” he said. He was right! Over the years there were day trips and extended trips, both near and far, on small craft and on sea-going vessels of all sizes — ships belonging to or operated by SIO, the Navy, Coast Guard, Coast and Geodetic Survey, U.S. Fish and Wildlife Service, Woods Hole, and Hudson Lab. There were at least five submarines, including the experimental *Albacore* and *Dolphin* and, of course, our very own FLIP and ORB.

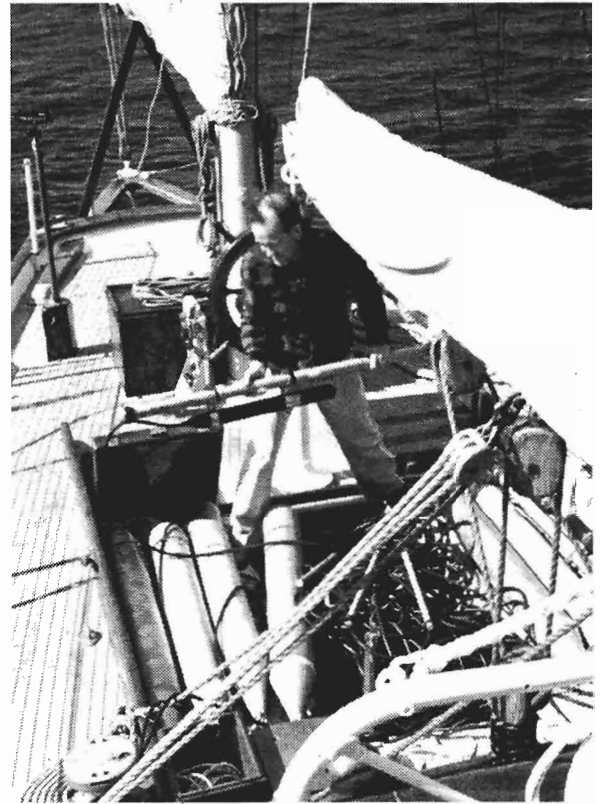
My first assignment, along with Bill Grimley and Earl Squier, was to design and assemble an electronic system involving a power amplifier/driver for an underwater acoustic transducer, a receiver amplifier, filters, monitor oscilloscope, recording system, etc. for installation on a craft we called the “83-footer” (later fondly called the “82.5-footer” after collision with the dock). This was in support of work being done by Dr. Raitt, having to do with reflection and refraction and absorption of sound waves. The lab space on the “83-footer” was a little hut on the fantail, no larger than 6 by 8 feet, if that big. The equipment was all mounted in two or three floor-to-ceiling relay racks. This was the era of vacuum tubes, way before transistors and integrated circuits. Vacuum tubes occupied lots of space, gave off lots of heat, and required large power supplies to deliver high, medium, and low voltages. Heat was a big problem in the little hut. This narrow-beamed craft wallowed badly in the troughs. That, coupled with the engine exhaust gases emitting from the stern adjacent to the hut, was a sure-fire cause of *mal de mer*. Russ Raitt suffered more than most, but he never let it interfere with his work.

In addition to the “83-footer,” there were a couple of other small craft operated by the Navy and available to us through the good offices of NEL. One was a mostly open motor launch of about 30 to 40 feet in length, called the *Buoy Boat*. The other was a high-speed, high-powered former rescue vessel for picking up downed Navy pilots in offshore waters. Called AVR, it was 40 to 45 feet in length and styled somewhat like a cabin-cruiser type yacht of the 30’s era. I can recall using one or the other of these on several occasions; some of Dr. Liebermann’s work comes to mind.

On one of Dr. Liebermann’s studies, we took the boat out around Point Loma and then north to a point just a few miles offshore from Torrey Pines Park. Once there, we lay to and streamed a couple of electrodes on the ends of cables off the two ends of the boat and down into the water. The location was chosen for low electromagnetic interference, and, for some reason or other, it was best done late into the night. It had something to do with atomic/subatomic particles hurling in from

outer space, I believe. The word neutrino comes to mind. The output from a high-gain audio amplifier, suitably filtered, produced an occasional chirp or zing or short whistling sound, increasing or decreasing in frequency. Recordings were made to document the statistical frequency and random pattern of these events, I believe.

Another of Dr. Liebermann's experiments involved measurements of attenuation of very high-frequency acoustical energy in seawater. In my youth I found this very confusing. I thought of audio frequencies as ranging from, say, 30 hertz (CPS — cycles per second — in those days) to maybe 40 KHz and radio frequencies from 15 KHz to thousands of MHz. Here was this scientist using a war-surplus radio transmitter to drive a very small quartz transducer with several hundred watts of power at frequencies in the 1-MHz to 3-MHz range to transmit acoustical energy over short distances through seawater to a suitable receiver. These were mostly conducted in the harbor, but some may have been done in open, less contaminated water. I learned the important difference between audio and acoustic. Yet another of his projects was a system of a small transmitting transducer and receiving transducer mounted a short distance apart inside a tube which was towed through the water to detect the presence of bubbles — a potential wake detector.



Vic Anderson as a graduate student did his thesis work on the scattering of sound by marine organisms. We used the AVR for much of this, and the work was carried out in the San Diego Trough some 20 to 30 miles off Point Loma. It was a quick but rough ride on the AVR to and from the operating area. Vic used a high-voltage discharge across a special spark gap of his own design on the end of a coaxial cable, lowered deep into the water as a broad-band sound source. A receiver nearby received the reflected sound energy from the marine organisms in the vicinity. The received energy was recorded on film in the form of a time/amplitude oscillographic recording for later analysis in the lab of frequency spectrum, time, and amplitude. Vic also did some related work on one of NEL's EPCERs. I don't recall if it was the 857 or the 855 on this particular trip. The EPCERs were identical craft, 220 feet in length and with a crew of about 40 men. I believe the EPCER designation formerly stood for "Escort-Patrol-Craft-Emergency-Rescue." In any event, we made the trip from San Diego south, down off the coast of Mexico to Guadalupe Island. The EPCER was accompanied by the *Saluda*, a former luxury sailing yacht also operated as a research vessel by NEL. I would guess that the *Saluda* was

Russ Raitt with seismic hydrophones on board Saluda, circa 1949.

about 70 feet long. On this trip Dr. Raitt and assistants were on the *Saluda*. In addition to Vic's scattering studies, Dr. Raitt was doing seismic reflection/refraction studies of the sea floor and the strata below it. Explosives were used as the sound source for Dr. Raitt's studies and the EPCER served as the shooting ship from which the explosives were dropped. The *Saluda* served as the receiving ship.

Prior to my joining the lab, Scripps had only one research vessel, the *E. W. Scripps*, a former luxury sailing yacht formerly owned by Hollywood actor Lewis Stone. The two masts on this auxiliary schooner were shortened from 100 feet to about 72 feet. In the very few times I sailed on the *E. W. Scripps* the only sail I ever saw rigged was a kind of storm tri-sail or reefed main hung between the masts to minimize the roll when riding in a trough.

In the same year that I came to work at MPL, Scripps acquired three more research vessels: *Horizon*, a 143-foot former seagoing tug; *Crest*, a 134-foot former mine sweeper; and *Paolina-T*, an 80-foot former purse seiner. At one time or another I was called to go to sea on all three, maybe once on the *Crest*, two or three times on the *Paolina-T*, and several times on the *Horizon*, including an extensive trip across the Pacific. I recall a somewhat humorous circumstance regarding the *Paolina-T*. In 1949 and 1950 Scripps had no on-shore radio station with which the ships could communicate. They could only communicate with each other via ship-to-ship or to their offices via commercial radio telephone or telegraph ship-to-shore services. There was a radio operator on the *Paolina-T* for a time who was a personal friend as well as a fellow amateur radio operator. Those were low-budget times for ship operations, and they were reluctant to use the commercial ship-to-shore services, except for emergency or high-priority messages. Since cross-band communications between ship-to-ship radio frequency assignments and the amateur radio frequency bands were not allowed, my friend Ben Switzler on the *Paolina-T* and I cooked up a subterfuge by which we could communicate when he was at sea. Ben had no equipment on the *Paolina-T* whereby he could transmit on the ham bands. At a pre-arranged time each evening Ben would call the *Buoy Boat*'s call letters on the ship-to-ship frequency. I would be tuned in at home to receive that frequency. Upon hearing his call to the *Buoy Boat* I would call his ham radio call-sign in Santa Ana. He would have a receiver on the *Paolina-T* tuned to my ham band frequency. Once we had established contact in this unorthodox manner, Ben would send his traffic, which I would forward first thing the next morning by phone to Jim Faughn, who was then the marine superintendent. If I had any traffic from Jim for Ben, I would transmit it back to Ben as though I were talking to his ham station in Santa Ana.

Anyone listening in to the ship-to-ship channel or on my ham-band frequency would only hear one side of the conversation and perhaps wonder why there was no sign of the other side, and have no idea where to look for it. It worked!

In midsummer of 1950, the *Horizon* and the EPCER 857 left San Diego on the longest, most extensive and ambitious oceanographic expedition ever undertaken by Scripps to that time. It was called Operation Mid-Pacific, or Mid-Pac. There were a large number of participants in this endeavor. In addition to scientists from Scripps/MPL and NEL, there were others from UCLA, USC, Stanford, and the U.S. Geological Survey. In addition to those of us who began with the expedition in San Diego, scientists joined and departed in Hawaii and in the Marshall Islands, far into the western Pacific. The exploration ranged south from San Diego to the equator, then through regions between the equator and latitude 40 degrees north and west to the Marshall Islands. As stated in *Scripps Institution of Oceanography: First Fifty Years* by Helen Raitt and Beatrice Moulton (p. 150):

Much of this area, lying as it did far from steamship lanes, was almost completely unexplored scientifically, and the findings of the thirty scientists and eighty-five technicians and crew members, it was expected, would comprise "a completely new level in our knowledge of the east central area of the world's largest ocean." Exciting discoveries were made, including the existence of the Mid-Pacific Mountains, a tremendous underwater range.

On Mid-Pac Dr. Raitt was on the *Horizon*, assisted by Art Raff and Wayne Runyon, while I was on the 857. For the purpose of Dr. Raitt's seismic reflection/refraction studies of the sea floor and the geology beneath it, which was carried out almost daily throughout the trip, the 857 was the sound source or shooting-ship and the *Horizon* was the receiving ship. I suspect that we dumped well over a hundred tons of explosives off the fantail of the 857, in bundles from 1/2 pound to 200 pounds per bundle. The explosives were war-surplus TNT blocks in 50-pound cases. We loaded before departing San Diego and replenished at Pearl Harbor and again at Kwajalein. We were assigned a Navy chief gunner's mate to supervise and assist in the handling of the explosives. The procedure was for the receiving ship to lie to and stream hydrophones on long cables away from the ship's own noise. The shooting ship would start from a position out some 30 miles distant and run toward the receiving ship while dropping charges at intervals specified by Dr. Raitt, usually 5 to 20 minutes. The most distant shots were typically 200 pounds and were reduced in size as the range to the receiving ship decreased and the received signal amplitude

increased. Upon passing near the receiving ship, the charge size might be 1/2 to 2 pounds, depending upon water depth and sea-floor structure. We had constant radio communications between ships, which provided for directions from Dr. Raitt and transmission of shot origination times in return. Shot origination times consisted of a voice announcement that the fuse was lit, the charge was in the water, and the output of a hull-mounted hydrophone signaled an accurate time of the actual blast, within a fraction of a second. Knowing the approximate range between the ships, the depth and the temperature structure of the water column, the receiving team knew precisely when to start their array of graphic recorders. After passing the receiving ship, the 857 would continue out to a range of approximately 30 miles, continuing the firings with increasing charge size until the profile was completed. A run would typically last 7 to 8 hours or more. This work was nearly always conducted during daylight hours for greater safety in handling explosives on deck. Once in a while, when we neared the end of a run, Russ would announce that the signals were fading, so the next shot would be the last. We would scurry around to get any leftover TNT back into the storage magazine before dinner, nightfall or whatever. Three or four minutes after the shot, Russ would come on the radio and ecstatically announce, "Oooh, but that was a good one! Could you please give me another as soon as you can get it made up?" Then we really had to scurry to break four cases of explosives back out of the magazine, open one to insert detonator, fuse, and igniter, close it back up, lash the four together and attach weights, light it, and get it over before we passed out of range. After many times this became sort of a joke on the 857: "Oooh, that was a good one!" and "Hey, guys, it's time to fire the sundown salute" became frequent sayings.

Lots of other work was being carried out on the two ships. There were long cores taken of the sea-floor sediments, dredges of bottom materials, bathythermographs, Nansen bottle casts for study of water chemistry, plankton net tows, weather observations, and more. Some of the notable names of our associates were, on the *Horizon*: Roger Revelle, Martin Johnson, John Isaacs, Jim Snodgrass, and Jeff Frautschy. Jim Faughn was the captain. On the 857 we had the company of Bill Batzler, Bob Dietz, Bob Dill, Ed Hamilton, Bill Menard, and Carl Shipek. Illustrious company! — and that's just a partial list. We were a month getting to Hawaii, spent a few days at Pearl Harbor for repairs to the 857 and to load fuel, water, stores, and explosives, then lots more science between there and Kwajalein and around the Marshall Islands and Bikini Atoll in particular. All the various scientific parties pitched in and helped one another with each one's projects. It was a grand experience for a young fellow like me. We even had a great party on the beach, on the lagoon side of one of the islands of Bikini

Atoll: a huge bonfire, beer, sandwiches, potato salad, and other goodies supplied by the *Horizon's* cook. Dr. Raitt and I, along with the rest of the seismic survey team and others, flew home from Kwajalein via Hawaii. There was a lot less science done on the two ships on the way home.

Other Work at Sea

Shortly after returning to the lab, I was asked to go to sea for a week on one of the U.S. Coast & Geodetic Survey ships to evaluate and improve the performance of their echo sounders (fathometers). Following that, Dr. Raitt and some of his cohorts, by way of Finn Outler, prevailed on me to go to sea for a month on the U.S. Coast Guard weather ship, the *Minnetonka*. This job was to adjust and tune their fathometer for best return and recording of signals returned from the deep scattering layer, and then spend 30 days with them on what was then designated "Weather Station Fox." Station Fox was a designated 5-mile square of Pacific Ocean surface approximately halfway between San Francisco and Honolulu. The Coast Guard maintained one of these ships at this location at all times. Their primary function was to make weather observations, including the launch of weather balloons several times daily, and to report their findings to shore. Secondly, they served as a radio beacon to aid aircraft navigation on the routes between the west coast and Hawaii. They also responded to distress calls from ships in the vicinity, once taking a seaman from a freighter and removing his appendix aboard the *Minnetonka*.

It seems that, prior to this data collection on the *Minnetonka*, all observations of the deep scattering layer and its twice-daily migrations and layer separations between the surface and about 200 fathoms had been made by ships in transit or at best lying to for a day while making other oceanographic observations. Even in those cases the fathometer was most often adjusted for best return and recording of the bottom echo, even to the point of eliminating the false-bottom indications occasioned by the presence of the scattering layer. The operating schedule of these weather ships provided the opportunity to study the behavior of the deep scattering layer in essentially one location in the ocean, thereby eliminating significant lateral variations from the study. The vertical variations could be correlated with weather and sea-surface conditions and the temperature gradients in the water column. My sole assignment was to enlist the cooperative assistance of the crew, sufficient to allow me to make continuous 24-hour-a-day fathometer recordings of the behavior of the scattering layer. In addition, I took with me several 1200-foot bathythermograph instru-

ments borrowed from NEL, along with 600 or more slides and a couple of spools of spare BT wire. Bathythermograph profiles were taken every hour around the clock. The crew of the ship were most cooperative, interested, willing and eager in their assistance to me. I did have a little lasting curiosity regarding my berthing assignment during my stay on the ship. It just happened that they put me in a stateroom with a chief warrant officer. It was a large beautiful stateroom far aft on the starboard side. My roommate's bunk was on the inside bulkhead, some 12 or 15 feet away from mine, which was on the outboard side of the room right against the hull. Where was the BT winch? Right, on the starboard side aft, mounted on the deck directly over the head of my bunk. Every hour on the hour, pay out 1200 feet of wire as fast as it will run, then wind it back in for 8 or 10 minutes. What a racket.

Most of my personal sea-going during the next couple of years was working with Vic Anderson in his scattering studies.

Capricorn Expedition

In 1952 the *Horizon* and *Baird* sailed away on the first leg of Operation Capricorn. Once again, they sailed from San Diego to the Marshall Islands. I'm sure they did science on the way out, but I'd not be familiar with it. I flew out later, along with other members of Dr. Raitt's seismic-studies team. We joined the ships at Kwajalein or Eniwetok, I don't recall which. This time the *Horizon* was the shooting ship and the *Baird* the receiving ship for Dr. Raitt's work. Eniwetok was making ready for a series of nuclear weapons tests. We ran seismic profiles from close to the outside shore of several islands in the atoll out in several directions for a considerable distance into deep water. A number of these runs were made before two of the tests, and an additional number of nearly identical runs following the tests. There was a lot of ship and small-craft activity in the lagoon, unlike Bikini during Mid-Pac, where we were the only people in the area. I recall one day, while riding at anchor in the lagoon at Eniwetok, an LCM that they used as a water taxi to transport people from ship to ship and ship to shore on the various islands came alongside the *Horizon*. A fellow in a Navy Commander's uniform looked up at a large pile of TNT boxes that were lashed down to the upper deck, and he hailed someone on deck to ask, "What do you folks have in the explosives boxes up there?" The answer was "It's TNT. Can't you read?" In less than an hour we received a radio call from the task group command requesting that we relocate our mooring to the unoccupied area at the far end of the lagoon. Also, Commander Monk

Hendrix, who was our Navy liaison officer on the *Horizon*, was requested to report to the Task Force Command office ASAP.

Dr. Raitt and I, along with a couple of others (maybe Will North and Alan Jones?), were privileged to watch the first nuclear test from the beach on the inside shore of Parry Island at the south end of the atoll. The device, a 100-kiloton atomic bomb, was dropped from a plane and detonated just a few thousand feet over the lagoon some 8 or 10 miles to the north of us. We wore goggles which were so dark that one could see nothing more than a very dull glow when looking directly into the sun. When the device detonated, for just an instant we could see the water, the beach and palm trees just as though in bright sunlight with no eye protection. At the same time the heat was so intense on our faces and bare arms that it felt much like a momentary blast from a blow torch. It was gone before I could complete the thought: *They goofed; we're dead men*. It also got totally dark until we could remove the protective goggles. Just a very few seconds following the flash, the shock wave hit us, nearly knocking us to the ground. A cameraman with a large movie camera on a tripod, very close to us, was knocked over backward, with the camera and tripod on top of him. I was amazed at how rapidly the mushroom cloud and fading fire-ball shot up into the sky, thousands of feet per minute. Horrible as it is, it was an incredible and unforgettable sight to behold.

Some time later in the operations, the second test was scheduled to go. This time, a one-megaton device was to be detonated on the ground, on the island of Elugelab — the very first ever hydrogen bomb. I believe that all of the people who lived on the islands were temporarily evacuated to the ships of the task group, which were stationed a few miles upwind of the atoll. There were no native islanders in the area at that time that I know of, just military, civilian support, and scientific people.

The *Horizon* was located about 40 miles downwind of the atoll, lying to with hydrophones streamed well away from the ship to record seismic shock-waves. Even though we were well over the horizon from the blast, the sky lit up very bright as seen through the protective goggles. We felt little or no heat from this one, and the shock-wave that arrived a couple of minutes or so later was considerably less than the one experienced with the earlier test. With this one, however, we soon experienced a small problem. Apparently, there were high winds aloft. When the mushroom cloud rocketed up into the sky, it then spread out and headed in our direction. In an hour or two, it became very dark and overcast, then started to rain. Our guest radiological monitoring people on board soon determined that the rain and the

debris carried with it were dangerously radioactive. All hatches, portholes, bulkhead doors, and air-vent ducts were closed off, and the ship's ventilating system was secured. After a time the rain stopped, the sun came out, and the inside of the ship became like a hot humid bake-oven. A small decontamination team, consisting of the two radiological monitors and three or four crew members, suited up in special disposable coverall suits, with head covers, boots, gloves, etc. They went out onto the decks to assess and minimize the hazard. They thoroughly washed down the entire exterior and jettisoned non-essential items that had been exposed to the rain, especially porous materials that had absorbed the radioactive rain and could not be washed clean. Wood, rope, cork, canvas and such, all had to go. We had some deck cargo of TNT in wooden boxes. They jettisoned that. There were even some giant clam shells which some of our people had collected from the lagoon. Those too went over the side. Really too bad!

After several hours the ventilation system was turned back on, and some time later we were permitted to return to the outside decks. It was a great relief to all. The good old *Horizon* would never be the same. Although the radiation level was said no longer to pose a threat to people working and living on the ship, it was said that the background level was sufficient to preclude later carbon-14 measurements that were so important to age-dating materials brought up from the sea floor.

We made quite a number of surveys in the days following the tests, both inside and outside the lagoon, particularly in the vicinity of Elugelab Island, which was now a submerged reef rather than the above-water island that we had seen only a couple of days before.

I didn't stay on with Capricorn Expedition after the completion of our work at Eniwetok in support of the Operation Ivy nuclear tests. I flew home, and R. J. Smith ("Smitty") boarded the *Horizon* for the expedition's visit to Fiji, Samoa, the Marquesas, Tahiti, and points between. Dr. Raitt, Alan Jones, Will North, and others of the seismic team also continued on to complete the expedition.

Work with Vic Anderson

Following Vic Anderson's return from the postdoctoral sojourn in 1954, most of my work for the remaining 27 years at MPL was with him and on work related to his projects. This did not diminish the sea duty very much. We designed, built, and took to sea at least 7 or 8 digital multi-beam sonar signal beamformers and processors

(DIMUS). These were taken to sea on World War II fleet submarines, a Navy destroyer, NEL's submarines *Baya* and *Dolphin*, the Navy's experimental submarine *Albacore*, and one even went to Ice Island Bravo very near the North Pole. I didn't go with that one; Gerry Denny and Commander Buck from ONR did that.

We had a project called reverberation studies, wherein an instrument designed and assembled by us was taken to sea many times, usually on the Scripps ship *Oconostota*. In this work, the cable-tethered instrument was lowered through water depths to a couple thousand meters. Sound pulses were transmitted at various frequencies in the 3.5-KHz to 25-KHz range. The reflected energy from marine organisms and whatever in the surrounding water column was observed and recorded as a function of depth, frequency, range, pulse length, and time of day. One trip with this system was a round trip to Hawaii, but I managed to lose the underwater portion of the system on the way home.

Then there was RUM: "Remote Underwater Manipulator." This one got a lot of laughs from various quarters. It is not well known, but the RUM II/ORB system performed some pretty significant and impressive work on the sea floor in 1972 and 1973. In 1972 a detailed statistical study of work performance was carried during five deployments between January 26 and September 1. We were at sea 58.5 days. ORB was moored and RUM operated on the sea floor in 11 locations. The operating depths for RUM ranged from 48 to 1880 meters. About 25% of the time at sea was spent in transit and the installation and recovery of mooring tackle for ORB. RUM spent 273.5 hours on the bottom, performing a wide variety of tasks. We recorded 9.9 hours of traverse over the sea floor for a total distance of 1680 meters; 49 sediment core samples were collected on 51 attempts; 29 vane shear measurements were made of the soil shear strength. Vehicle drawbar-pull capability was measured in several different soil conditions. On one of the deployments, which involved three different work sites, we performed some interesting work tasks for the Naval Civil Engineering Lab (NCEL) at Port Hueneme and the missile range at Point Mugu. In the Santa Barbara channel RUM was used to find and connect a steel lift cable to a large concrete slab, which was part of a sea-floor construction experiment, so that it could be recovered. Some distance offshore at Point Mugu, at about 700 meters of water depth, we had to find another concrete slab, smaller than the first, but still weighing about 3000 pounds. This one had a small taut wire fastened to its center, going up to an instrumented buoy, submerged 70 to 100 meters below the surface. We found the slab and made a connection with a short heavy steel cable attached to RUM. When RUM was lifted off the sea floor, the connection became taut and the

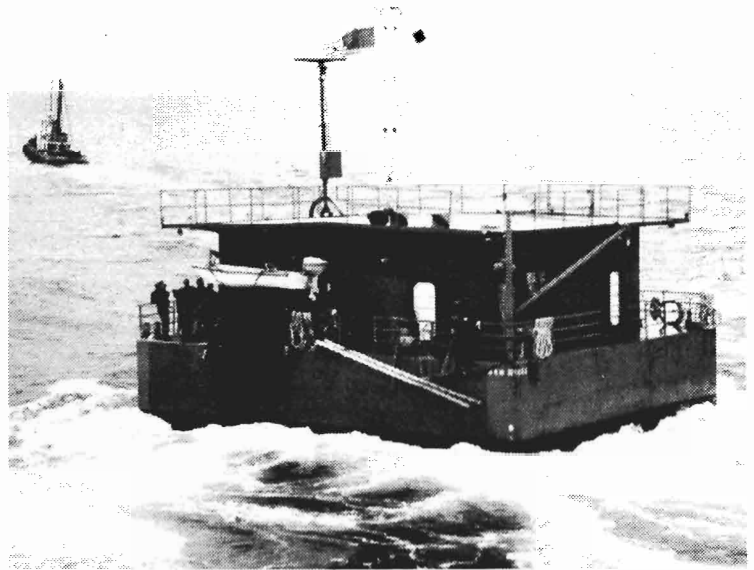
slab, after breaking loose from the sediment, simply followed RUM to near the surface. We picked up the buoy and tether wire as RUM and the slab ascended. Once near the surface, our trusty divers attached a piece of very heavy line to the slab and the load was transferred to a capstan on ORB. We called the guy at Point Mugu who owned the stuff. He was delighted, said he would be out first thing in the morning to get it. He was there bright and early with an LCU to which his gear was transferred. He passed us a case of cold champagne as a token of his appreciation. Then we were delighted!!

For the third task NCEL, some years before, had installed a set of nearly identical arrays of concrete and steel, in a line going down slope, in a somewhat southwesterly direction, to the south and west of Santa Cruz Island. They were dropped to the bottom from a surface vessel to depths ranging from about 500 to 850 fathoms. They were spaced such that each one in line should be nominally 15 to 20 fathoms deeper than the preceding. The simplicity of the design of this experiment was interesting. There was no electronic or mechanical instrumentation, yet some accurate data could be obtained just by visual observation. The object of this NCEL experiment was to test concrete construction materials in the sea-floor environment. Each array was made up of a buoyant, hollow concrete sphere with an identifying number painted on four sides, i.e., at 90-degree separation around the sphere's equator. To the bottom of the sphere was fastened a length of fairly heavy anchor chain, the length of which was sufficient that only about half of it was needed to overcome the buoyancy of the sphere. The other half would lie in a clump on the bottom to anchor the sphere. The spheres looked to be about a meter or more in diameter and they floated in the water column about 6 to 8 meters off the bottom. Just below the sphere was a cube of concrete, about 25 to 30 cm across each side, attached to the chain. If one simply knew the weight of each chain link and the weight of the concrete cube, one could determine the precise buoyancy of the sphere by counting the chain links suspended off the bottom. We were cautioned that the spheres, which were a meter or more in diameter, were moored through a range of depths which included their design-collapse depth. They were therefore potentially destructive bombs if one of them happened to implode near our vehicle. We were also told that there was a reluctance on the part of some of the manned submersible operators to approach these arrays. That is why we had been asked to make this survey. We were able to locate several of these arrays, through the critical-depth range, including one that had imploded some time previously. We found and photographed pieces of concrete shrapnel lying on the sea floor, scattered out to a distance of at least 10 meters from the clump of chain and the concrete cube. We recovered

the chain, cube, and a couple of pieces of the sphere. This provided samples upon which NCEL people could later take core samples for strength tests, water absorption measurements, and whatever. On the other arrays that we located and observed, we noted the ID number and counted the chain links suspended off the bottom, to determine the current buoyancy of the sphere. These observations were documented on color super 8-mm movie film with RUM's on-board camera.

Then, in 1973, Dr. Robert Hessler put together a program to study the benthic biology on the floor of the San Diego Trough, using the ORB/RUM sea-floor work system. This operation, dubbed Expedition Quagmire, was a month-long study, sponsored in part by the National Science Foundation, along with substantial support from Scripps, and some German, Dutch, Scandinavian, and possibly other support. This work is covered in the German scientific journal *UMSCAU* ("Ferngesteuertes Unterwasserfahrzeug erforscht Tiefseeboden," by Hjalmar Thiel and Robert R. Hessler, vol. 74, no. 14, 1974, pp. 451-453). In addition to Bob Hessler and Dr. Thiel, there were quite a number of other scientists involved in the operations on board ORB, and a number of scientific papers followed. The people included Ken Smith and other Scripps scientists, as well as Dr. Thiel and others from Germany, the Netherlands, and one or more of the Scandinavian countries. We were told that several heretofore unknown species of marine life were identified. A number of other scientific firsts were also alleged. The English summary of the paper cited above states:

RUM, the Remote Underwater Manipulator was used for the first time by a team of biologists, chemists, and geologists studying a bathyal, benthic community at 1200 m depth off San Diego. Bottom samples were taken to analyze the fauna, and the chemical and geological properties of the environment. The biological and chemical oxygen consumption of the seabed was measured, and for the first time respiration rates of deep-sea fishes were determined in situ. Baited traps allowed behavioral studies of mobile scavengers. Closed-circuit television allowed observation of the fauna and RUM's activities. As a result, all manipulations by the vehicle could be controlled with deliberate care. First results from these investigations are presented.



First ORB.

In closing I would like to pay tribute to that remarkable assemblage of people in Vic Anderson's group that I worked with so closely over many years. They were a grand and diverse group of many exceptional talents, who performed exceedingly well on a wide variety of projects. They always gave their best in many very difficult work situations, be it in the bilges, in the slippery slimy smelly sonar dome of a submarine, the frozen reaches of the arctic, a small-boat transfer in heavy seas, the saturation of major portions of their bodies with various oils in which ambient-pressure systems were immersed, or just handling instruments and gear on deck at any hour in foul weather. That is Marine, and believe me it's Physical.

Capricorn Expedition, 1952

Alan C. Jones

On September 26, 1952 the small sea-going tug *Horizon* slipped quietly out of San Diego with a motley crew aboard. Supplies and equipment had been hastily thrown aboard during the last frantic days before sailing. Our destination was a BIG SECRET, but it was fairly obvious that we were heading for the atomic testing area in the Marshall Islands. The first night out was quite miserable. The weather was poor, making the boat roll and pitch. All the loose equipment and supplies went flying, and some of us were seasick. The third day out the wind shifted from astern. The motion of the boat was more tolerant with a following sea. There was still much water boiling over across the fantail.

For twenty days we zigzagged our way west and south, stopping only at one of the outermost Hawaiian Islands, Necker Island, to repair the steering chain on October 6. It was a welcome stop, even though we could not get ashore. The island is uninhabited, with 300-foot cliffs rising up from the sea. Many booby birds came out to look us over. They were completely unafraid of us. I guess nobody comes to this part of the world. After repairs, we were off again at 10 knots on a zigzag course to the Marshall Islands. I was seasick most of the way, but managed slowly to get the wave-recording equipment finished and ready to install.

As we got further south, our quarters became hot and humid. Some tried sleeping up on deck, but rain squalls made this difficult. Of course, there was no air-conditioning on the ship. The *Horizon's* wash-down system was tested several times. This caused a salt-water downpour all over the super-structure. It was supposed to protect us from radioactive fallout. As we got closer to the Marshall Islands, we were ordered not to take any notes, and our cameras were locked up.



Work at sea on board the Horizon in the Pacific Ocean during the Capricorn Expedition, 1952.

Only the expedition photographer, John MacFall, was allowed to take pictures.

On October 15 a Navy pilot came aboard at 6 a.m. to pilot us into Eniwetok lagoon. We anchored in the lagoon, along with a lot of larger Navy ships. We were a misfit among the sleek Navy ships, with our rust-streaked white sides and junk all over the decks. The next morning Willard Bascom assembled his motley crew, including me. Most of them went off in the small launch (called the "tuna tender" because many of the crew were ex-tuna fishermen) to look for a good place to lay a heavy cable for the first wave recorder.

During our wait for "IT" we ventured out to other islands to install wave recorders. Sometimes these involved working on islands that were very radioactive from previous tests. Of these, Bikini Island was the most interesting. This island and its beautiful lagoon had been the site of the 1947 atom-bomb tests. No one worried about radioactivity then, except that maybe it was not too good an idea to stay ashore too long. The Navy would not allow the natives to move back on the island. The island was covered with expensive earth-moving equipment, jeeps, quonset huts. Most seemed to have been left in good condition, but were radioactive.

On October 17 and 18 the first cable was laid down from the lagoon to Runit Island. It was very hard work. The wave recorder was installed in a block-house that had six-foot thick walls. The next morning we ran a seismic profile off Jean Island, which is where the first H-bomb was being installed. There was a tall tower in the center. A large white rectangular tube extended across several adjacent small islands for about two miles. A very large square block-house was at the base of the tower.

On October 20 we set out to locate several sea-mounts on which to install wave recorders. The instrument was lowered down with a cable leading up to a raft holding the recorder and batteries.

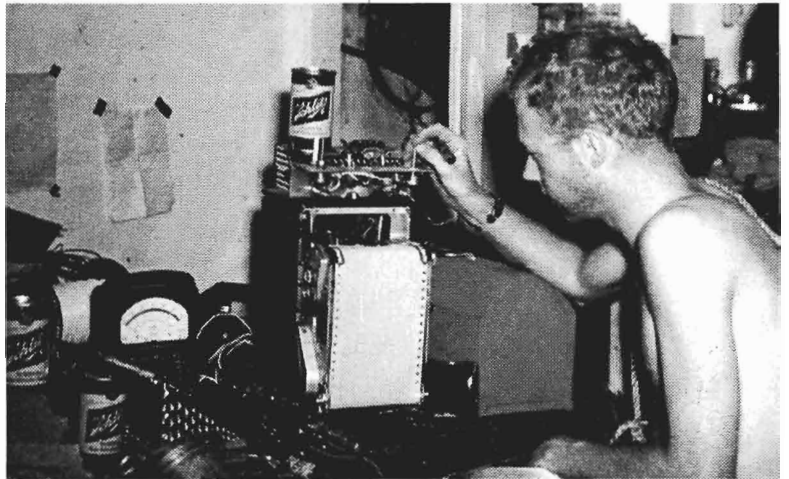
On the morning of the detonation of the first H-bomb, we were still installing the last wave recorder at sea. Willard Bascom and John Isaacs were out in the skiff fooling around with the raft, to which a cable led down to the sea bottom where the pressure sensor was located. I was up on the flying bridge with MacFall. We had protective glasses and were told to face away from the explosion. Our position was 90 miles away, while the Navy ships were located about five miles offshore.

A great orange fire-ball appeared on the horizon, followed by a large boom, with 17 more booms echoing off the stratosphere and ocean. Great clouds appeared, and it got rather dark. Then the radioactive rain started. The wash-down system was turned on, and the Navy gave us a course to get out of the rain. We were in the wrong place at the wrong time. The sprinkler system proved to be rather useless, as the water that it pumped up was radioactive.

The Navy radiation officer that had been put aboard to watch us at least saw the danger, and ordered everything on deck thrown overboard to reduce the radioactivity. We were only allowed out on deck for short periods, and the level of radioactivity got quite high. Even Russ Raitt's collection of shells that he had gathered while waiting for *Horizon* to arrive got thrown overboard. The *Horizon* was very miserable inside, as no fresh air could be brought in. For a while it was doubted that the ship could be kept in service, but with

constant washing, and much thrown overboard, the level of radioactivity got down to what was then considered to be a safe level. We had to run around picking up the wave recorders from the sea-mounts. These were extremely radioactive. The recorders were recovered, and the rafts were shot to pieces with gunfire.

The *Horizon* was the only vessel in the entire fleet to be hit with radioactive fallout.



Alan Jones working on wave recorder, Capricorn Expedition, 1952.

Qué Será Será

R. J. Smith

November 1954

One of our research ships is making a series of magnetometer stations and will be in Acapulco for a week, three days hence. The university has tried, through the State Department, to get permission to put a portable magnetometer in that area. Their answer: "Perhaps in one year."

My wife and I are at Nogales, U.S.A., across from Nogales, Mexico. Since the university would not risk one of theirs, I am driving my boss's Chevy pickup, three-quarter ton, four-speed, with a make-shift doghouse aboard. I am towing a 16-foot house trailer, *La Casita*, down to her marks with a portable magnetometer, on loan from Varian Associates of Palo Alto.

In Nogales I called the consul's office and talked to a Mr. Newton, who said, "Please be at my office tomorrow at 8:00 a.m. Please leave your rig on the U.S. side." There he said: "Mr. Smith, if, as you say, you have driven the length of Baja's 'no-road' twice, you are indeed a resourceful person. Baja, however, is not the mainland. It is more than 1700 miles from Nogales to Acapulco, and it is the rainy season. Poor roads will be quagmires, better roads will be flooded, bridges may be out of service. Mechanics and repair parts are difficult to find, gasoline often unavailable.

"Food and drink may be poisonous, and doctors and medical supplies are very scarce. Animals, tame and feral, roam at will, making day driving risky and night driving perilous. If you are involved in an accident resulting in an injury, your rig will be impounded and both parties jailed until guilt is established. *Mordida* (graft) is a way of life. If the contents of your trailer are discovered, you will be the special target of the poor trying to get rich and the rich trying to get richer.

"Eye contact will attract unwanted attention and different driving habits will be noticed. You will pass through Mexico's largest city,

having all possible urban ills. You will climb to 9000 feet and drive on a six-lane toll road where traffic is very fast and abundant. You are driving an old, tired truck, towing a flimsy, over-loaded trailer on an ill-conceived, ill-advised mission within a time constraint.

“Your rig, you and everyone involved (pointing to my wife) in this venture will be in jeopardy! And you are still going to go?”

“Yes, sir.”

“I will do what I can for you.”

The rest of that day was spent talking to a judge in his domed courtroom, to two immigration officials and to a bail bondsman.

The next morning we crossed the border with a handful of “papers,” probably not worth a *peso* outside the issuing jurisdictions, if there.

Mr. Newton was right. All the dire road conditions were met and some exceeded. At one place we crossed a river on a one-line railroad trestle. Planks had been placed along the iron tracks. But railroads don’t have guard rails. In the jungle around Manzanillo, I drove with extra caution, since there were occasional eight- to ten-foot boa constrictors sharing the roadway. At 9000 feet there is “less air in the air” and the extra gear in the transmission proved to be very useful. Then came the long descent to sea level, where we were met by most of the ship’s personnel, agnostics and believers alike. Here we met Dolan Mansir, an engineer from Varian, on site to operate the magnetometer.

Later we met Sig Varian and his charming wife, who invited us to dine at Acapulco’s finest hotel, after which we watched the famous cliff divers perform. The next day we flew in Sig’s four-place Cessna to some remote *ranchos* and returned with a big black saddle and many liters of local liquors, all to be carried to San Diego on an already overloaded *La Casita*.

We spent a week making magnetometer stations and then headed north, stopping in Mexico City for permission to exit Mexico at El Paso and to eat a fancy meal at the big new airport. Better roads and higher speeds brought increasing protests from the truck’s differential. I found a local man who spoke “Ingles” and knew the truck. For \$36 he rode a bus 28 miles and returned with a bag of parts, which we finished installing by little more than candlelight. That night the Smiths slept in the doghouse to guard *La Casita*. The next morning my wife felt sick.

Another night was spent in an old walled, iron-gated *hacienda*. I parked the rig on a patio, paved with colored, patterned, glazed tile,

too beautiful even to walk upon. After eating, we retired to a “ball-room” furnished with a giant four-poster bed, complete with canopy, a mattress almost a meter thick, and a curtained area with a pitcher of water, a wash basin, and a chamber pot. We slept like royalty — well, kind of. The next morning, my wife was sicker.

After three more, mostly uneventful days, we crossed the Rio Grande. Three more, and we were in San Diego. The next day my wife reported her visit to the doctor: “We talked and then he jabbed a 10-inch, one-pint hypo into my derriere, while saying that the disease is gone, but recovery will take a long time.”

Next, I called Nogales: “My name is R. J. Smith, and some weeks ago you folks helped me enter Mexico with a small truck and trailer. I am in San Diego.”

“Mr. Smith, we know where you are, but thanks for calling.”

More than a year passed. My wife was almost recovered from amoebic dysentery. Sig Varian and his Cessna had crashed somewhere in Mexico, and both were beyond repair. Dolan Mansir was recovering from an airplane crash incurred while pursuing his profession. *La Casita* was a “throw-away” but her electronics still worked. The Chevy pickup was rolled and had gone to “Truck Heaven,” I hope. My scars didn’t show.

In summary: The magnetometer land stations that we established in the Acapulco area were part of a giant grid of such stations, which may have helped lead to the theory of “continental drift.”

I Bare My Soul

(Another true tale by R. J. Smith from the late 1950s or early 1960s)

A large well-known research laboratory on the east coast of the U.S. was in trouble. Results per dollar had fallen to an unacceptable level, and it was scheduled to be closed. Financial loss to the community and political considerations in Washington would make this closing difficult, if not impossible.

My boss said: “The Office of Naval Research, the sponsoring agency, has decided that the best way to mitigate these complications is to remove the lab’s entire stockroom quickly to a remote location, resulting in a kind of ‘fait accompli’, so to speak.”

I was east of Yuma, Arizona when I had some disturbing thoughts. I was an employee of a small but highly successful research laboratory (the “L” in MPL), and I believed, then and now, that research was our best hope for a better tomorrow. And now I was going to deliver a *coup de grâce* to a research laboratory — a doomed one, but nonetheless a research laboratory. Now, I am not a “bleeding-heart type” and it wasn’t as though I was to burn the Library at Alexandria. And yet . .

When I arrived, I was met with stony faces (they knew), and I was handed all the keys to the stockroom.

It was after the second of four 14-wheelers had headed west that the disturbing thoughts recurred. As I said before, I am not the “bleeding-heart type”, and it wasn’t as though I was opening the door to a new Dark Age. And yet . . .

Soon the entire stockroom was on the road to San Diego. I left the walls. I left no friends. The next morning I signed the final papers. Outside, I turned the key in my six-cylinder Ford Mustang and headed out. After three long and therapeutic days and 3000 miles, I was in San Diego.

At MPL things were normal — or what we laughingly and lovingly called normal.

Time passed, and then came a telephone call in the front office for R. J. Smith. “Mr. Smith, I am calling from and for the ‘Office of Naval Research’ to thank you for your recent efforts on our behalf. You have saved this office a great deal of time, money, and political maneuvering. Again, our thanks.”

“Thank you, sir. Please tell my boss.”

I bled for only a little while.

No hay mas.

A Beginning in Undersea Research

Fred Noel Spiess

When I came to MPL in 1952 the Lab's ties to the Navy were still very strong, as well as the ties of the Navy to us. Although UCDWR was born out of antisubmarine warfare motivation, it was significantly involved in pro-submarine activities. For example, I have been told that UCDWR was the organization that developed the scanning sonar used by U.S. submarines to penetrate the minefields that were supposed to keep non-Japanese craft out of the Sea of Japan, and did until late 1944.

Following the pro-sub path, MPL had a major project, directed in early 1952 by Leonard Liebermann, to make it possible for our submarines to detect snorkeling submarines or surface-running ships at ranges of the order of 30 miles. The origin of this project is complex, and probably best known to Leonard. One element of its beginning is clearly spelled out in the summary technical report of UCDWR, assembled by Eckart (*Principles and Applications of Underwater Sound*, pp. 231 and 242). The MPL approach was to adapt for submarine use techniques similar to those being developed by Bell Telephone Labs (BTL), Navy Underwater Sound Lab (USNUSL) and others in a much larger program using seafloor-mounted hydrophone arrays — the undersea hydrophone systems (SOSUS) that MPL scientists and others are using today to listen to earthquakes, volcanic eruptions and whales.

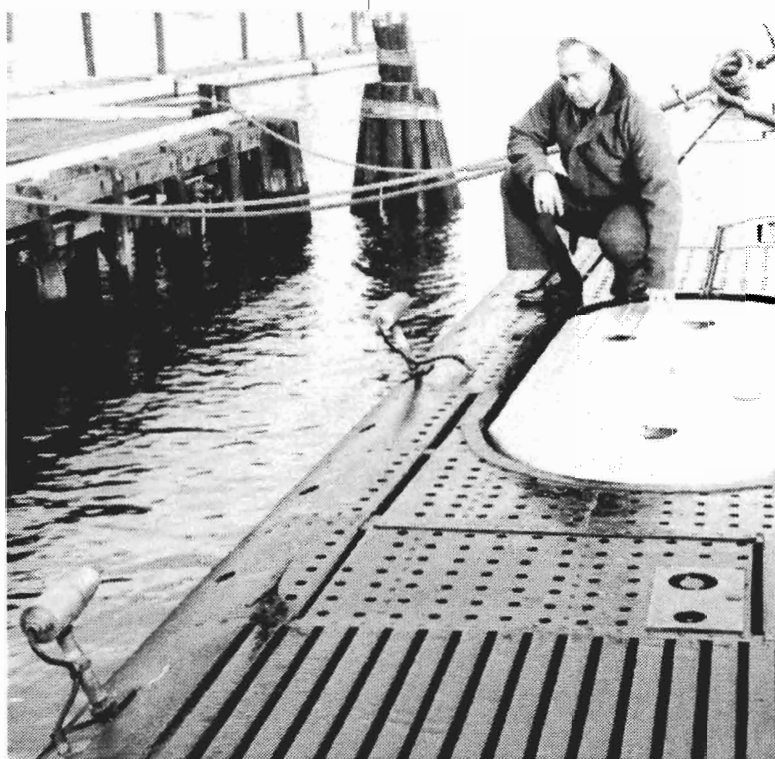
I arrived at a time of transition in MPL's development. Leonard Liebermann had been awarded a fellowship to go to Yale to work in the area of physical acoustics — an extension of research on sound absorption that he had started after joining the Lab in 1947. Shortly after I arrived, Carl Eckart decided to take leave from his position as MPL's founding Director and go on sabbatical to the Institute for Advanced Study in Princeton. I was thus recruited by Carl and Roger Revelle to take over Liebermann's long-range listening project, and, shortly thereafter, Sir Charles Wright came on board as Acting Director, replacing Eckart (a replacement that lasted longer than anti-

pated, since Carl decided not to resume the MPL directorship upon his return from leave). Thus there was a substantial changing of the guard.

My decision to leave nuclear physics, in which I had only recently finished my Ph.D. at Berkeley, was not a difficult one, since, after working all of World War II as a submarine officer, I had remained active in the submarine reserve during graduate student days, and still felt a strong tie to the ocean as well as to the rest of the dolphin-wearing community. My slight twinge of conscience about leaving “real” physics was substantially reduced by the fact that a far more eminent physicist, Eckart (whose name I had known for many years on the basis of his 1920’s proof of the equivalence of the Schrödinger and Heisenberg formulations of quantum mechanics), was one of those encouraging me to take that step.

In taking over the Long Range Listening project (no acronym was generated for it), I had the best possible entry into the world of undersea research. I knew more than the others about submarines, but they had been doing real, competent at-sea work for some time. The group included Stan Lai, Maurice McGehee, and two graduate students — Vic Anderson and Bill Whitney. Stan and Vic had been at MPL for 5 years by then, and Maurice and Bill had a few years each. They were great, patient teachers for me.

The MPL interaction with the Navy was at a level that has long since disappeared. At that time the Commander Submarines Pacific Fleet was Admiral Charles Momsen — developer of the Momsen Lung (that I had used in submarine training) and head of the Navy’s experimental diving unit at the time of the 1939 *Squalus* rescue operation, one of the first times mixed gas was used in operational diving. Momsen was an enthusiastic supporter of research and had established a set of research projects in which his submarines could be involved. Our long-range listening development was one of these. This meant that one of the Submarine Division Commanders in San Diego was assigned to us as project



Hydrophone array mounted along deck of Sub 242 for Long Range Listening project.

officer, and we could negotiate use of submarines directly through him.

In our case that meant that we could install a set of hydrophones in the submarine's superstructure — order of 15 at intervals along the entire length of the boat; we were working at low frequencies, thus that spacing was quite appropriate. Our shop, led by Archie Dunlap, was allowed to modify a conventional hatch cover that we used on each of several submarines to replace the forward torpedo-loading hatch cover. It had enough stuffing tubes in it to let us bring in a separate lead from each hydrophone. The forward torpedo room was our laboratory. We mounted our electronics and processing gear in a series of small racks that were in turn mounted in a torpedo-loading skid, making good use of the 20 linear feet of waist-high equivalent of bench space. Inside the forward torpedo room we had our individual hydrophone amplifiers, whose outputs were then combined in a beamformer to concentrate sensitivity in any of a wide range of directions. The beamformer output was then in turn fed into a signal processor on loan from the BTL/USNUSL project. Our experiments to determine detection ranges required use for several days at a time of both our submarine and a target snorkeling submarine, well away from San Diego. Both boats were under our operational control — obviously a real commitment on the part of the submarine force.

Since submarines generated quite a bit of noise in the frequency bands of interest, during these operations our submarine could hear most effectively when hovering in ultra-quiet (no air conditioning; steering and diving planes in hand power, etc.) — essentially the condition that some of us had been used to while under depth charge attack 10 years earlier. This was one place where the fact that I had been a wartime participant helped greatly — the submarine commanding officers were willing to hold this condition for us until the temperatures in the refrigerated spaces began to rise close to the limit.

Our project was successful enough that I went on active duty for three months in 1953 on the submarine *Blackfin*, operating our installation for an intelligence patrol in the western Pacific. This was an interesting run for me — I was senior to the CO, and it was my first extended snorkeling operation. My nuclear physics background came in handy, since two of the officers on board were being considered for assignment to *Nautilus* (both

were so assigned and made the first undersea visit to the North Pole).

This period also put me much more deeply in touch with the emphasis on the sub vs. sub warfare that was developing fast in those days. This background paid off later as we moved into development of a bearings-only target-motion analysis method (submariners' "Spiess Plot"), sub-to-sub communications and the long-range fire control problems of the SubRoc missile development — the source of the initial funding for FLIP and Deep Tow.

The signal processing aspects of this project started for us with the beamforming problem. Initially we used a single delay line in which the elements were all summed to produce a single beam. This approach was much like what we had learned from the German World War II sonars (we even had one of their delay lines in the Lab). It was clear from early on, however, that a device that could produce multiple beams simultaneously was very desirable, if only because one could then look for an output on one beam that was different from the others. Ingenious Anderson came up with a dielectric recorder version, rather similar to a multibeam approach being applied by others using a magnetic recording drum. We (Vic, Bill Whitney and I) presented a 1955 paper at an Underwater Sound Symposium in which we pointed out a variety of approaches to building beamformers. Not all were easily realized at the time. In particular we outlined an approach that used a simple binary digital representation of the signals. It would have required so many vacuum tubes to assemble a useful version that we joked that it would melt the submarine's hull if really built. This concept was revisited later when we had learned how to work with transistors, and Vic built the first one in connection with an ambient-noise measurement system (the Great Stellated Icosahedron — Dan Gibson's story includes the final chapter on this one). DIMUS (Digital Multibeam Steering) was thus born and, as Vic describes in his account, went on to great things.

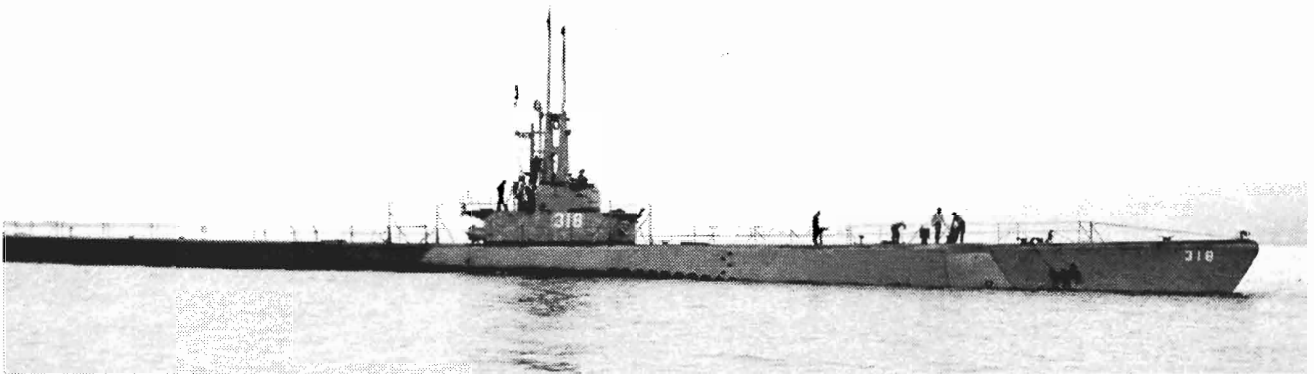
The days of direct cooperation between the operating forces and MPL (except occasionally with Submarine Development Group I) ended in the 1950's. After that the most effective means for interaction between the research people and the ship operators was through participation in advisory committees and workshops involving the lead people in labs such as MPL and

Naval officers and the civilian scientists in the government labs. Through the 60's and 70's these lines of communication were very fruitful for both the Navy and MPL; the number of committees and workshops in which Vic and I took part from 1955 to 1975 is too large to count, but the payoff in knowing what was important was very good for both MPL and the Navy. In addition there was a series of knowledgeable, imaginative naval officers assigned to help the entire Lab (and other similar labs) be productive. At our MPL 30th birthday, then Chief of Naval Research RADM Robert Geiger noted: "Even in comparison with some of the Navy's in-house laboratories, MPL has kept in close contact with the Navy's problems and needs." Gradually, however, increasing administrative barriers were built that have weakened these links. The days of comprehensive interaction with Charlie Bishop, Swede Momsen, Jr., John Bajus, Beau Buck, are long gone, leaving us today primarily with a small number of high-level committees, and fragmented direct relationships between individuals and particular program officers.

The Value of MPL to the Navy

Charles B. Bishop

The principal reason for the creation of MPL in 1946 was the recognition by the Navy of the valuable work done by its predecessor, UCDWR (University of California Division of War Research). The Navy wanted to keep groups of academic researchers and engineers interested in Navy problems, and for this reason established the Office of Naval Research in 1946, and made arrangements with several universities to support research laboratories. The Marine Physical Laboratory was one of the first to work with the Navy on the solution of undersea warfare problems. In the ensuing years, MPL has contributed to the advancement of the Navy's capabilities in both pro- and anti-submarine warfare through the extension of knowledge about the marine environment as well as through the development of engineering devices and techniques.



My personal remembrances of MPL's research activities began with my first task at sea in command of USS *Baya* (AGSS-318) in 1953. She was a World War II submarine assigned to research support duties and based in San Diego at the Naval Electronics Laboratory. Dr. Philip Rudnick of MPL came aboard to install a special electrode which was to be streamed by cable from an after-torpedo tube while *Baya* proceeded submerged under the Scripps Institution ship *E. W. Scripps*, a converted sailing yacht, from which Phil would measure electromagnetic propagation through the seawater. All preparations were made and we were ready to dive to start the test when I came to the bridge through the conning tower and noticed that *Baya* was

USS *Baya*, circa 1953.

backing down. Ordering “ALL STOP,” I then asked the OOD (officer of the deck) why we were backing, and was told that it was to adjust our position before diving. As it turned out, we had plenty of time to do that, since we had to stream another electrode to replace the one that we had just cut off with our propellers! Wasn’t it nice that Phil had thought to bring along a spare electrode? This was an early example of the quality of MPL’s seagoing experimental work.

Several months later Dr. Fred Spiess came aboard with a device called a Vening-Meinesz gravimeter. The first test was to be made near Jasper Seamount, the top of which is a few hundred feet below the sea surface about 200 miles from San Diego. Fred planned to anchor a buoy on top of the seamount to use as a reference point. The anchor was an old railroad wheel connected to the buoy by a long steel wire. Without davits and cranes, deploying such a rig from a submarine is no piece of cake, but we figured out a way to do it. We didn’t have GPS (Global Positioning System) in those days, so finding the top of the seamount by celestial navigation and fathometer wasn’t easy, but we did it. Over went the railroad wheel, followed by the wire and then the buoy — which all kept going as the wheel rolled down the side of the seamount. At this point, *Baya* wasn’t doing too well in support of MPL’s sea-going research!

Some time later Chris Harrison from England joined with Fred Spiess, and they took the gravimeter to sea in *Baya* for several measurements close enough to San Clemente Island to get good navigation. In order for the measurements of gravity to be useful, the vertical accelerations of the platform had to be almost zero, which is why a submarine was used for the job. This meant that *Baya* had to maintain depth control within inches for long periods of time. By rotating diving officers and planesmen between tests we were able to have a great competition, which gave the desired results. Submariners just like to drive submarines!

In 1954 I took command of *Bashaw* (SSK-241) in San Diego, and was busy helping to develop anti-submarine tactics, which were limited by our inability to communicate under water with other SSKs. There was no equipment available to do the job, so we had to improvise. On *Bashaw* we developed “Method Slugger,” which had a brawny torpedoman pound on the hull in the forward torpedo room. The other SSK would time the sound arrivals to get the range between us. This worked all right, but it was bending the hull frames, so we needed a new idea. Sure enough, Fred Spiess came up with SPUME, the Short Pulse Underwater Message system, which we took to sea for some of its earliest tests. While it was technically OK, it suffered from

multipath interference, which led to another acoustic propagation investigation at MPL.

In 1955 I went to Washington for shore duty in the Office of Naval Research, Undersea Warfare Branch. My experiences with MPL and NEL were very helpful in understanding and assessing the research activities of the many academic and commercial contractors who came to ONR for support. I worked with Fred Spiess on the Long Range Propagation Group, which brought together oceanographic scientists to work on underwater acoustic problems for the Navy. One problem involved the development of high-power active surveillance sonar, which resulted in the creation of the Trident Project, in which Vic Anderson played a large part. At that time, Vic had developed the DELTIC signal processor and was having trouble getting it understood in the Bureau of Ships. Fortunately, it caught on, which resulted in DIMUS, which revolutionized sonar capability. Another joint effort with Fred Spiess was on the SUBROC Technical Advisory Group, which initiated the requirement for a stable research platform for accurate acoustic directional measurements at sea. This generated the funding for FLIP and SPAR, and supported the tests at sea that proved the accuracy of submarine acoustic fire control data.

In 1963 I was back in Washington in the Submarine Research & Development Office in the Office of the Chief of Naval Operations. When the nuclear submarine *Thresher* sank, I became co-chairman of the Technical Advisory Group supporting the Navy search operations. We had to find any and all possible equipment and techniques that could be used to search the deep-sea floor to find the submarine. Sure enough, Fred Spiess had some good ideas and was very helpful in evaluating the many schemes and gadgets that were offered. In particular, after many weeks of fruitless searching, one of the engineers from the undersea salvage community recommended dragging a heavy chain in a spiral search of the bottom. This might have caught some wreckage, but it would surely obliterate any scars of other traces that parts of the submarine might have made on the bottom. Fred Spiess was a strong supporter of avoiding that approach and for keeping the bottom clear for scientific search techniques, which eventually led to success.

These memories illustrate the value to the Navy of MPL, not only in the quality and relevance of its research programs, but also in its service to the Navy as advisor and partner in solving difficult undersea problems. MPL's success for these 50 years has been the result of continuing recognition by the Navy of MPL's capabilities, which come from the inspired persistence of its scientists, the thoroughness of its engineers, and the high quality performance of its support personnel.

The Outhouse

Fred Fisher

When I first came to MPL in April, 1955, to start on my second research project for my Ph.D. thesis for the University of Washington Physics Department under Leonard Liebermann in Building 106, I began my experimental work in what is now the MPL conference room and library. At this time both Bill Whitney and Maurice McGehee were working in the same area as part of Fred Spiess's group. Some time during the year that I did lab work in Building 106, McGehee received a telephone relay rack, a six-foot tall one, that was shipped in a very nice wooden crate. Shortly thereafter, we were joking around one day, and McGehee and I painted a black crescent at the top of the crate when it was standing up and put hinges on so that it opened like a door to an outhouse. Inside the box we placed some kind of fire hydrant-looking fixture and then moved it from the lab into McGehee's office. We thought it was pretty funny and awaited a visit from Fred Spiess to see what his reaction might be. His reaction, if any, was never made known to us. (I wonder if he even remembers the incident.) Anyway, McGehee and I had a lot of fun with it for quite a while as a conversation piece.

Exploring the Gulf of Alaska and Beyond

George G. Shor, Jr.

I began at MPL in 1953, to work with Russ Raitt, who was then working on data from the earlier Midpac and Capricorn expeditions. Rather quickly, Russ turned over to me all of the logistics, instrumentation, and similar work. All we were lacking were major sea trips on which to go out and use the equipment, and I didn't really know how to create those major projects. Earlier major SIO trips (such as Midpac and Capricorn) had been organized by the Director, as far as I knew. How to get out to the deep ocean to explore? My interest has always been in exploration. The only projects that we were able to bootstrap then were local trips to the area off southern California and Baja California.

Russ took a leave of absence in 1955, to go to (among other places) Algeria, to teach a French geophysical company how to do seismic-refraction surveying, leaving me somewhat at loose ends. During that year I was invited to a meeting "topside" at NEL (Navy Electronics Laboratory), which unexpectedly became the starting point for me and others at MPL for a continuing series of projects to explore the oceans. At the meeting were a group from the Office of Naval Research, who had something of interest to say. Invited to it were quite a few group leaders from NEL, and from MPL and other parts of Scripps Institution of Oceanography.

The conveners informed us that the Navy was interested in environmental information from the northeast Pacific and the Gulf of Alaska and the Bering Sea, and they wanted to know what kinds of observations we thought should be made. Suggestions from a number of people present included: measurements of bathymetry, crustal structure, background noise, deep currents, shallow currents, and a host of other things. After considerable discussion, the ONR people revealed that they had a considerable sum of money that needed to be committed for this work, and it could support two field seasons in the Gulf of Alaska and the Bering Sea, but they wanted to know who would coordinate the effort. At that point, everyone sat on his hands; there were no volunteers. Feeling a bit junior, but wanting desperately to explore these unknown regions, I finally stood up and said that I would

coordinate it if nobody else would volunteer. I was elected by default.

We scheduled the first work for the summer of 1956, and were given as much time as needed on the *Spencer F. Baird* and the *Stranger*. As I remember the funding, it was several hundred thousand dollars for each year (which went a long way in 1956). I appointed graduate student Robert Hurley as my assistant, I co-opted the services of Max Silverman from NEL, and we started putting together the program and buying equipment. I had been struggling to build what later became known as a “gating box” (to gate out Raitt’s deep scattering layer so that we could see the bottom), to go with the old Navy EDO echosounders and the MPL homemade “precision time source” that we had been using for echosounding. We heard about the Mark V PDR, the first commercial precision depth recorder, which included gating and precision frequency. I still remember that, when I phoned Bernard Luskin at Times Facsimile to say that we wanted to buy two units, his voice broke when he said “Two?” It was their first commercial order. The devices proved to be absolutely indispensable for mapping on the abyssal plains of the Gulf of Alaska, although we did have to learn a few tricks to deal with “400-fathom errors.”

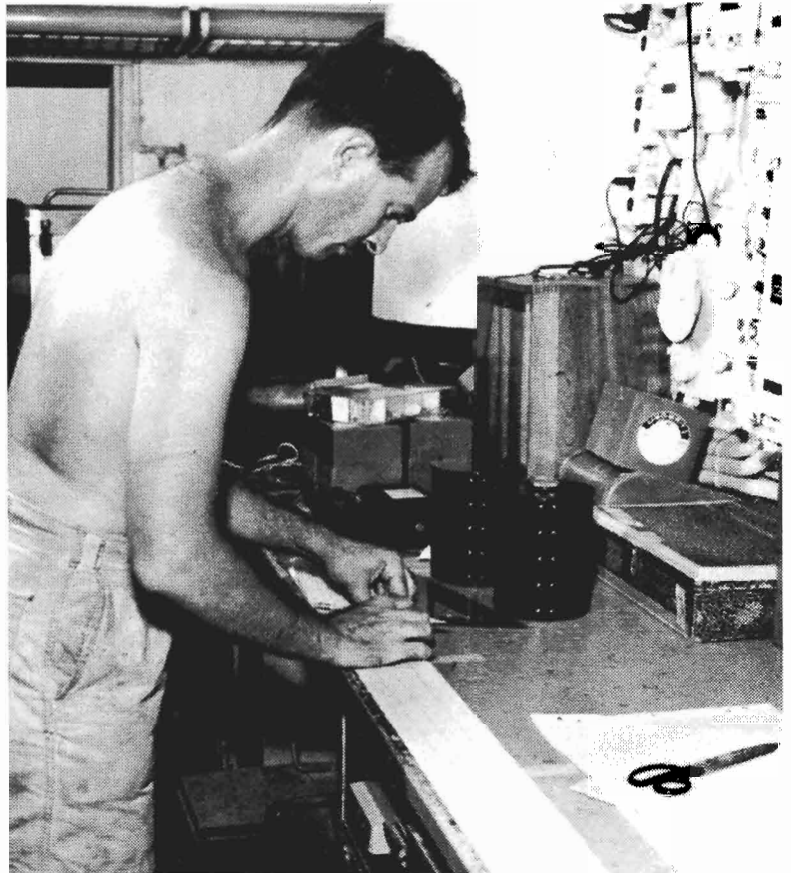
The first year’s program, Chinook Expedition, included two-ship seismic-refraction work, coring, dredging, ambient-noise measurements by a group from NEL, deep-current measurements by John Swallow from England (the first American use of the neutrally buoyant floats devised by Swallow) and John Knauss from SIO, deployment of taut-wire moorings for navigational reference and shallow-current measurements, hydrographic work, magnetometer towing, and other projects that I don’t remember. We built the taut-wire moorings out of used hotwater heaters and rather poor wire, we put together new dredges and new piston and gravity corers, and we assembled all of the other hardware. One of my problems before sailing was preventing theft of the pretty glass floats that were used in the “ball-breakers” on the cores; everyone wanted to take home one of each color for ornaments from the boxes sitting in my office.

On the first expedition we set a rather rigid schedule of work 24 hours a day, and most of the time we managed to stick to the schedule regardless of weather, going all the way up into the Bering Sea, and down past Adak to Hawaii. That was Chinook Expedition, the name of which is still on the bathymetric maps of the north Pacific as the mysterious Chinook Trough south of Adak. The Chinook Trough really confused us. The PDR had a 400-fathom expanded scale, and few choices of programming. If the depth changed abruptly by about 400 fathoms, one would get a short “white-out” of the record and then

an erroneous series of readings. Both ships zigzagged back and forth across the trough trying to figure out how deep the water really was, and whether there really was a linear deep exactly 400 fathoms deep. There was. We had a lot of fun and acquired a great quantity of data. Bob Hurley produced a Ph.D. thesis out of the bathymetry, and some of us resolved to go back again.

The next year ONR gave us the second increment of funds for Mukluk Expedition, which took us into the Bering Sea. In 1961 we obtained NSF support for Leapfrog Expedition (using the *Hugh M. Smith*, the *Stranger*, and the Canadian ships *Oshawa* and *Whitethroat*) and in 1964 for Kayak Expedition (using *Oconostota*), and subsequent cruises. We became well acquainted with such ports as Prince Rupert, Ketchikan, Kodiak, and Adak, the last known as “The Emerald Isle washed by the cool blue waters of the Bering Sea” (ocean temperature about one degree centigrade). On one trip I organized a UC Extension course, Field Work in Oceanography, for a selected few applicants from anywhere in the U.S. I remember writing a letter to all of them in advance, telling what life was like aboard ship, including the statement: “We do station work from dawn to dark. In addition, you will stand one four-hour watch a day.” What I didn’t mention was the hours of “dark.” One student, after all day on a seismic station that ended in the dusk at midnight, followed by the midwatch from midnight to 4:00 a.m., and then the next station starting before 4:00 a.m. at dawn, told me, “George, you told the truth, but it was a lie just the same.”

Weather was a problem, because we had no good predictions. On one trip into the Bering Sea, each day’s weather report from the Navy station in Kodiak showed a storm exactly where we were operating. No matter what we did, the storm followed. It finally dawned on us: our afternoon weather report one day became the Navy’s predicted location for the storm the next morning, because nobody else was fool enough to be up there! On that trip we got so tired of bouncing around that both ships limped into a quiet inlet on Akun Island and dropped anchor. We went for a walk ashore while waiting for the weather to



George Shor splicing seismic records in ships' lab.

subside. With no more reports, the storm went away.

Another time we put in to Adak on July 4. We had been in a fog bank for days in the area south of the Aleutians. We came out of the fog as we came around through the pass to the harbor at Adak, and tied up to the dock. The scene was absolutely beautiful. The sun was shining, the sky was blue, and across the bay we could see Great Sitkin Island, snow-capped, with steam coming out of its volcanic crater. I commented to one of the locals on the dock how beautiful the view of Great Sitkin was; his grumpy answer was, "Yes, it's the first time we've been able to see it this year." Fog is a way of life there.

People asked me why I went to the Gulf of Alaska and Bering Sea every summer. I answered, "Because it's a hell of a place to go in the winter." Actually, the real reason was that I didn't have to write detailed proposals that listed all previous work in the area; there was practically no previous work, and anything that we found was interesting to oceanography and the Navy.

One of the long-term effects of this work on MPL was acquiring a tradition of being the capable organizing group for multi-disciplinary "mapping" operations, in addition to its previous "hypothesis testing" role. The advantage of MPL in this role, of course, was that we had a continuing experienced central support group, shop facilities, and an engineering staff — all coordinated by Finn Outler, and no hardware request was considered impossible.

Chinook Expedition, 1956

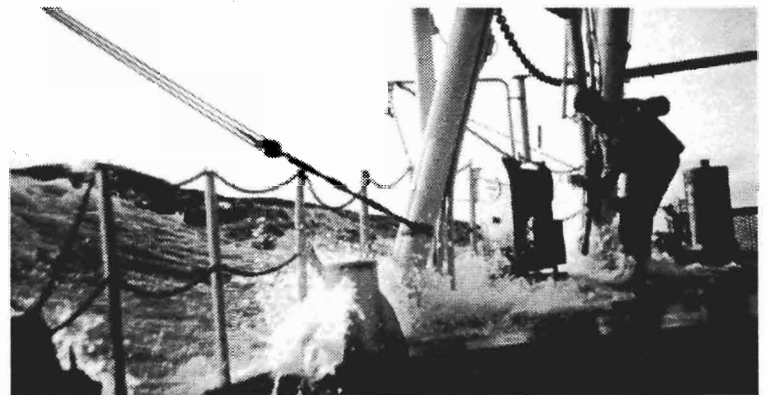
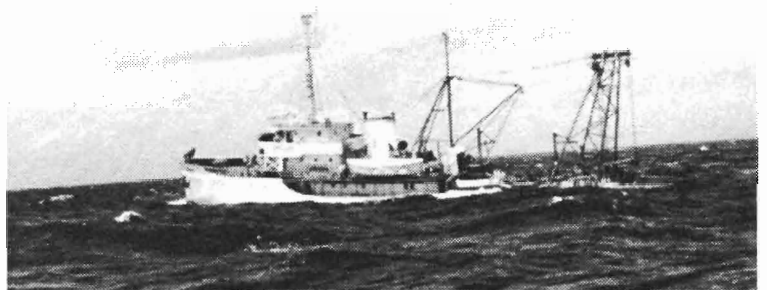
Alan C. Jones

(The following was written while on Chinook Expedition)

For the past three weeks we have been bounding over the cool, placid waters of the North Pacific. The good ship *Stranger* is fairly leaping from one crest to another. We are, no doubt, the envy of the many land-locked people back in San Diego. Just imagine yourself living the life of a carefree tourist aboard a yacht like the *Stranger*. This yacht was designed with great care to enhance the most restful of cruises. (Nobody told the designer that this luxury liner might someday leave Seattle yacht harbor and venture out onto the bounding main.)

As we waited at the dock back in that far-off port of San Diego, we were given a present of a hydraulic B.T. winch by the good people of Scripps Field Annex. This little gem was fully guaranteed to wake up every person aboard (including Joe) every two hours around the clock. It also ensured a good coat of oil on the teak-wood decks. Fortunately for the *Stranger* adventure fans, the damned thing didn't work, so at the last minute it was taken back by the good people of SFA. The crew finally managed to untie the knots that held us in San Diego, and away we rolled in the great yacht. It wasn't long before BTs, oscillators, and roast pork were flying all over, especially in the "living room."

Two days later a few of the *Stranger* Rangers emerged from the "basement" to become lounge lizards on the "sun porch." By the fourth day all were recovered from the strange ailment that hit almost everyone off Point Conception.



Spencer F. Baird, top. On board Baird, north Pacific, 1957, bottom.

Occasionally we stopped to throw instruments into the water, and chase ground loops by the hour. We still don't seem to be able to pick up KSON. When we get tired of this pastime, the Great Yellow Monster blasts off. It always sank into the ocean though. I feel certain that with a little different blend of day fuel it will fly fine. Every other day or so a strange white boat [*Spencer F. Baird*] comes up over the horizon, passes close by, and calls us nasty names. Such impudence!

Ah yes, it's hard to beat this gracious living out aboard a yacht: the gentle motion (rolling up to and including 52 degrees!); the attractive, spacious living room (if one is really determined he can crawl through the maze of wires and tubes that completely fill the labs!); and the delicious meals expertly cooked to please the slightest whim of the happy tourist. (I've been trying for three weeks to get a soft-boiled egg.)

It is very reassuring to know that we have aboard one of the most expert sawbones (or bone sawers) in the Scripps fleet. One of the *Stranger* Rangers forgot to bring his big foot in out of the cold when he gently closed the door. The result brought the ship's sawbones running to the scene with a gleam in his eye, and an accident denial form in his hand.

Then there was the time that one of the smaller adventurers (6-foot, 4-inch) became a free-floating object and flew across the living room, stopping suddenly against the TV set. (The fathometer was out of commission for four hours after that.) One fine moonless night the *Stranger* inmates had to scramble up the walls (or was it down?) to avoid being battered by flying objects when the yacht decided to lie on its side for a while. We were glad the yacht wasn't in the trough of the waves, because the sun porch might have gotten damper than it did.

On to Adak!

(The following was written many years later)

The *Stranger* was indeed a luxury yacht, used for many years for fun, frolic and wild parties before it was finally donated to Scripps Institution of Oceanography. It had a huge smokestack to keep smoke off the party-goers. This contributed to its top-heaviness. The propulsion consisted of two large, slow-speed diesel engines, directly connected to two propellers. When the captain was ready to go forward, compressed air was shot into the cylinders, and off we would go; there was no warm-up at the dock. To reverse, the valve trains were moved to make the engines rotate in the opposite direction. The *Stranger* had

other very unusual features. One was that it rolled considerably in a flat calm sea. Its own wake made it roll. The other was that no matter what the wind and sea, stopping the engines made the ship act like a sailboat, so it headed around and into the wind. This made it just fine for work on stations, but it was terrible running between stations in poor weather. After the episode mentioned above of not recovering before the next wave hit, the engines were immediately stopped and the ship slowly righted itself. After that we proceeded on very slowly until the sea calmed down. It was determined by the experts back home that the ship was unseaworthy. The smokestack should be cut way down and many tons of ballast should be added. On Chinook Expedition the ship continued at very reduced speed unless it was calm.

The *Stranger* was much easier to carry out work when in cold climates. Once, when we were south and heading toward the Hawaiian Islands, it became unbearably hot in the lab. This was in the days before transistors, and lots of vacuum tubes made things worse. Then there was the hydrographic winch mounted right over the lab. This really drove me crazy trying to work under it. I had to use lots of seasick pills on that expedition.

Operation HARDTACK I

W. Robert Cherry

Almost 40 years ago, in the winter of 1957 I was hired on to the MPL team that would participate in this nation's last atmospheric atomic tests. The operations, designated HARDTACK, were conducted in the Pacific Ocean (HARDTACK I) and Nevada (HARDTACK II) in 1958. Between May and August of that year for HARDTACK I there were 35 detonations: 23 at Enewetak (spelled Eniwetok at that time) Atoll, 10 at Bikini Atoll, and 2 rocket-launched shots from Johnson Island. This was indeed an ambitious undertaking — a shot almost every four days. Of the 23 Enewetak tests, two were underwater shots: Wahoo, which was detonated 16 May at a depth of 500 feet in 3200 feet of water, and Umbrella, detonated 9 June on the floor of the lagoon, in 180 feet of water. It was for these two that MPL was charged with instrumenting the water-wave phenomena resulting from the blasts.

This brief treatise is an attempt to fill a gap in the chronology of MPL projects, perhaps forgotten by some, or even unknown to others. Unfortunately, everything that I recount here is from memory, for all the data, reports, drawings, pictures and what-have-you have left the MPL/UCSD facility and gone to who-knows-where. Of our team of eight, only Jim Stewart and I survive.

We had basically two data-recording devices: Speedomax recorders, of which we had three; and six in-house underwater instruments named “turtles” because they resembled one.

The Speedomax recorders were paper-chart devices with an ink stylus (as I recall). They occupied about three cubic feet, and, if you picked them up off a bench, they weighed about 50 pounds. But, if you had to transfer them from a pitching deck to a not-so-pitching deck, they weighed probably in the order of 500 pounds. I quickly developed an acute dislike for those suckers!

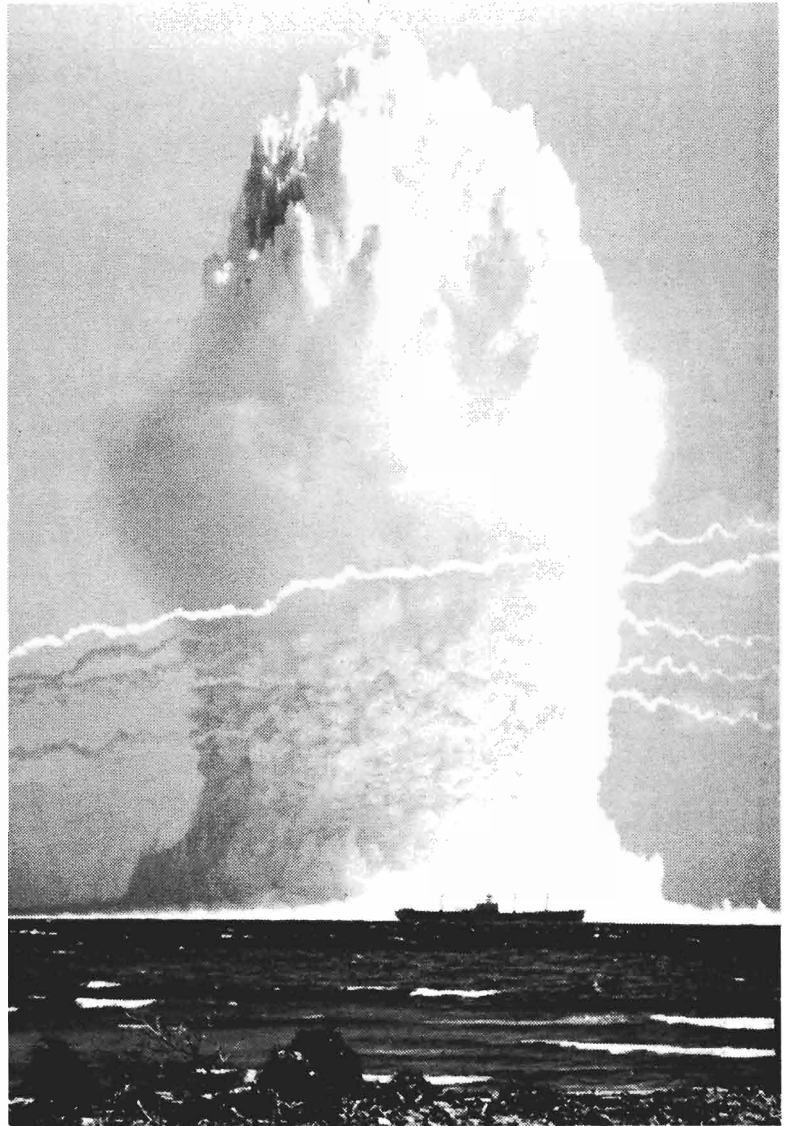
The turtles were pressure-cased instruments strategically placed on the bottom of the lagoon for the Umbrella shot. The heart of the units was a basic BT (bathythermograph) mechanism that detected changes in

the hydrostatic pressure and placed the data on a smoked-glass slide. After the shot the turtles were to be recovered — that is, once they were found — and returned to the beach to be read.

My task was to provide data on the pitch and roll of two sacrificial Navy destroyers that would be moored near ground zero, one for each shot. I had nothing to start with except the Speedomax recorders. After investigating and exhausting many avenues, I managed to borrow from North Island Naval Air Station three yaw-and-roll gyro instruments from the spare parts inventory for the Douglas Skyray interceptor aircraft. These instruments, when I rotated their base 90 degrees, instantly became pitch-and-roll gyros, and, since they conveniently had potentiometer outputs, I was in business. Three complete systems were built. All the significant sub-assemblies were shock-mounted in 3x4x8-foot, 3/4-inch marine plywood boxes, shellacked, with gasketed and secured tops. (None of us had any “blast” experience, so we didn’t know what to expect.) These instrument containers were enormously unwieldy and heavy; for one thing, each contained one of the aforementioned Speedomaxes! When we hauled them to Long Beach to load on the destroyers — alas, the two assigned to us were the outboard ones in a raft of four!

Skipping over the details of setting up on the atoll, our two shots went off on schedule and without a hitch. Were we successful? Yes, partially. Some of the devices worked, and some didn’t. No surprises here. I never saw the data worked up, nor did I ever see the final report. To this day, I wonder if anyone ever read it.

There are a few interesting personal experiences and observations of a non-technical nature that I recall. We flew to Enewetak via MATS (Military Air Transport Service). It was pretty exciting: I had never flown backwards before! All my life I had lived in the horse latitudes, and the wind always blew from the west. Enewetak is in the trade-winds belt, and for the first two months of my three-month stay I was



Defense Nuclear Agency Photograph taken 6-8-58 at Enewetak.

completely and constantly disoriented. Like — why does the sun rise in the west and set in the east? Our quarters were newly constructed, corrugated-aluminum four-room barracks — no glass windows, only big openings with aluminum shutters hinged at the top (always open) and without screens (no bugs). I had my first helicopter ride; that was the standard intra-island transportation. All water transportation to the many many ships was done by World War II LCM (Landing Craft Mechanized) boats, popularly called M-boats. It seemed that one could hear the distinctive sound of their twin diesels going some place 24 hours a day. The swimming was great, and at night were movies under many many stars. Once, standing in line for the noon meal, I made a startling scientific observation: I cast absolutely no shadow! In the shower room there was only one valve per shower head; in the morning the water was cool, but in late afternoon it was so hot you couldn't stand under it — solar heat before its time! I never ate an egg the entire time I was there; each Sunday morning as that huge plate of scrambled eggs was passed my way, I would always try but could only manage one bite. They were absolutely terrible. The first thing I did when I returned to Hickam Air Base was to order two fried eggs.

Finally, if you could divorce yourself from what was actually happening on this tiny atoll, the many pre-dawn shots that I witnessed were magnificent beyond description! I am grateful that I was given the opportunity to participate in these historic events.

DELTIC and DIMUS, Two Siblings

Victor C. Anderson

Once upon a time — more specifically in the early 50's, about the time I was trying to complete my thesis at the Marine Physical Laboratory — there was a beginning. It was the beginning of ideas that culminated in the birth of two siblings, DELTIC (DElay Line Time Compressor) and DIMUS (Digital Multibeam Steering). This is the story from beginning to end.

In the beginning I became interested in electrical beamforming techniques. The basic idea of beamforming is to delay the electrical signals from a distributed array of hydrophones by time delays that will compensate for the difference in arrival times of sound from a specific direction so that when the electrical signals are summed together that sound will be reinforced. That sum is referred to as a beam, analogous to the beam of light from a searchlight. In a passive sonar application, the average power out of a beam is compared with the average power from a beam in another direction to recognize the presence of a sound source such as the propeller of a ship. The longer the beam outputs can be averaged, the more sensitive is the detection. For a passive sonar to be most effective, it is necessary to look in all possible arrival directions at the same time for maximum averaging time. This requires a multibeam steering system.

The basic beamforming process consists of the operations of time delay, sum, detect, and average. To accomplish this the multiple array signals have to be stored in delay lines or recorded on tracks on a moving medium and then recovered or played back at different time delays. In order to listen on more than one beam at a time, for multibeam steering a set of playback circuits is required for each beam. The hardware rapidly escalates in complexity as the number of elements grows. For 30 elements and 30 beams, 900 playback circuits are required.

Magnetic recording was in vogue at that time, but playback heads were not small, and the magnetic coatings had a finite life for continuous use. For the purpose of generating time delays for beamforming, the long-life storage of magnetic recording was not required; a storage

time of just a few milliseconds would do. Dielectric recording, depositing an electrical surface charge on a moving insulator and reading it out with a capacitive probe, seemed to be a possibility. We tried it, and after some experimentation it worked. Using the dielectric recording technique, we built a 32-element, 32-beam rotating-drum multibeam steering system and used it at sea.

There was a problem with the dielectric recording technique: it was difficult to design the ionization recording head with enough conductivity to erase thoroughly the old signal while recording the new one. On the other hand, as it turned out, it was quite easy to reduce the conductivity and control the build-up and decay time of wave forms that were synchronous with the drum rotation period. By exploiting this characteristic, we developed the dielectric recorder into a new instrument: an analog synchronous averager with an adjustable averaging time for extracting periodic wave forms from a noise background. Unfortunately, we didn't have an application for it at that time.

That work with the beamformer and the dielectric recorder laid the foundation for the next development stage, the delay line time compressor DELTIC.

Faran and Hills at the Harvard Acoustics Research Laboratory had done a theoretical and experimental study of the clipper correlator which used only the polarity of the wave form. They showed that for signals in a random noise background the correlator processing gain using just the polarity of the wave form was only slightly lower than using the full amplitude range of the data. That study vindicated the use of single "bit" data, a "0" or "1", representing the positive or negative state of the wave form, for computing the correlation. In general, the correlation operation consists of multiplying two wave forms and averaging. However, in the case of the clipper correlator, where the wave form can have only values of +1 or -1, an exactly equivalent operation, except for a constant offset, is sum, detect and average.

If the correlator is used to process the outputs of a pair of separated hydrophones in the ocean, the output for a sound source is a maximum if a time delay corresponding to the difference in arrival time between the two hydrophones is introduced. Thus, in order to look in all directions, the correlation must be computed for all possible differential time delays. The display of correlation versus time delay is the correlation function. The computation for the clipper correlator is time-delay, sum, detect and average — the same as that for the

beamformer as mentioned above. So we come full circle: a correlation function generator is a multibeam steering system for a two-element array.

The rotating-drum recording medium that we used for the beamformer had an interesting analog in the ultrasonic delay line memories used in early computers. In these memory systems an ultrasonic pulse is transmitted into a facet on a block of fused quartz and then received after a travel time at another facet. The received pulse is detected, reshaped and reclocked, and then sent to the transmitter to be retransmitted. In this way a train of several thousand pulses representing digital information — 1's or 0's — can be recirculated indefinitely without loss of information. The intriguing thing was that the tens of megahertz data rates of these ultrasonic memories were a thousand times higher than the tens of kilohertz acoustic data rates with which we were working. This meant that, if the acoustic data samples could be loaded into the recirculating memory, the samples could be processed a thousand times faster than the rate at which the acoustic data were received. The time scale would have been compressed by a factor of a thousand, making it feasible to generate signal-processing functions such as correlation in real time.

In retrospect, it is hard to explain why it took so long to recognize the obvious way to load the slow acoustic data into the high-speed delay-line memory. If I had to pinpoint the flash of inspiration that marks invention, it was realizing that the obvious way to load the acoustic data is to replace the oldest sample each time it appears at the output of the delay-line memory with a new sample. That is how the DELTIC works. Each sample of the acoustic data remains in the high-speed recirculating replica for as many times as the number of samples stored in the memory. This means, for example, that, for a thousand samples stored, the high-speed replica data could be analyzed for a thousand different time delays in correlation processing.

It turned out that the DELTIC concept was ideally suited to carry out auto- or cross-correlation processing because the data precessed through the high-speed replica in a regular fashion so that, if the precessing data sequence would be compared with a stationary reference sequence, the displaced time increments for computing the correlation function would be inherent in the instrument.

There was a limitation to the DELTIC's signal-processing capability in extracting a signal from a noise background. The length of the stored signal would not provide an averaging time that was long enough to average out significantly the random noise. However, remember the

dielectric recorder problem that gave rise to a synchronous averager? That averager was an ideal match to the need for a longer averaging time, with the ability to average over times of from 0.1 second to 100 seconds.

Needless to say, I was excited and enthusiastic about the concept, and I prepared a paper on the DELTIC correlator for the U.S. Navy Underwater Sound Symposium in 1953. I'll never forget that session at that meeting. Before my paper came up on the program, Bob Isaac of the Navy Electronics Laboratory presented a paper on a secure acoustic communication system concept with experimental data analyzed off-line to demonstrate the principle. All that was missing was the technology of a real-time cross-correlator to build a prototype. Next, John Munson of the Naval Ordnance Laboratory presented a paper with experimental data and off-line processing on a wide-band passive acoustic-ranging system that just needed real-time cross-correlator technology to make it work. My excitement over the DELTIC concept reached its peak as my turn came up to present the paper on the real-time cross-correlator technology concept that they were seeking.

After the session both John and Bob came to me and said, "We need to talk." Talk we did — at length. Events moved rapidly after that. Professor Ted Hunt offered me a postdoctoral fellowship at the Harvard Acoustics Research Laboratory, where Faran and Hills had carried out their work on the clipper correlator. Development of a DELTIC correlator was launched on three fronts: myself at Harvard, John Munson at NOL, and Bob Isaac at NEL. Within a year the DELTIC was working and off and running as a signal-processing tool. The cooperation and interchange of ideas among the three groups materially shortened the development process and stimulated the integration of the DELTIC technique into the Navy's fleet.

As my year at Harvard drew to a close, I began to think back on our work with the multibeam steering system that processed more elements than the hydrophone pair that could be processed with the DELTIC correlator. The clipper correlator, using just the polarity of the wave form, worked so well in the DELTIC correlator that it seemed appropriate to try polarity processing for beamforming with a multi-element array. The time-delay, sum, detect and average process would clearly be an extension of the clipper correlator.

I ran some simple experiments with a 10-element array with hard-limiting amplifiers for the polarity of the wave form. I used the same multiple-speaker sound field set up in the Harvard Acoustics Research Lab anechoic sound chamber that Faran and Hills used in their clipper

correlator experiments. It worked, and the digital multibeam steering DIMUS concept was hatched. The concept was to use a single-bit shift-register delay line for each element signal and sum the appropriate delay tap of each delay line to form a beam. The summation would be done in a simple resistive adder network so any arbitrary number of different beams could be readily formed by using a separate adder for each beam.

My work with DELTIC was at its end (except for finishing the report). The concept had been proven and the technique passed on to programs within the Navy's research labs. Not to worry. DIMUS took over and dominated my interest with its development and application for many years.

Back again at MPL at the end of my Harvard fellowship, with the support of Dan Gibson and his group, we embarked on a DIMUS program in earnest. First we built a 32-element, 32-beam system for the same array that we had used for the dielectric drum beamformer earlier. Next came a passive DIMUS system for a destroyer SQS4 sonar. Then we designed and constructed two passive DIMUS sonar receivers for the submarine BQR2 sonar. One of these was shipped off to the New London Underwater Sound Laboratory as an experimental system to initiate development of an operational DIMUS submarine sonar system. We operated the other in detection experiments on several submarine sea trips. We also designed and built a unit for the Navy Electronics Lab LORAD program.

Based on the demonstrated performance of the DIMUS passive-sonar technique, we were funded to design, install, and operate a spherical volumetric passive-sonar array and DIMUS processor for the experimental submarine *Albacore*, operating out of Portsmouth, Maine. The array had 196 elements and the DIMUS formed 1200 beams in elevation and azimuth. That meant almost a quarter of a million resistors in the summing matrices for the beams. The DIMUS was the operational passive sonar for the *Albacore* and our MPL group maintained and operated it for a year during the boat's



Bob Cherry installing DIMUS on board Albacore.

sea operations out of Portsmouth Naval Shipyard.

Later we assembled a DIMUS for the ARTEMIS large-aperture array project in a cooperative experiment with John Munson of NOL, using his DELTIC correlator equipment for on-line adaptive beamforming.

Encouraged by that adaptive beamforming experiment, we designed and assembled an adaptive interference cancelling beamformer called DICANNE (Digital Interference Cancelling Adaptive Null Network Equipment). This involved a 31-element array of precision hydrophones mounted on the stern (bottom at sea) of FLIP, a magnetic-core memory that provided 256 time-delay steps for each of 32 channels, and logic and addressing circuits to select time-delay samples from each channel, which were combined to form an estimator beam trained on an interfering noise source. The estimator-beam wave form was subtracted from each channel sample to cancel the interfering noise and then reinserted in the memory to introduce a complementary delay to correct the original relative time base of each channel. The noise-cancelled outputs went to one of a pair of DIMUS beamformers; the other DIMUS was connected to the original uncanceled element channels for a side-by-side comparison of the effectiveness of the interference cancellation.

The DIMUS program at MPL culminated in the ADA (Acoustic Distribution Array) that was used to study the spatial and temporal statistics of background noise in the ocean. ADA was a very large array, 75 by 30 feet. The platform was like a barge. It was towed to sea behind the surface platform ORB and lowered on an electrical strain cable down to depths as great as 3000 feet. Mounted on the deck in a spatially tapered distribution were 720 hydrophone elements. The processing electronics and platform control circuits were enclosed in a walk-in pressure chamber, 60 feet long and 6 feet in diameter, designed to withstand the hydrostatic pressure at a depth of 3000 feet. This was the largest and most complex DIMUS system that we ever built. The 720 input channels were transformed into 1500 beams. The distribution of the beams was fully programmable over the umbilical cable, and the orientation of the beam set was electronically stabilized in both azimuth and elevation by reference to an on-board gyro.

Completion of the ADA project marked completion of MPL's DIMUS program. By this time we had used DIMUS processing extensively in our ambient-noise research, and had seen it accepted into the operational Navy's submarine sonar suites. It took a while, but we finally reached

THE END

MPL and ARTEMIS

Victor C. Anderson

One of the projects of the Office of Naval Research that MPL participated in was Project ARTEMIS. The main MPL participation was through myself as a member of the advisory committee and also through some hardware development with Dan Gibson's group. It is not my intent to discuss the ARTEMIS project as a whole, but rather to link project problems with the hardware development that took place at MPL.

Project ARTEMIS was an experimental multi-laboratory program initiated in the late 1950s to investigate the feasibility of very long-range active sonar for submarine surveillance. Unlike most project names, ARTEMIS was not an acronym. Artemis was the Greek goddess of the hunt, and was chosen as the project name because the concept behind the project originated with Professor Ted Hunt of the Harvard Acoustics Research Laboratory who proposed a system to search an ocean an hour.

There were a number of different committees coordinating the efforts of the various laboratories and contractors involved. I was a member of the Signal Processing Committee, which initiated the specifications and had the oversight of development contracts for the receiving array and associated signal-processing system.

The initial task of the Signal Processing Committee was to establish a basic configuration for a very large-aperture receiving array. The size dictated that it be a conformal array mounted on the seafloor. Also it had to be positioned within the water column at a depth near the axis of the deep sound channel and close enough to deep water to utilize purely refracted paths that did not reach the bottom. Bottom-reflected



Vic Anderson at workbench, circa 1961.

paths would exhibit too great a propagation loss and the reflection from the irregular bottom would scatter the acoustic waves and seriously degrade the coherence of any signals.

After studying available bathymetric charts, the group chose the island of Bermuda. The underwater terrain was a steep mountain slope at the edge of a large deep oceanic basin. A large area showing a nearly uniform slope on the bathymetric chart was selected as the site.

Now that the site was established, the Environmental Committee sponsored a new, more precise bathymetric survey of the site. The Signal Processing Committee undertook the definition of an appropriate array configuration that would match the acoustic characteristics of the long-range propagation paths and minimize the cable and installation requirements. The configuration that we chose was to use sub-array modules that selected the cluster of refracted long-range arrival directions and connect these module outputs to standard multiconductor underwater armored cables.

The initial installation concept was to pre-lay the cables equipped with connectors that could be mated under deep-sea pressures in situ. The modules would be dropped in free-fall to the bottom from shipboard. A bottom-oriented remotely operated crawler vehicle would then be used to retrieve the modules, position them, and mechanically mate the module connectors to the cable connectors.

The first important committee task in any major project is to coin an appropriate acronym. That we did: RUM for Remote Underwater Manipulator. With that task out of the way, we solicited bids for design of a RUM vehicle.

Concurrent with the design task, MPL was funded to build a "bread-board" RUM to try out some design concepts. We were facing a new technology: a remotely controlled unmanned deep-sea vehicle operating in contact with the seafloor.

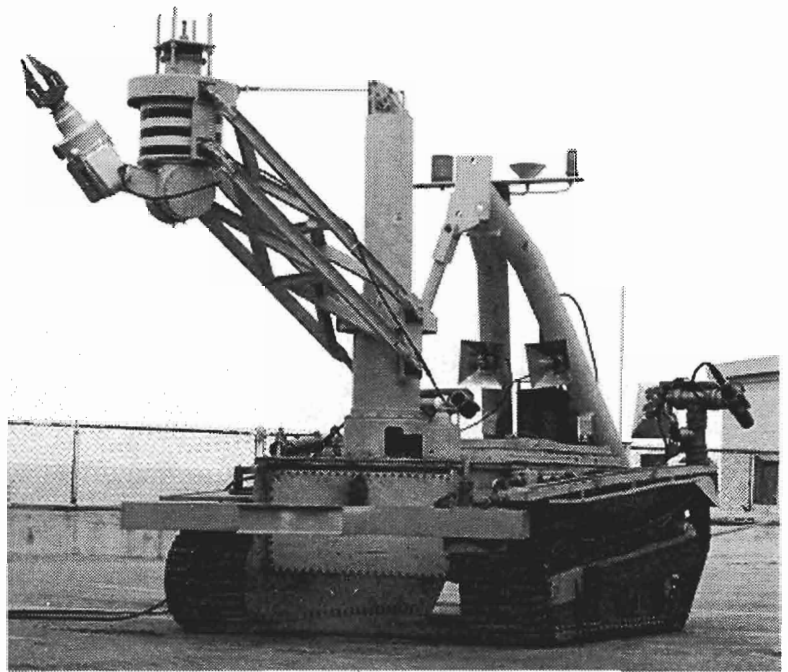
As a starting point in 1958 we selected a surplus lightweight tracked rifle, the Marine Corps "Ontos," as a chassis. Gutting it, we retained the tracks, drive gears, and the hull. This offered the lightest track pressure of existing military vehicles, a step in the right direction for the fragile sediments of the seafloor.

Our specific objective was to provide a vehicle for use in installation of the ARTEMIS array modules. This influenced our choice of power and control cable for the vehicle. We had two options. One was to

suspend RUM from a surface ship on a high-strength umbilical cable and maintain the ship on station while operating RUM on the seafloor. The other was to use a lightweight unarmored cable stored on the vehicle, which could be laid on the seafloor behind RUM. The latter option was our choice, and we designed a lightweight coaxial cable for power and control and stored a five-mile length on a constant-tension drum on RUM.

One of the concepts we were interested in was the use of ambient-pressure electrical and mechanical systems that were protected from the seawater environment by a non-conducting oil fluid but were subjected to the full ambient pressure of several thousand psi. Jim Snodgrass at SIO had experience with operating small d.c. electric motors in oil by using increased brush pressure to squeeze out the insulating oil film between the brush and commutator. We applied this technique successfully to a pair of five-horsepower d.c. electric-drive motors to provide the variable speed control for the individual tracks.

In order for any effective work to be done, a manipulator hand and arm would be required. At that time there was not a deep-sea manipulator arm on the market. The closest thing to it was a sealed manipulator arm used in “swimming-pool” reactor environments. These swimming-pool manipulators could operate in shallow water, but their seals and case could not withstand deep-sea pressures. General Mills was a major supplier of manipulators and, after considerable discussion, we convinced them to modify their electric actuator motors to operate in oil so that they could fill the sealed case with oil that was pressure-equalized to the ambient deep-sea pressure. G.M. did that and shipped us the first of a family of deep-sea manipulators that found application on later deep-submersible vehicles. That first model-500 manipulator we procured was the one that we used for many years in the MPL RUM projects.



The first RUM.

Early in the game, we thought that a public demonstration of our new underwater track propulsion system would be a good idea. What a disaster! We had the basic chassis with motors installed and power and control supplied by a multiconductor cable, but no television or sonar to let us know what was going on. Nevertheless, on the occasion

of a site visit by Navy brass in May 1959, we ran the RUM chassis into the bay from shore nicely. Then it was time to turn around and return to shore, so forward on one track and reverse on the other to turn around smartly — right? Wrong! That was not the right maneuver. Nothing happened. RUM didn't reappear. In fact, we soon realized that RUM wasn't even moving, so we sent a diver down to see what had happened. He found that RUM had managed to dig a hole and settle into it to the bottom of the chassis. Then the sediment that had been disturbed settled back around the tracks, locking them in place so that they couldn't move. Highly embarrassed and with some lame excuse, we dismissed the site visitors to their next demonstration, which I'm sure was more successful. It took several days to recover RUM from its embedment in the bay bottom.

Everything that we installed on RUM had to be designed or adapted for deep-sea operation: sonar, slow-scan television, lights, compass, pitch and tilt meters, camera booms, manipulator boom and turret, and the telemetry multiplexed on the coaxial power cable.

Eventually everything was checked out and then, a year after its first baptism in the bay, RUM and its control van were moved out to SIO in May 1960 for tests off the beach north of the Scripps Pier. With considerable apprehension, we pointed RUM to the sea and got underway. We knew that we would be in deep trouble if RUM had an on-board failure that disabled the propulsion system. There was no way that the oil-filled electrical compartments could be serviced under seawater. The surf zone was no place for a tow truck, and it was too shallow and hazardous for a ship of a size that could recover RUM. We did have a failure in the power system, but fortunately it was in the control van on shore. The failure occurred at the worst time, when RUM was in the active surf zone. Our concern for the several hours it took for repair was that the surf would scour down RUM's tracks into the sand and RUM would be embedded in the sand the way it had been in the bay sediments. No problem: RUM remained free and mobile after the repair, and we carried out several excursions through the surf out into deeper water. Our breadboard RUM was working and ready for a task.

Meanwhile, back on the ARTEMIS project, the Environmental Committee had sponsored another survey of the site, and the contours didn't appear to be nearly as smooth as on the original chart. They went back with higher precision echo-sounding equipment and found even rougher terrain. Finally, they resorted to sampling the terrain with deep-sea camera equipment and found that the underwater mountain slope that we had chosen for a site was anything but smooth.

There were overhanging cliffs 20 to 50 feet high, rocky outcroppings and crevices that presented insurmountable obstacles for a bottom-crawling vehicle.

The result was a shift of gears for the RUM project. The design contract was modified to include a helicopter type of vertical lifting attachment to RUM that would enable it to fly over obstacles and crevices and to negotiate down slopes.

At MPL we resorted to the alternate cable configuration. Instead of the cable being stored on RUM, the vehicle would be suspended on an umbilical cable from a surface vessel. The breadboard RUM activity at MPL now shifted to include the design and fabrication of a surface-support barge, ORB (Ocean Research Buoy), that could operate RUM on the end of an armored umbilical cable. The tension on the cable was controlled by a cable-accumulator system that could accommodate the surface-wave motion of ORB and decouple that motion from RUM when it was on the bottom. There was another advantage to the ORB system. Now that there was a high-strength strain cable attached to RUM, we would be able to recover the vehicle readily if — or should I say when? — an electrical failure occurred on board RUM. A further advantage was that, by adjusting the tension on the cable accumulator, we could vary the track pressure to allow operation in very soft deep-sea sediment areas.

The RUM/ORB combination worked out quite well. ORB could be towed out to station and moored in place, using the same deep-sea mooring lines and tackle that had been developed for FLIP. RUM could then be lowered through the central well of ORB down to the seafloor.

It took a number of sea trips to work out all of the “bugs” in the launch-and-recovery procedures and in the electronics and control circuits. I recall one particularly frustrating bug that we found. After several days of operation in shallow water where divers could observe RUM in action, we moved on out to deeper water for further tests. All went smoothly, checking out the different systems until we reached a depth of about 300 feet. At that depth we turned on several actuator motors, but they wouldn't turn off. We turned off the power to RUM, recovered the vehicle, and opened the electronic compartment. What we found was an accumulation of carbon on the contacts of the relays. We cleaned them up and tried again. At 300 feet the same thing happened again. It was difficult to understand because there had been no problems with the relays in our shallow-water tests. Back at the lab we ran tests in the pressure chamber and repeated the effect. It turned

out that when the relays operated in oil at lower pressures, the explosive nature of the electric arc on breaking the circuit extinguished the arc and blew away the carbon formed by breakdown of the oil in the heat of the arc. At higher pressures the arc was constrained and did not blow out, and so the carbon rapidly built a conducting bridge across the contacts even though they were open. Once we understood the problem we solved it by using solid-state semiconductor switches for the lower currents and larger solenoid-actuated electrical contactors with their larger gaps and more forceful action for the higher currents.

With the new RUM/ORB combination we had in hand an operational deep-seafloor vehicle system. Although RUM was never used for the ARTEMIS array installation, it continued to operate under various Navy, NSF and Sea Grant seafloor projects for several years.

The reason that RUM was not used for ARTEMIS was that an alternate method was developed to lay a set of sparsely populated module strings using a conventional cable-laying ship. This method allowed us to fill the array aperture with 10% of the ultimate design number of modules. That partial array was installed; a large quantity of data on deep-water propagation was collected and studies of the wave-front shapes of long-range arrivals were made. One thing that needed to be done was to establish the feasibility of actual beamforming, or coherently combining the full set of module outputs. MPL undertook that task.

This major signal-processing effort on the part of MPL consisted of the construction of a DIMUS multibeam processor, which was programmed for the partial-array module positions as determined by acoustic surveys. DIMUS formed only a small subset of beams out of the large number that would be required for the full system, but this subset could be oriented in any direction. Realizing that the location data were not as precise as required for beamforming and that there were uncertainties regarding the wave-front shape for arrivals from distant acoustic sources, we enlisted the cooperation of the Naval Ordnance Lab, in particular their DELTIC correlator hardware, to adjust the time delays in an adaptive beamformer configuration. Fortunately, things change slowly in the deep ocean, and we were able to adapt the delays manually to focus the beams. The technique we used was to correlate the beam-sum signal with each individual element signal to measure the delay-time error. We then dialed in an error-correcting time delay in each individual element channel. By iterating the process many times we built up a maximum response on one beam of the multibeam set and watched it progress through the beam set as the source moved.

The DIMUS experiment vindicated the feasibility of coherent processing of arrivals dispersed over the spatial extent of the full array aperture.

ARTEMIS terminated with the completion of experiments with the partial array. Our participation in the project had provided MPL with a seafloor work system and signal-processing experience that laid the foundation for future research projects.

Early Days of MPL

Christine Baldwin

In the early 1950s the personnel of the Marine Physical Laboratory were scattered in three separate buildings: 106 (main office and laboratories), 10W (electronics shop) and 120 (machine shop), all on the grounds of the Navy Electronics Laboratory at the waterfront. There was quite a bit of interaction with the NEL groups, the waterfront library and the Operations Office. Finn Outler, our Marine Technical Superintendent, obtained a number of bicycles from the government surplus agency for use of our personnel. Vic Anderson was riding one of these on his way to the machine shop when the front wheel dropped into a newly dug trench across the street. All 6' 4" of Vic went sailing over the handlebars and his face, chin, arms, hands and legs skidded to a grinding halt on the macadam road surface. One of the requirements of the Navy Electronics Laboratory was that any accident had to have a completed accident report filed at the Operations Office within 24 hours. The report (quite governmental and some four pages long) was no doubt designed to cover any incidents, large or small. When we finally got to the last page came the question, "How could this accident have been avoided?" Vic answered briefly, "By staying off a bicycle."

* * * * *

Two of the statisticians working for Dr. Raitt were Gloria Slack (now Cobarrubias) and Gwen Roy. Gwen was an excitable redhead who, like Gloria, could pound a Frieden calculator for hours on end. Vic Anderson had obtained (he may have fabricated it) a time-delay adapter which he plugged into the wall-socket which was out of sight beneath Gwen's desk and he then plugged in her Frieden. She was pounding away when all at once the machine quit. This particular machine had a habit of stopping every so often. Gwen would take a pencil and poke it into a hole in the back of the calculator and it would resume working again. This day she did that and it worked the first time. However, the calculator would only run for a short time and then stop. It would start again after a bit whether she poked it or not. Her irritability increased tremendously after each episode. By this time most of the office force was standing in the hall listening to what she thought of calculators in general and her Frieden in particular. By the

time Vic relented and removed the plug she was exhausted.

* * * * *

In the early 1950s the Marines guarded the gates to the Laboratory. We had to show our passes, and they could require us to open the trunks of our cars for inspection. Midge Kolesar (now Wing) was a shapely, long-legged, good-looking girl who had just purchased a new car. The Marines were quick to ask her nearly every night to open her trunk, or the hood, and they would also look in the back seat. As the days went by, her fuse got a little shorter, and on the day that one Marine asked her to take off her hubcap, she told him that if he wanted it off he could take it off — but she was going to report him to his superior officer. They never again requested her to comply with any inspection!

* * * * *

Dr. Spiess, our Director, was away on a sea trip for an extended period and the laboratory was quite busy and he was sorely missed. We got our photographer to enlarge a picture of Dr. Spiess that had been taken for the Acoustical Society into a poster size. The caption read “WANTED, ALIVE AND WELL.” It was posted on the wall beside his office. We were all watching when he came to work the morning after his return. With a perfectly straight face he went right past the picture and into his office. This went on for several days. The only way we knew he was aware of the poster was when Sally Spiess called and asked if she could have it!

* * * * *

Word had filtered out Wednesday from Washington, D.C. that there was some ONR money available for a project which was compatible with the work being done at MPL. The catch was that any proposal had to be in their hands by Thursday. We worked hard and long to get a proposal together — text, budget, and fitting into ship’s time frame — as well as reproduction. Time was running short, but we had visitors from the east coast. Al Vine (from Woods Hole) was asked if he was going to Washington. Yes, he said he was. He was asked if he would take the proposal to ONR for us. He said he would be glad to, as he had an appointment with the head of ONR. He was given the documents and taken to the airport. Early next morning there was a call from Al and he said he had the appointment for the following week at ONR. He was in the airport in Boston and after he looked at his ticket he realized that he indeed had been on a flight to Boston and not Washington, D.C. Al said if we would pay his airfare to Washington, he would deliver the



Chris Baldwin on board the T. Washington for an airgun test trip, 1966.

proposal personally. He was guaranteed airfare, taxi fare and eternal thanks. He flew, he delivered, and MPL got the funds!

* * * * *

MPL had just acquired a brand new state-of-the-art Thermofax machine. This machine took special paper, and a 220-volt system. The electricians from NEL installed the 220-volt outlet just before noon. They had left their equipment to pick up later. Bea Young and I decided that we would get the machine hooked up. The available rolling table it was to go on had a back that stuck up some six inches. The plug was about two and one half inches in diameter which pushed the machine out over the edge of the narrow table. We decided that if Bea held the table and I manned the heavy-duty drill the electricians had left, we could drill a slice out of the back of the table. We were hard at it (high heels and all) when Dr. Spiess came through with the visitor of the day, Admiral Momsen. Without batting an eye he said to the Admiral, "I told you that MPL employees are innovative."

* * * * *

When we were still over in Building 2W (now the Fleet Sonar School) my desk was just outside Dr. Raitt's office, and I answered his phone most of the time. He was known as "Uncle Fud" which was just an affectionate term most of us used in referring to him (thinking he was not aware of this). Imagine my surprise one day when I was late in answering the phone to hear him say "Fud here." Peter Bergman from the Columbia Division of NDRC was visiting one time and came in to see Russ. Russ had just had lunch and was leaning back, resting, in his swivel chair. Peter put his finger to his lips and tiptoed in, grabbed the back of the chair and swung it swiftly. Russ went round and round and Peter laughed and laughed. The next time Peter came to see us, Russ heard his voice and quietly went down the back stairs located next to his office so I could officially say he was out!

* * * * *

We had worked all day to get a proposal together so Dr. Anderson could take it back to Washington (he was flying the "red eye") that night. The final draft had been cut on stencils, proofed, and run. Drs. Spiess and Anderson were nervously watching as we laid out the 20-plus pages on the conference table. Three of us were gathering pages by going around the table. We were soon joined by both of them. Vic's steps were much out of sync with the rest of us, so he was put onto the stapling job, after Dr. Spiess had checked to make sure the pages were all there and in the right order. Vic roared off to the airport, proposal in hand. I believe his wife, Ann, met him there with his suitcase. It was teamwork all the way.

Finn Outler's secretary, Barbara Ness, was expecting a baby and had announced that she would be quitting in another month. Finn looked thoughtful, then said in his calm, controlled voice, that he had a solution which often worked in (his home state) Georgia. We could simply clean out the bottom file drawer, and put the baby in there. Barbara could then dip the baby's finger in molasses, then give the baby a feather and the baby would be so busy that she could work all day! Barbara did not buy this solution.

* * * * *

Sir Charles Wright had a unique way of solving problems. One of his solutions for approval, by the Navy, for our report distribution list was to invite Chesney Moe (the Navy representative stationed at NEL) to lunch, ask me to come and bring the distribution list. We would meet at a local restaurant, have a drink, a nice lunch, and Chesney would glance at the list, sign it approved by C. Moe. Much faster than the mail!

* * * * *

When MPL first moved over to Building 106, NEL had a small branch library in the waterfront. All the new material came to the main library topside. Dr. Liebermann had located a book he wanted at the main library and needed it immediately. He told me to take his car, a small two-seater, and go get the book. I could just reach the gas pedal and was unable to move the seat forward. About halfway up the steepest part of the hill, the seat just slid way back. I was hanging on to the steering wheel for dear life, peering under the rim of the steering wheel, and barely managing to keep my foot on the gas. The car made it, the book was delivered — and it was a much nicer trip back.

There's Always a Way Around the Rules

George G. Shor, Jr.

What's In a Name?

In the 1950s, when we were using a lot of explosives for refraction and reflection work on trips to the Gulf of Alaska and the Bering Sea, we had problems with “dud” shots when using small explosive charges. A shot that does not go off and continues to float can be a hazard to navigation, at least. Max Silverman suggested that we should try waterproofing the charges by stretching a condom over them. He found a drugstore on Point Loma that would give us a very good price on condoms in quantity; I wrote a requisition for a case (a great gross) of them: 1728 condoms.

The Purchasing Office at SIO declined the requisition. Their reason was not stated (perhaps the purchasing agent was embarrassed?). So, I rewrote the requisition for “1728 elastic waterproof covers,” giving the drug store as a source. Then there was no problem at all; the purchase went through.

As it turned out, they weren't really needed; the problem of “dud” explosives was a batch of defective fuse. Also, we lost a lot of them due to what in merchandising is called “stock shrinkage.” They were brought aboard the ship at the same time as a lot of the ship's dry stores, where the cooks spotted them, and they began to disappear.

Glass Balls

For one of those same Alaskan trips, we needed glass fishing floats (about 2 inches in diameter) that were used by the geologists in the “ball breaker” that signaled when a corer touched bottom. We used them both in the ball breaker and as floats. I checked prices, and found that one could get them in many different colors, or in “mixed colors” at a considerably lower price, so I ordered a case of fishing floats in “mixed colors.” The box of floats, along with many other things ordered for the trip, sat in a corner of my office. It had been opened for inspection. At some time before loading ship, I looked in

the box and found that most of them were gone! I knew that they were commonly used for “parlor decorations,” and I suddenly realized that many of my friends had helped themselves. The problem was that, because there were about 8 different colors, everyone took one of each color. I had to order another case, and this time we paid the higher price for a single color: blue. It was cheaper in the long run.

The Stolen Building

I once reported to the UCSD campus police that a building had been stolen. It had indeed disappeared, but it was actually a case of mistaken identity.

When UCSD was established in the early 1960s, and Camp Mathews was transferred to the campus by the Marine Corps, a lot of vacant buildings were left from the days of the marines. Like everyone, we on-campus MPL people were desperate for storage space for seagoing equipment. In the area now occupied by the UCSD Medical School were some long wooden buildings that contained nothing but toilets and urinals; they were the heads for the marine trainees who lived in tents. We were offered the use of these buildings, and I put in my claim for one. I was told the building number, the first in a long line of buildings, and I looked it over. Some time later, we decided to move some equipment out of lab space and put it into storage. I went up to check on the building — and it wasn’t there. Only the foundation was left! I promptly notified the campus police of the loss, which left them puzzled also.

The answer came in the following week. Some students had asked for one of the buildings, so that it could be moved to the Revelle campus and turned into a “Coffee Hut.” The Chancellor had approved the move and an unassigned building was designated for them. Then the house-movers arrived, accompanied by a man from Buildings and Grounds. The house-movers said that the buildings were so close together that they couldn’t take out the assigned building because they couldn’t get their equipment in. The man from B&G suggested that they take the identical building at the end of the row (mine). Then, he used some real initiative. Noting that the building numbers were on little wooden blocks nailed to each building, he pried off the blocks from the two identical buildings, and exchanged them. My building still existed on the records, down in the middle of the row, out of numerical order! The “Coffee Hut” is still there, now named the Che Cafe (on Scholars Drive South).

Seaweed Canyon Storage

This inspired us to a similar piece of chicanery. At a later date, when the Medical School got going, they proposed to tear down the “head” buildings to build a parking lot. We would have lost our storage. House moving was cheap in those days, so I proposed that we take some of those buildings and relocate them onto the SIO campus. I looked at many possible locations, and checked with the campus architects about each site in turn, and was always told: “That is the site of a planned future building.” Finally I asked about the old dump in Seaweed Canyon, a campus dump and also used by the city to dump seaweed scraped off La Jolla Shores beach, but which use had just been abandoned. The architects admitted that they couldn’t possibly build there because of the great depth of uncompacted fill. Without asking any more questions, I started the wheels turning.

MPL and the Ocean Research Division, including Finn Outler, Jim Faughn, and Dave Wirth, cooperated on this project. We selected a site on the dump, chose several of the old heads, and hired a house-moving company. Faughn found some money in ORD. Finn assigned the MPL carpenter to build forms for a large concrete slab, almost twice the area of the buildings, and then checked for anyone at MPL who had ever done any concrete finishing (there were a lot of them). The forms were built, the concrete trucks came, the slab was finished, and tie-downs were installed. Then the buildings were moved down and put on the slabs, and the MPL carpenter built roofs and walls between them, which nearly doubled the space. It is my recollection that the whole project cost about \$4500, about one dollar per square foot. The buildings, installed without any “site studies” or involvement by Campus Planning, Physical Plant, or the Architect’s Office, are still there; much later they were actually added to the campus space inventory, with no indication of how they came into existence.

A Saga from Graduate Student to FLIP

Fred Fisher

What the Hell is the Second Viscosity of Liquids? or
Squeezing Epsom Salts for 30 Years

My whole scientific career and personal life was determined the day I read Leonard Liebermann's 1949 paper, "The Second Viscosity of Liquids." This was some time in my second year (1950-51) as a graduate student in the Physics Department at the University of Washington. The graduate students' guide said in effect: "Thou shalt read the literature and find thyself a thesis project." Liebermann's paper was an intriguing experimental one that made use of Carl Eckart's paper on the theory of acoustic streaming, published in 1948. The forces generating acoustic streaming are proportional to the first and second coefficients of viscosity. Second, viscosity is also called a dilatational or bulk viscosity, in which a material has a frequency-dependent response in its behavior to pressure. That is, how it behaves depends upon how fast you squeeze it — similar to the behavior of silly putty. My idea was to investigate the possibility of measuring bulk viscosity effects in superfluid helium below the lambda point where the superfluid component of helium exhibits no first or shear viscosity. During September 1951, I visited professors Carl Eckart and Leonard Liebermann at Building 106, the headquarters of the Marine Physical Laboratory, who encouraged me in my efforts.

At the University of Washington my committee chairman was Prof. Ronald Beballe, who was in charge of the new cryostat for making liquid helium. After building the apparatus, I found out that I could never calibrate it, never getting the right reflection coefficient for the diaphragm that confined the streaming to a cylinder in which the ultrasonic crystal was mounted at the opposite end. By the end of summer 1954, I went down to UCLA to learn some more about acoustics, and I worked with Herman Medwin on acoustic streaming in gases. During this time Carl Eckart gave a lecture at UCLA, and I told him about my difficulties with calibrating the apparatus at Washington. He suggested I go down to San Diego and talk with Leonard Liebermann.

While at UCLA, I met my first wife Julie at the YWCA Co-op and by the second week of December we were engaged, after going out every night for six weeks.

In San Diego, I explained my difficulties in calibrating the streaming apparatus. Dr. Liebermann answered in seconds saying, "Of course, you have bubbles on the diaphragm!" At 2 megahertz, these bubbles are not visible to the naked eye, and I had no inkling regarding the cause of my troubles. I was crushed and depressed on hearing this, and said that I dreaded going back up to Washington. He asked if I could work at MPL with him for my doctoral thesis, and my chairman at Washington was agreeable to this suggestion.

I arrived at MPL in April 1955, and Julie and I were married in June after she finished at UCLA. She got a job at Convair as an engineering aid, took courses in math and also worked on her Ph.T. (Putting husband Through).

I started on a different project based on another paper of Dr. Liebermann, "Sound Absorption in Chemically Active Media," my introduction to squeezing epsom salts — magnesium sulfate, the cause of anomalous sound absorption in the ocean below 100 kilohertz. Prof. Bob Leonard at UCLA and his students had made this discovery in 1948. Since it was assumed that the absorption was due to a pressure-dependent chemical relaxation, the idea was to measure absorption as a function of pressure using a technique similar to that at UCLA, only using cylinders instead of spheres to measure the decay of resonance modes.

Archie Dunlap's Shop

In 1955, one of the first things I came to appreciate very quickly about MPL was the wonderful and talented people in the machine shop headed by Archie Dunlap. If memory serves me correctly, there were about six tool-and-die machinists there, the aristocrats of the trade.

As my troubles with blowing up high-pressure cylinders mounted, I kept on experimenting with different steels. To make my sound-absorption measurements up to 20,000 psi, I wanted the strongest steel so I could minimize cylinder wall losses and make measurements in low-concentration solutions of magnesium sulfate, which caused sound absorption in the ocean to increase by a factor of 30 at frequencies below 100 kHz.

One cylinder blew up at maximum pressure when my wife Julie was only a few feet away from it; fortunately no one was hurt, but room 3224 in Ritter Hall was decorated for years with white epsom-salt crystals on the ceiling. This was a very high-strength chrome vanadium alloy that I had to plate with silver to keep it from corroding. The silver-plating process had embrittled the steel, and I did not know that it had to be annealed to get rid of hydrogen embrittlement.

After another try using a nickel-plating process, courtesy of Ryan Aeronautical, which failed because of air between the nickel plating and the steel, I searched for other alloys with high strength and corrosion resistance. When I found an Armco stainless steel known as 17-4 PH, my troubles with explosions were over for a while. However, it was very hard to machine, with nickel inclusions that would wreak havoc with tool bits, making the machining process very slow.

Arnie Force was doing the machining for me and told me he could work a lot faster if he had the proper tool bits. Archie Dunlap had worked for many years in the auto industry, and for some reason he would not allow Arnie to get Rex AAA tool bits. When Arnie told me what he needed, I went down to the supply house and bought the tool bits. I delivered them sub rosa to Arnie, with Archie never being the wiser. We never found out why Archie wouldn't be happy to get the right tool bits for the job.

An important thing I learned was how wonderful it was to have these machinists help me with whatever I tried to build. Whatever kind of sketch I made, when I explained to them what I wanted, they invariably helped me do things in better ways because they knew materials and how to work with them.

Finishing the Ph.D.

From 1955 for the next two years I explored the difficulties of working at pressures as great as 20,000 pounds per square inch, around 1300 atmospheres, blowing up cylinders here and there until I finally had enough data to finish my Ph.D. thesis. I received my degree in June 1957. My thesis research ended abruptly when I blew up the 17-4 PH cylinder for my low-frequency absorption work as I was attempting a second run of measurements. This one blew up because of chloride ion-caused pit corrosion due to the fact that I was using a sodium chloride solution for calibrating the wall losses. Fortunately, I had enough data already to complete the thesis. Ever since, I've been a strong believer in Murphy's Law; anything that can go wrong, will!

Dr. Liebermann helped me obtain a postdoctoral appointment at Prof. F.V. (Ted) Hunt's Acoustic Research Laboratory at Harvard during 1957-58, where I continued my research, trying to measure the modal behavior of the cylinders and instituting temperature control and safety measures against flying fragments in case I blew up another cylinder.

New Projects at MPL

In 1958 Fred Spiess, director of MPL offered me a position at MPL, in which I could pursue squeezing epsom salts for half the time and, for the other half, tackle the problem of how environmental effects in the ocean would affect sonar bearing accuracy at long range. While the ocean-going work was being pursued, I had a small group continuing laboratory pressure measurements, trying to improve temperature and pressure control to improve measurement accuracy and to work at lower concentrations, more nearly like seawater. Several undergraduates helped me in this work: R. Bruce Williams, Ralph Christianson, Jr. and Don Wilson, Jr.

Later, Spiess made it possible for me to go to Australia to make measurements at high pressure of the electrical conductance of magnesium sulfate solutions. It was clear from the work of Leonard, Kurtze and Tamm, and M. Eigen that the associated ion-pairs were somehow causing the sound absorption. By doing the conductance work it would be possible to tie up the understanding of the whole process, or so I thought. Spiess very kindly let me have six weeks to work with Dr. Sefton D. Hamann, head of the division of physical chemistry of CSIRO (Commonwealth Industrial Scientific and Research Organisation) at Fisherman's Bend, Melbourne, Victoria. This was during June and July of 1961, just before the August birth of our second son.

Leading to FLIP

The ocean-going problem that Spiess gave to me was related to the coming SUBROC (SUBmarine ROCKET) missile system for submarines. The Navy's need was to understand how the ocean environment would bend or refract acoustic signals, thereby introducing bearing errors in targeting for the SUBROC missile at long range, which at that time was about 30 miles. The work commenced in October 1958 and, after reading the literature on fluctuations in inhomogeneous media, seawater in particular (including several Russian papers), I started to do experiments at 5 kilohertz at convergence-zone ranges.

The 5-kHz frequency was based on the temperature-fluctuation measurements of Leonard Liebermann which indicated a correlation distance of 30 centimeters or one foot, the wavelength of 5 kHz in the ocean. Phil Rudnick of MPL took care of hiring an engineer for me: Philbrook Cushing and, later on, Len Lancaster who became our electronic technician.

By piggy-backing on the acoustic research submarine, the AGSS 318 named *Baya*, with Frank Hale and Henry Westphal of the Navy Electronics Laboratory, who were conducting research on sound propagation at convergence-zone ranges (about 30 miles at the latitude of San Diego), we learned how to do research at sea at these ranges. Our sound source called SAUSS (self-actuated underwater sound source) put out 5-millisecond pulses at a depth of 300 feet 30 miles away, and we recorded 8 of the hydrophones mounted on the 100-foot wide wings or booms of the *Baya*, which were hydraulically folded out upon submerging.

On one of those trips Fred Spiess was in charge of his own program. Our source and its pressure switch were lashed to the deck of the submarine and the cable was spliced to a cable going through the torpedo hatch to our batteries. We were to conduct our tests after Spiess was done with his. (To appreciate my situation fully, you should realize that Dr. Spiess was the Director of MPL and I was a very green brand-new Ph.D. with no sea-going experience.) As the submarine dove for Spiess's acoustic experiment, a terribly loud pinging noise rang throughout the sub: SAUSS had started pinging once a second at a kilowatt level with five-millisecond pulses, well above a depth of 300 feet. You can imagine how Spiess felt with my equipment messing up his experiment; I was mortified. Needless to say, my experiment ended before it began, since the only way to shut off the source was to cut the battery cable. It turned out that I learned about stagger-splicing the hard way; that is, the short was caused by the increasing pressure which forced our spliced wires together through the insulation we had wrapped them in. (The other lesson I learned was to prepare for possible failures because at sea Murphy's Law is always working.)

As I said above, we recorded bearing and amplitude fluctuations at depths of 300 feet. However, when we wanted to compare the bearings that we recorded acoustically against a reference, we ran into difficulties. We were using the periscope at a depth of 60 feet to look at a flashing Xenon light suspended from a helium-filled Kytoon flown from the bow of our sound-source ship, either the *Marysville* or *Rexburg* (EPCER Navy craft based at NEL). At periscope depth at the

surface, the wave motion made it impossible to make accurate visual bearing measurements through the periscope to compare them with our very accurate acoustic measurements with the acoustic bearings as the *Baya* yawed at a depth of 60 feet.

These difficulties plus the fact that several months of preparation to go to sea for two weeks as one of a dozen groups meant that, if nothing leaked, we would have only a few hours for our experiment and might get only a few minutes of data as we passed through the convergence zone. From our results with the *Baya* at 300 feet we did obtain enough data to demonstrate the stability of the medium for doing highly accurate bearing measurements and very low-amplitude fluctuations for single direct arrivals (no surface reflections) over short periods of time.

Upon reporting to Fred Spiess the difficulties in making comparisons between optical and acoustic bearings and the very limited time available for making such measurements, he immediately mentioned the idea of Al Vine (of Woods Hole Oceanographic Institution) of upending a submarine to make a stable platform with a deep draft. As Fred commented on how Al had gotten his idea by noticing how stably a Navy swab would ride the waves upon being lost overboard, we both wound up saying, "Why not?" This was in January 1960.

Upon close examination of the ballasting structure and compartmentation of submarines, we quickly decided that we would be better off by starting from scratch to design a stable vertical platform. After considering Al's idea for a while, we concluded that all the metal on a submarine was in the wrong place, and we began a new design. The draft of 300 feet was dictated by the need to resolve acoustic multi-paths, with a sound-source depth of 300 feet at a boresight calibration range of 2000 yards, and the requirement to make measurements below the mixed layer.

With the idea that the platform would be berthed at the "B" finger of the NEL pier adjacent to MPL, a call to Al Fambrough who ran the NEL pier determined the 10-foot draft (and therefore, the 20-foot diameter) of the platform, since that was the minimum depth of the water at the proposed berthing site. However, a cylindrical hull of uniform diameter would have a resonant period of 18 seconds (the same as for a 300-foot pendulum), too close to the spectrum of wave energy in the ocean. By reducing the spring constant from 10 tons per foot for a 20-foot diameter cross-section to 4 tons per foot for a 12.5-foot diameter in the upper half of the underwater part of the platform, Phil Rudnick showed that it would have a resonant period of 27 sec-

onds, well away from energy in the ocean and, in addition, would have a null response at 22 seconds. That is, at this period, because of the exponential decay of wave motion with depth, a small pressure acting on a large area at the bottom of the platform would result in an upward force that would be equal and opposite to the force exerted by a larger pressure acting downward on a smaller horizontal area at half the depth.

Fred and I wanted to keep the platform as simple as possible, so it had no propulsion of its own, and we guessed that, if we designed it to work in 30-foot waves, that would be good enough. We thought a laboratory space that could hold 12 six-foot-high telephone relay racks would be adequate, in addition to an engine-room space and a couple of other spaces where we could eat C-rations and sleep on cots when we were in the vertical and on board. That is, there were four spaces at 8-foot intervals, starting at 15 feet above the 300-foot mark, and the 16 spaces comprising the bow section. Initially we thought we would tow the platform in the unmanned condition and transfer over prior to going to the vertical.

A particular requirement was to have an optical polarimeter to measure the twist between a reference mirror in the laboratory and the hydrophones at the bottom of the platform as it transitioned from horizontal to vertical. To do this we made provision for a clear optical path (minimum 12-inch diameter) running from the bottom of the platform to the laboratory. By means of a Wild T-2 theodolite, a laboratory mirror was referenced to the hydrophones at the bottom to an accuracy of 0.01 degree. Bore-sight calibrations of a sound source from a tug near the platform were conducted using a mirror that had been aligned with the hydrophones in the early morning hours (midnight to 4 a.m.) when the effects of waves and temperature upon the hull were minimal.

The Name

When it came to the point that Fred Spiess and I needed help to translate our requirements for a stable vertical spar buoy into naval architectural reality, he had me go to San Francisco to visit four naval architectural firms. The idea of towing a 355-foot long spar buoy out to sea to become a research platform in the vertical position with a 300-foot draft fell on deaf ears



Phil Rudnick, Fred Fisher and Fred Spiess (left to right) holding the first model of FLIP.

at three of the firms. One, Rosenblatt and Sons, knew what we were talking about because they had been involved with taking oil-drilling platforms out to sea on barges with the platform resting on its side on the barge. By partially sinking the barge at the stern, the oil platform could be made to slide off the barge and float on its side in the water. Then, by controlled flooding of the platform, they could get it into the vertical position, an operation they always referred to as the “erection” process.

Given the rather phallic shape of the spar buoy we wanted to build, I didn't really want to say “erection” when I gave talks about the buoy, and while using a 1/100 scale model to describe the operation of going from horizontal to vertical. The model had been made by Marvin Crouch of our machine shop out of a Louisville Slugger baseball bat. I wanted to use the verb “flip” to describe the operation, so one day, in the lab I had in Building 105, I used some decal letters and made the verb into a noun, pasting the letters on the side of the model. The letters barely show in the picture of Phil Rudnick, Fred Spiess and me holding the first model of FLIP.

The Launching

FLIP was built in Portland, Oregon, at the Gunderson Brothers Engineering Company, and was launched on June 22, 1962. As the construction was nearing completion, Thaddeus Stevens — the foreman in charge — began to wear a hard hat that had FLIP on the front and FLOP on the back. When asked about it, the answer was that he wasn't sure about the launching, saying that he was afraid it might roll over since the ballast was not in the keel yet. As I recall, Lloyds of London was not willing to insure it for the launching.

As it was, the only minor problem the launching presented was the classic one that amateur boat-builders encounter: it was too big to go out the opening of the shed in which it was built. That is, the 50-foot hydrophone boom on the stern was too wide for the exit where the ways went into the Willamette River. This oversight became apparent as the tanks were added to the after tank where the boom was located. The problem was solved by cutting off about five feet from the boom ends. With this temporary alteration, the launching went off in fine fashion with Sally Spiess doing the christening honors after a speech by Senator Warren G. Magnuson of the state of Washington, a legislative pioneer in promoting oceanographic research and programs.

FLIP, ORB, K-395 and Charlie Bishop

When Capt. Charles B. Bishop, who retired in 1972, came to MPL as Assistant Director in charge of FLIP, ORB, and the shop, he initiated efforts with the Naval Sea Systems Command (NAVSEA) to get support for maintaining FLIP and ORB as well as obtaining free towing services for them with Navy tugs. It should be mentioned that he had already had a great deal of experience with the Navy research community, having been the skipper of the USS *Baya*, AGSS 318, the research platform at the Navy Electronics Laboratory, and later as the ONR Program Officer for Code 466, which among other things supported MPL entirely with only a one- or two-page letter required from the Director of MPL once a year. In the early 1960s, that amounted to about two million dollars annually. Charlie also served as Project Officer for ASW systems research in the Office of Naval Materiel before he became the Commanding Officer of NUC (now NRaD), within which command MPL is a tenant activity.

Funding for FLIP came entirely out of science budgets and cost the research budgets around \$5,000 per day, in addition to the costs associated with tugs and other services required. This was causing great hardship on the research groups. Charlie, from his Navy experience, knew that NAVSEA supported the operation of the USS *Dolphin*, AGSS 555, the deep diving submarine for research berthed at NOSC.

He not only established a line of support from NAVSEA for FLIP and ORB but also got a K-395 project number so that we could get towing services from fleet tugs on a priority 3 basis. For the tugs, towing our unique platforms gave the crews added experience at sea, particularly in making and breaking tows in the open ocean and also in putting in our moorings in deep and shallow water. It should be noted that about 80 tons of deck gear is required for putting FLIP into a three-point mooring at sea; about 45 tons of that is anchors and anchor chain alone.

This was a godsend for those of us who used these platforms, and it reduced research costs to only the incremental costs associated with going to sea, around \$1,500 a day for FLIP. As one of the grateful ones at MPL, I say: thank you, Charlie, so much for all your help.

FLIP Cracks its Neck
Earl Bronson and Fred Fisher

In 1963 Walter Munk used FLIP to become an "island" to fill in the

gap between Hawaii and Alaska for making measurements of storm-generated waves from the South Pacific, thousands of miles away. FLIP was part of an array of several wave-observation stations for this study funded by the National Science Foundation. At such long ranges, what remained of the storm-generated waves were only millimeters high.

During the long tow to station, north of Hawaii, FLIP encountered heavy seas, during which a routine watch inspection by First Mate Ed Childers revealed daylight coming in at the junction between the cylindrical hull and the bow section, the “head” of FLIP. Earl Bronson, the Officer-in-Charge of FLIP, assessed the dangerous situation and decided that the only course of action was to flip to the vertical and repair the damage. With the seas running at 20 to 25 feet, this meant a cracked hull was going to be flipped in heavy seas with lots of slamming of the bow section during the very slow part of the flipping operation, around 20 minutes or so as the stern tanks flooded under only a small pressure difference with FLIP down by the stern less than 20 degrees. The question running through their minds was: what would happen to the crack during the flipping operation?

As the stern began to sink, Ed Childers was monitoring the crack while Earl Bronson worked the valves for the operation and got reports from Ed as to how much the long crack was widening. At the most, it opened up to a width of two inches at a low angle. At a certain point in the flipping operation, when enough ballast is on board to take it to the vertical, it goes very quickly from about 30 degrees to vertical in about 90 seconds.

Once FLIP was in the vertical position, the gap closed and was repaired by Ed Childers. This was done by welding the crack shut with steel from the tow ship, the Scripps vessel *Horizon*, a converted World War II Navy tug boat (ATF) under the command of Captain Noel Ferris. As it turned out after the repairs were completed, the position of FLIP was close enough to the desired station that the scientific party under Jack Northrop could successfully complete their operations.

Once FLIP returned to port, the transition from the cylindrical hull to the head of FLIP was strengthened with a conical fairing that Earl Bronson and naval architect Larry Glosten agreed on. It still serves flaw-free to this day, more than 34 years after FLIP was launched.

Anchoring FLIP in Deep Water

Earl Bronson

Although FLIP was originally designed to conduct experiments in the drifting mode, Fred Spiess wanted to extend its capability to do experiments with an array of vertical hydrophones being developed in his group by Bill Whitney and Bob Gorman. However, there was too much flow noise around the hydrophones if FLIP drifted freely so it was necessary to develop a mooring system in 3000-fathom deep water, that is in 18,000 feet of water.

The purpose of the mooring was twofold: to place FLIP into a three-point mooring to hold position over duration of the experiment as well as to minimize data contamination from flow noise. Detailed descriptions of the mooring procedure are contained in two articles [1,2] so only a cursory one will be given here. Basically, at the end of each mooring line, about ten tons of surplus anchor chain attached to a 750-pound Danforth anchor serve to hold FLIP in position. Each line is about one and a half times as long as the water is deep, consisting of 6000-foot sections, each contained on a mooring reel. In early moorings, the anchor chain was flaked out on the deck on the tow ship and cut loose when all the line had been paid out and pulled taut. The line was 1.5 inches in diameter and made of nylon, called powerbraid, with a breaking strength of 70,000 pounds. Earlier, we had determined that a one-knot current would exert a horizontal drag force of about 10,000 pounds on FLIP. The whole three-point mooring could be accomplished in a dawn-to-beyond-dusk operation in the summer if nothing went wrong. As time went on, anchor chain bins were built to make a more compact and rapid loading procedure.

Then There was SLIP

Fred Fisher

At one time MPL inherited an acoustic calibration barge from the Navy Electronics Laboratory facility at Sweetwater Lake, and we moved it to Lake El Capitan in the late 1950s or early 1960s. It was a useful platform for hydrophone calibrations, and it provided a base station for Bud Mundy and me when we were testing flipping operations on various configurations of the 35-foot-long sheet-metal model of FLIP.

Bob Rasmussen and Norm Head were among others who used the barge and, because of one of its many deficiencies, they christened it SLIP (for Slightly Leaky Instrument Platform), in honor of its buoyancy characteristics. My thanks to Chris Baldwin who reminded me of MPL's sister platform to FLIP.

My Worst Sea Trip

George G. Shor, Jr.

While working for MPL from 1953 I averaged two months per year at sea on Scripps ships for 25 years, chiefly on seismic projects. Most of the trips were pleasant, sometimes exciting, and always productive. The worst, without any competition, was Leg 1 of Monsoon Expedition in 1960. It was the maiden voyage of R/V *Argo*, and I once described it as the “longest shakedown trip ever.”

Argo (the former USS *Snatch*, ARS-27) was provided to Scripps by the Navy and was given an overhaul and conversion after having sat in the mothball fleet for 14 years. Many curious things happened before the ship sailed, including the discovery (by the Secret Service) that two new crew members were counterfeiters, who had stored their bogus bills aboard under a bunk. Fortunately, they tried out their bum \$20 bills in the worst possible place — a Tijuana bar — and were caught promptly. There were other delays, however, and there was time only for a few days of shakedown.

We left San Diego 26 August 1960, with Captain Laurence Davis in charge, a scientific party that was “one of each discipline,” and a very raw crew, headed from San Diego to Cairns, Australia, with a very abbreviated scientific program, and then to start on the International Indian Ocean Expedition.

The ship had four engines, which were supposed to give us 13.5 knots. The Chief Engineer had a different idea: he thought that the ship had two engines and two spare engines, so it was a running battle. He won. Fuel lines cracked and broke several times a day, and there were very few hours of four-engine operation. I could see all the scientific time going down the drain.

One horrible morning, at 3:00 a.m., when we were about equidistant from San Diego and Honolulu, the ship coasted to a stop. For those who have never been to sea: you get used to the noise of the engines, and when they stop you wake up wondering what happened. I came on deck to be told that the steering cables had parted. The design was odd: the steering engine was way forward, and was connected with

the rudder by two cables, which went aft on the two sides of the ship hidden behind panelling. One cable had come unravelled. There were no spares aboard, although somebody remarked that he had wondered about all those pieces of pre-cut wire rope that had been in the hold and had been unloaded before departure. After some consultation, it was agreed that a jury-rig could be done by using parallelled lengths of hydrographic wire, although it probably wouldn't last long. Some of the ship's engineers set to work to pull out the broken cable and lead the replacement through behind bulkheads and over sheaves.

Hoping to recover a little bit of the scientific program, I called out the member of the scientific party who wanted to make camera lowerings to look at manganese nodules at "random locations" across the Pacific. This was clearly a random location. He hooked his heavy camera/strobe rig on the dredge wire, which was lowered to near the bottom to take pictures as we drifted. It went down, he allowed time for pictures, and it was brought up to only a few hundred meters below the surface when suddenly the traction unit of the dredge winch made some horrible noises and stopped. One of the drive gears had shed a few teeth. That was at 5:00 a.m. The next command decision was: bring the wire and camera aboard using the crane with "come-alongs." It was a slow way to bring in wire, but it got the camera back.

At this point, we were still waiting on steering repairs, with clearly lots of time to go, so I asked technician Norm Anderson if he wanted to take a bottle cast. He did, and we tried that from the starboard hydro winch. Everything went well on the way down, but on the way back up the bearings on the hydro winch overheated and started to smoke. That was at 6:00 a.m. Rather than go through the same procedure as on the dredge winch, we rigged a fire hose to cool the bearings and kept coming in. The cast came up, but as soon as we stopped the winch, it froze up. It turned out that the winch drum had stretched under the tension of the full wire, and the bearings were rubbing on the frame. There would be no more hydro work until we got new stronger drums.

At some time during all of this — say, 8:00 a.m. — the cook came on deck to report to Captain Davis that he had been defrosting the main freezer and the compressor had suddenly "gone bang" and cracked its head. At 9:00 a.m., an oiler ran up on deck to report that we were leaking water into an empty fuel tank at a high rate. We moved the cores from the deck-mounted core reefer van into the chill box, set the temperature of the van to freezing, and mobilized the scientific party to move meat. (The freezer was filled with many tons of meat, because the port steward was convinced that there was no good beef in Austra-

lia!) The first engineer checked on the fuel-tank leak and verified that it was just a valve problem, so no emergency measures were needed.

The steering was temporarily fixed, with the warning that it wouldn't hold up for long, and that we had better steer by adjusting the speed of the two screws. We changed course to head to Honolulu and reported back (roughly paraphrased; I don't have the message): "Steering gear broken. Main winch gears stripped. Hydro winches inoperative. Main freezer out. Leak in one fuel tank. Heading for Honolulu, steering with the engines. We can still take BTs. Shor and Davis." We spoke too soon: the BT winch quit the next time we tried to use it.

Arrival in Honolulu was a bit hectic. We used the steering gear, but had a tug standing by in case it was needed, and we tied up downtown under the Aloha Tower. Waiting for us was the chief engineer of Western Gear (which had built the dredge winch) with the good news that he had worried quite a bit about the Navy's detailed specs on the winch gears, had calculated that those gears would last only about 100 hours, and had persuaded the company to risk its own money on building a new larger gear box, which was to arrive very shortly by air freight. The catch was that the larger gear box would only fit if we cut down the mount for the old one, which meant some cutting and welding — only a few feet from the ship's magazine, which contained about 20 tons of explosives. Larry Davis and I retreated to a safe distance to discuss the problem: he had not told the port authorities that we had explosives aboard, and we were in the middle of downtown Honolulu! We agreed that one might as well be hung for a sheep as a lamb, that there was no point in upsetting people, and that he and I would split a "fire watch" on the magazine.

The rest of the time in Honolulu was relatively uneventful. Max Silverman figured out that we could cut down the base of the gear box by using a portable milling machine instead of a torch. He persuaded Pearl Harbor Naval Shipyard (owner of the only such machine in



George Shor being hosed down by Helen Kirk.

Hawaii) that we were on a secret mission that he could not disclose, so that they would give us priority on the use of it, and he hired shipyard workers to moonlight the night shift on the repairs. The steering cables were replaced and numerous spares provided; the freezer was repaired; the valve was fixed; new heavier drums were built for the hydro winches; we got all new fuel lines for the main engines; we fixed the BT winch; and we did about 30 other things.

We were ready to sail by about 4:00 p.m. on a Friday afternoon. The only catch was that we were supposed to go to West Loch of Pearl Harbor to pick up SOFAR (pressure) detonators for the "shot heard round the world" that was to be fired off Western Australia. The explosives depot would not send these little detonators through city traffic to the Aloha Tower, and we could not get to West Loch before they closed for the weekend. So we sailed without the official detonators, relying instead on the home-made explosive devices that Gordon Hamilton had sent to me before we sailed (which he had put into a briefcase on a plane). We went on to Cairns, Australia by way of Howland Island and the West Pole.

In Cairns we completed charter arrangements for the *Malita* as the second ship for a two-ship seismic-refraction program. We had essentially a pleasure trip around to Darwin, where I left the ship, and Russ Raitt and Bob Fisher went on to do some significant work in the Indian Ocean.

Sometimes people wonder how I seem to remain calm and start planning "what do we do now?" when everything goes to pieces. The answer is that nothing since has ever quite equalled Leg 1 of Monsoon Expedition on R/V *Argo*.

Christmas 1960

22 December 1960

To: MPL Staff Members

Through the year we are all so busy working on the laboratory's varied projects that we do not often stop to realize the value and significance of the performance of all the individuals who make up our group. It is clear from the growing support provided to us and the respect with which our staff members are met wherever they go that our work is greatly admired and appreciated. Certainly no one is in a better position than I to see that this is the result of the combined efforts of all those in MPL. We have been over the years a group in which individual performance which in other places would be commended as far above average is more or less expected of everyone. Although it is expected and given it is also still worth considerable thanks.

I hope you will realize my own strong feelings of gratitude for the many things which you have done during the year and that you will have a pleasant holiday season.

Merry Christmas and a Happy New Year.

Sincerely,

F. N. Spiess

MPL Stories

Phil Rapp

Getting Stuck!

My first visit to FLIP was in 1964 after I first started working for MPL. I was asked to shoot some film of FLIP off the coast of San Diego. I went out to sea on the ship that towed FLIP and shot footage of the flipping. I was to transfer to FLIP to shoot more footage aboard. I got into the Boston whaler that was crewed by Rich Silva (who would become FLIP's Officer in Charge later). He offered me a very large, type 1 life jacket, and I had to double it around me, which left a big lump in the front. I was told to jump onto FLIP's ladder when the waves lifted our boat. I jumped with gusto onto FLIP's ladder — and got stuck with the waves washing over me as they rose. Luckily, Rich kept the boat's propeller away from me. I removed the jacket's ties and climbed aboard. Earl Bronson (First Officer in Charge) told me to take a shower in my clothes to get rid of the salt. I shot my pictures in wet clothes for a good part of the day.

An Important Recipe

The following recipe was concocted in 1963 by Tony Sousa, who was the first cook on FLIP. His Portuguese background served him well in coming up with strange recipes to try out on the crew and scientific staff. FLIP is very unstable in the horizontal position, and seasickness often results. When FLIP goes vertical, it is time for Tony to make his magic and cure the seasickness. I ate these on several occasions and survived quite nicely, thank you. The recipe was even published in Deep Dishes: Favorite Seafood Delights, to which I submitted it.

Galvanized Pork Chops

Take 2 dozen pork chops. In a baking pan add a bottle of wine vinegar. Add the juice of 6 freshly squeezed lemons. Peel and chop the cloves of 6 heads of garlic, sprinkle in. Salt and pepper the pork chops to taste and add to the concoction. Let chops marinate overnight, or 2

days if you are brave. Add a small amount of salad oil after marinating. Bake in the oven at 300 degrees until brown, turn over and crisp the other side.

Rob Pinkel's Hawaiian Accident!

I was sent to Honolulu to photograph sea trials after FLIP was damaged by a huge storm wave off Hawaii. They had set up a perimeter along a pier. Longshoremen in their forklifts were dropping pallets of materials right in the area that we were given. Rob complained to them, but this didn't set too well. Before we knew it, they were loading giant pallets of wet animal skins right along our perimeter. The smell was terrible, almost unbearable. While they were unloading, a forklift accidentally bumped a shed that we were storing our equipment in, and a heavy box fell on Rob's ankle as he ran from the accident. He was taken to the hospital for repair of his foot, and that was the last we saw of the union workers.

Hawaiian Tugboat!

I took my quarters on the Hawaiian ship that was to tow FLIP. I was given a berth in the forward hold. It was so rough that I had to tie myself into my bunk. A crew member had opened the hatch for some air, and when we took a large wave over the bow, we all got soaked. That was the end of sleeping for that night, as I headed for higher "ground" in the galley. Speaking of the galley, I almost couldn't eat while watching those huge Hawaiian crew members eating lard with every meal.

Falling out of Bed!

While I was setting up to film FLIP in Hawaii, Earl Bronson asked Jack Grace, who was doing welding on equipment, to go with me on the Hawaiian tug. Jack and I shot all of our film on FLIP and it was time to transfer to FLIP to shoot some interiors. We worked all day and, when the sun set, Jack thought we could rest. But, no, it was time to clean lenses and ready equipment for the next day. Jack and I got to bed about 1:00 in the morning. He got into his upper bunk and I retired in the lower bunk. We didn't understand or know the procedure of locking the bunks. About an hour after I had fallen asleep I was wakened with a loud bang, and there was Jack on the deck. He had rolled over and out as the bed swung over. He wasn't hurt, and we

started laughing. At this point I fell out of my bunk onto Jack's chest, and we laughed uncontrollably. Then the other crew members threw us out of the compartment. We regained our senses by drinking coffee in the galley.

John Brown on the Roof of Building 33!

One year all ships of the Scripps fleet were in port for the Christmas holiday, and Chris Baldwin asked John Brown and me to go onto the roof of Building 33 (now the main waterfront building of NR&D) to take photos. John was having eyesight problems, and I did a lot of his work, such as focusing the enlarger. We went up on the roof, and I was busy assembling the camera equipment while John was setting up the tripod. I looked up — and John had set the tripod about six feet from the edge of the roof, but his footprints went around the front of the tripod about six inches from the edge of the roof. I panicked and called to him, "Lie down and crawl toward me." We were both shaking from the event, but I don't think that John realized how close he had come to disaster.

Memories

Jim Helle

In February 1964 I was preparing for a dive in Jacques Cousteau's small submersible *Soucoupe*. My mission was to measure and record the thermo-microstructure in the ocean off San Diego at a depth of 500 feet. As part of the dive, I wanted to know our speed through the water, so I designed and built a small speedometer using a savonius rotor. As I was busy installing it on top of the submersible, Captain Cousteau approached and, apparently upset that I was modifying his submersible, asked in a gruff voice, "What are you doing?" I told him, and he said that he could tell me how fast his submersible went: four knots. I thought, "Great, now I won't have to fool around any more with the speedometer." So I asked him, "Is that 4.00 knots?" He replied, "I think you had better continue installing your speedometer."

In the summer of 1965, CBS wanted to film a dive of my wooden submersible *Submonaut* for the evening news. They wanted something unusual, so we decided to try to set a world record for the most people in a two-man submersible. They arrived on schedule, and we decided to use five crewmen, which, looking back, was a foolish idea. All five of us piled in and down we went into Mission Bay to a depth of eight feet. The commentator of CBS talked to us through our underwater telephone and instructed us to move the sub about to add some action. The sub's control box, which allowed for forward, reverse, and up-and-down motion, was only two by four inches in size and was located at the end of a six-foot mini-cable. What with the box being very small and all of us being packed into the sub, we could not locate the box. We had to explain to CBS that there would be a slight delay. One crewman started at the wall junction box and followed the cable for a few feet, and then the next crewman followed the cable for a foot or so, until the box was located. One of the crew was sitting on it. We all had to shift around a few inches until the box could be retrieved. We then did a few movements until CBS was satisfied. After surfacing, we were all very glad to be alive.

Jumping off FLIP

Fred Fisher

In the afternoon of December 1, 1969, after more than 36 hours of rising sea swells, FLIP's crew experienced a terrifying time, when one 80-foot swell from a distant storm came to within two feet of the top outside platform where they were standing; that is, within 17 feet of the top of FLIP's hull in the vertical position. The situation was aggravated by the loss of electric power on board. These swells had periods of 16 to 20 seconds and had been building up over several hours while FLIP was drifting near the Hawaiian Islands, at 27°30'N and 157°45'W. That is, 95% of the hull was under water during the highest waves. Fortunately, there was only minor leaking, which shorted out the cooling pumps for the engines. The big chill box for food, adjacent to the piping for the flipping controls on the lowest platform, was swept overboard without causing any damage to the piping. There was some comfort for the crew in the fact that the ship they were operating with was standing by. During the next day they were rescued after jumping off FLIP when the blue-water swells were down to about 40 or 50 feet between crest and trough. Ray Hasse of the U.S. Navy Underwater Sound Laboratory in New London made 16-mm movies of the waves during this time.

According to Dewitt Eford, from whom I heard the story, the crew was instructed to step off when the swells were at their peak. They all did so, with the exception of the cook, Ben Parker. He hesitated just long enough so he fell the full distance to the trough of the wave and went under with a big splash, but made it back safely to the surface. John Russell of the scientific party made an unusual exit, by stepping off at the top of the wave. He went under water, leaving only his flip-flop sandals floating on the water, with the thongs up. He was picked up safely. No one was injured and FLIP suffered only minor damage. Earl Bronson supervised the difficult operation of flipping FLIP back to the horizontal for returning to port for repairs. The incident was reported by Phil Rudnick and Ray Hasse in the paper, "Extreme Pacific Waves, December 1, 1969" in the *Journal of Geophysical Research* (vol. 76, pp. 742-744, 1971). A two-minute video tape made from the 16-mm film shows part of the rescue operation.

FLIP in the 1970s

Rob Pinkel

In the spring of 1969, Carl Eckart posted a notice to all first-year graduate students, inviting them to join MPL, to continue and extend Bob Zalkan's studies of upper-ocean internal waves. There was concern at that time that internal waves would significantly refract propagating sound. (It would be another twenty years before it was appreciated that internal waves constituted an ultimate limit to acoustic predictability.) I signed up and was quickly introduced to MPL, FLIP, and the host of folks that populated the MPL shops and waterfront.

At that time FLIP was berthed at the B Street pier, downtown. Earl Bronson presided over the B Street operation with a salty, genial hand that was easy to appreciate. Rich Silva was the recently appointed officer in charge of FLIP, assisted by Walter ("Freddie") Fredricks (engineer), Rick Wilson (boatswain), and Ben Parker (cook). While support for FLIP was fairly plentiful, the demands on it were enormous. In addition to a busy schedule, techniques for deep-sea mooring, acoustic-array handling, instrument-boom deployment, etc. were being developed, with little technological precedent.

The waterfront pace was intense. Logistics were complicated by the fact that shop support was five miles away, on Pt. Loma. Earl permitted just enough mischief to keep the operation sane. For example, MPL diver Jack Donovan made his practice dives at B Street pier only when there were no Navy ships moored out in the bay. It developed that Jack was tending unmarked lobster traps. These were planted, for easy location, at the base of the Navy mooring.

It's well known that FLIP encountered 80-foot waves in an operation north of Hawaii in December 1969. It is less well known that, three months later, the platform was refurbished, modified to withstand "washovers" and towed back out to (nearly) the same site. The rematch trip, termed Parka IIB, was a long-range acoustic-transmission experiment. Bill Whitney led the FLIP scientific team. His goal was to deploy a precisely calibrated receiving array with hydrophones which, for the first time, went quite deep (about 4 km). There was a sense that sound was propagating in the sea much farther than it was

supposed to, given existing models for acoustic attenuation. The thought was that, at high pressure, sound didn't attenuate as much as near the surface. Noise-free acoustic measurements were required at great depths to verify the issue.

Bill's vertical array, however, was putting up a heck of a struggle. Noise was appearing in the data to a far greater extent than in the lab. As the noise problems were beaten, the hydro cable itself began to fail. The shorts had to be tracked down one at a time, and the bad sections of wire excised and re-spliced. Bill and his team battled to exhaustion and beyond, while a thousand miles away, source ships waited impatiently. Toward the end of the operation the good guys started to win. A deep hydrophone was successfully deployed and was working with little noise. Bill and his team watched the performance of this hydrophone with great relief. After nearly three weeks of struggle the experiment would start paying off. The joy faded, however, after the first hour, when noise levels began to rise systematically. I witnessed the growing dejection in FLIP's lab, sharing in the disappointment. A shout from the crew penetrated the gloom. When we looked up, our ocean-view lab porthole was solid green. A huge freighter was passing close on the port side, coming to see —as usual,— if we were “really sinking.” When the freighter passed, noise levels dropped and data collection began in earnest.

Navigation in the 1970s was an uncertain proposition. FLIP had a permanently dysfunctional OMEGA system and a primitive LORAN A which also had quite a personality. The LORAN consisted of a mysterious box with lots of knobs and dials. Rich would spin the knobs, curse the dials, and consult with a book of time delays (or was it astrology?) in order to get a fix. He was aided in this effort by the fact that FLIP rarely drifted more than 10-15 miles per day. Over the course of time we could establish the platform's position pretty well.

The tugs that towed FLIP were typically in much worse shape in regard to navigation. They would often attempt to rendezvous by dead-reckoning their position from San Diego, using radar for the final homing. In rough weather, neither FLIP nor the tug were easy to detect on radar, given the problem with sea-clutter. Many a trip ended with the classic radio exchange:

Tug: “I've arrived at position X as requested, where are you?”

FLIP: “We're at position X as specified, where are you?”

On one trip, a full day was spent while the tug searched for FLIP. In rough conditions, the radars were just not much help. Finally, at nightfall Rich directed FLIP's searchlight straight up. It illuminated the clouds overhead. The tug homed in on this beacon.

Returning to the California coast could also be an adventure. Again, the accepted tugboat practice was to aim in the general direction of San Diego, and use the radar to find out where they actually ended up when they closed in on the coast. Moving at 8 knots, a one-kt current could significantly affect our landfall. On one memorable trip, the tug blew an engine 200 miles southwest of San Diego. In true tugboat fashion the skipper pointed the bow at San Diego and we proceeded to head home, now at 3 knots. Three days later we closed the coast — off Ensenada! A one-knot current really matters when you only go 3 knots. We spent another full day limping north, singing the praises of the tug skipper. Ah, the good old days.

Tony Parra and the Sisselmann of Svalbard

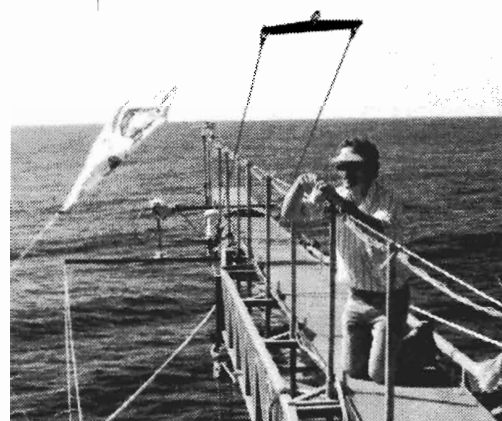
By the 1980s a new team was on FLIP with Dewitt Efirid in charge, Tom Golfinos as engineer, and Tony Parra in the galley. Tony was an ex-chief in the diesel submarine force. He was all enthusiasm, and had little patience for others who were not gung-ho. He was also an excellent chef.

Our group, not with FLIP, had started doing research in the Arctic. The logistics involved flying out onto the Arctic ice, establishing a camp (a collection of 5 to 15 tents, an electrical generator, etc.), and deploying equipment through holes in the ice to view the ocean below.

At the start, Arctic operations are labor-intensive. One needs to eat 3000 calories a day to keep going, and 5000 calories doesn't hurt. On our first Arctic experiment, these calories were provided mostly in the form of greasy toasted-cheese sandwiches. We thought much about Tony's cooking back on FLIP, as we coped with the hardships of the Arctic menu.

Our second trip was to be staged in the eastern Arctic, north of the islands of Svalbard (Spitzbergen). We prevailed on the folks managing the logistics to let Tony do the cooking. This worked out pretty well, as there was free time in the FLIP schedule. Also, the previous Arctic cook was committed to working a nearby experiment at the same time.

We flew to Norway and then to Svalbard to prepare our experiments. The two cooks flew to Greenland to obtain food for the ice camps from the U.S. military bases there. The experienced Arctic cook advised Tony exactly how many toasted-cheese sandwiches 35 men



Rob Pinkel in a lighter moment on FLIP.

will eat in 40 days on the ice. Tony, fortunately, had no trouble disregarding this advice. Using his Navy chief's instinct, he did a fine job raiding the commissaries of Greenland. He arrived in Svalbard with pallet loads of lamb, shrimp, lobster, etc.

The Norwegian settlement on Svalbard is a small community, presided over by a Governor-General, known in Norwegian as the Sisselmann. A major function of the government on Svalbard is to prevent the smuggling of goods into and out of Norway. As such, the Sisselmann himself was often at the airport, inspecting arriving goods and passengers. Norwegians are experts at living well in a harsh environment. The quality of Tony's food shipment was apparently noted.

Shortly after Tony's arrival the weather closed in, and the island was cut off from civilization for a few days. Supplies of fresh milk ran low. The Sisselmann, recalling the recent food shipments for the research camps, asked the experiment leader if he could borrow some milk for the school children of the island. Tony was, of course, happy to oblige and he delivered the milk personally. The Sisselmann, appreciative of the cooperation, ended their meeting with the words, "If there is ever anything I can do for you, just let me know." These words are not to be uttered lightly to a guy like Tony.

The weather subsequently cleared, the ice-camps were deployed, and research began in earnest. Cooking for 35 people in the Arctic conditions proved to be an enormous amount of work. Tony was on the go for 15-18 hours a day. The worst part of the ordeal was that, toward the end, supplies of beer ran low and cigarettes were totally depleted. (In a misguided effort to "reform" Tony, his friends refused to send him more cigarettes from back on Svalbard.)

Finally, after 40 days and 40 nights of extreme effort, the camp was packed up. Tony and his colleagues spent much of the two-hour flight to Svalbard relishing their impending visit to the island's only pub.

Alas, on arrival they were reminded that it was a Sunday. The pub, and indeed all stores (both of them) were closed. The group was initially discouraged, but then Tony recalled his new friend, the Sisselmann. So, early on this Sunday morning Tony called the Sisselmann at his home, "Hi, I'm Tony, remember me?" Ten minutes later cases of beer and cigarettes were being loaded from the Sisselmann's basement into the experiment's rent-a-truck.

The Sisselmann was true to his word. Both he and Tony were quietly toasted in a subdued but extended celebration that Sunday in Svalbard.

A Bit of Nutty Science at MPL

Victor Vacquier

Men working in science sometimes get ideas that make strange things happen, as when in 1969 I got the idea that one could take snapshots from night to night of the slipping of the Pacific plate past the North American plate by photographing two lights with a long-focus camera. The camera and one light would be on one plate and the second light on the other. On the map it seemed that the best place for the experiment was the channel a few miles wide between the mountainous island Angel de la Guarda and mainland Baja California, about two-thirds down the Gulf of California. It seemed a good location as there was a resort motel some fifteen miles south at Bahía de Los Angeles, where turtle steaks were served for dinner nearly every day. Dick Phillips, a former student of Russ Raitt, took John Mudie and me for an inspection flight. By flying down the channel, we spotted a likely location for the camera and the lights.

The next step was to get a permit from the Mexican government to do the work. Nicholas Grijalva, who was teaching oceanography at the school in Ensenada, flew with me to Mexico City, where his brother, a lawyer, took us to the appropriate ministry where we sat for an hour in the waiting room, along with a dozen others, before being admitted to the presence of the official person. He gave us the permit. Grijalva, not having been a tourist, had never seen the pyramids and the excavations near Mexico City, so I took him there, and he was impressed by the size and beauty of these monuments. In the meantime, Chuck Corry, my super-technician, designed the camera and the lights to be built by the MPL machine shop.

Finn Outler set up the logistics of the project by providing us with a landing craft into which our four-wheel-drive truck just barely could fit. The truck was used to haul supplies from the beach landing to the base camp at the foot of the rocky path to the site of the installation close to the top of the mountain.

The construction of the piers and shelters for the camera and lights was handled by an American whose name I forget, who was a resident of

Ensenada and who was very skillful in the building of the CICESE building in Ensenada. To haul up the material, such as 2X4's, concrete, water for it, etc., Grijalva got three Mexican marines. The fellow from Ensenada cooked not only for us but also for the marines, which shocked Grijalva, who maintained that the Mexican military should be feeding itself. They were good lads who competed with each other as to who could run up the mountain the fastest. One of the MPL annual reports has a picture of the camera installation. It consisted of two shacks connected by a stovepipe 30 feet long. One shack housed an 8-inch concave mirror that formed images of the lights on a photographic plate in the other shack. The film was moved stepwise by a timer for a few exposures during the night. A clock turned the system on in the evening and shut it off before dawn. The lights were a little larger than flashlight bulbs and were fed from a battery charged by a small solar panel during the day. The light was turned off by a photocell during the day. A. E. Whitford, the director of Lick Observatory on Mt. Hamilton, sent me some fine-grain photographic plates for our camera.

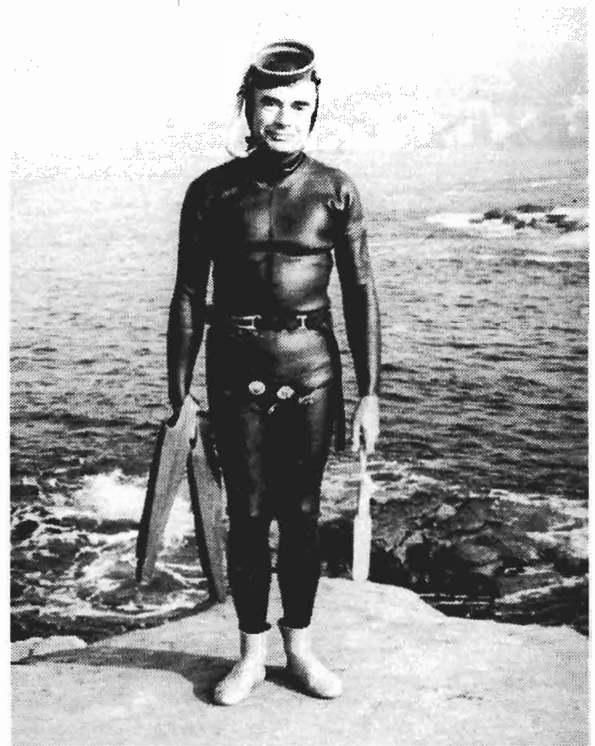
After the marines and Grijalva left, our provisions were running low, so Corry sailed for provisions to the motel in Bahía de Los Angeles in *Charlie Bear*, the landing craft. A couple of days went by, while we got hungry enough to try the large iguanas. The island is famous for its huge rattlesnakes and iguanas. The latter are slow-moving and easily killed by well thrown stones. They taste like stewing chicken and have to be boiled to make them edible. Some of the seaweed growing in the shallows we found delicious. Finally *Charlie Bear* arrived, and we ate normally again.

One time Corry was alone on the island. He anchored the craft far enough offshore so as to be sure that it would float at low tide. Next morning when he came to the landing, he found *Charlie Bear* sunk in some six feet of water, because the anchor line was too short at high tide. Imagining being marooned on the island without water to drink and with only iguanas to eat, he rushed up the mountain and shouted "MAY DAY! MAY DAY!" into his walkie-talkie. Late that evening a radio ham in Iowa heard him and got the Scripps radio station WWD alerted. Corry was rescued, but it took Finn Outler, Jim Rogers, and R. J. Smith a long time to get the landing craft serviceable again and to bring back the truck marooned on the island. It was so long, in fact, that Corry left to get an education, eventually a Ph.D. During that salvage operation, late one Friday afternoon I got a plaintive telephone call from R. J. Smith, asking me to rescue him from the Mexican immigration office in Tijuana. He had been arrested in Bahía de Los Angeles for working without a permit and his bail was \$800. I rushed

to my bank a few minutes before closing time and raised the needed cash. I then drove to Tijuana. I found R. J. in a small room in the bridge structure over the entrance to the border.

Dick Whiteman replaced Corry, and finally he and I took some pictures of the lights. By estimating many times the distance between the images of the lights, I found that one could detect a displacement of three millimeters between the two lights. This was the only result and not very useful, demonstrating that money is spent more wisely when it is scarce. I gave a ten-minute paper on the three millimeters at the 1972 annual meeting of the AGU in Washington. Someone I had not seen since wartime days said it was a good paper. It was published in a scientific journal.

Shortly after the money ran out, a 6.3-magnitude earthquake occurred in the channel at our site, which must have upset our camera installation. We shall never know as we have not returned to find out.



Vic Vacquier at La Jolla Cove, 1958.

How Things Looked in 1976

Fred Noel Spiess

(In 1976 MPL held its first birthday celebration. At the time, I had been in the Lab for 24 years and Director for 18. The following is my copy of the introductory talk that I gave that day, November 11, 1976.)

It is a pleasure to welcome all of you to our ceremonies this afternoon. We appreciate your coming to share our pride in completing 30 years of achievement.

I have been here at MPL for nearly 25 years, and this is the first time we have held a birthday party. I think the reason is that we are so busy looking toward the future that we have simply not taken the time to gather together as a group to look backward. This is a laboratory with a strong feeling of continuity. What we are doing today, and what we plan to do tomorrow, flow quite naturally from what we have done in the past. This has been borne home to me quite strongly during the last three weeks or so, during which I have had the opportunity of going over our program with four review groups. Looking ahead in each instance, it was clear that the various innovations we have produced each became part of our heritage and a foundation stone on which to build toward further advances.

A look at some of these innovations reveals the kind of people we are: Dedicated to learning about the sea and using it effectively, but quite pragmatic about getting out and doing what we can now rather than waiting for the appearance of some magic fully optimum system to do the job in the future. We have been primarily experimentalists with an incurable optimism. The primary concept seems to be that we can take a good idea which might seem marginal in terms of feasibility, and make it pay off simply by going ahead and carrying it out.

One of the oldest examples is the technique devised by Russ Raitt long ago for suspending hydrophones in the sea with minimum noise generation. He knew that he wanted to make listening elements float in the surrounding water. He devised a configuration to do this and went to sea using and modifying it until the bugs were out. The result is still, after 25 years, the quietest system in existence.

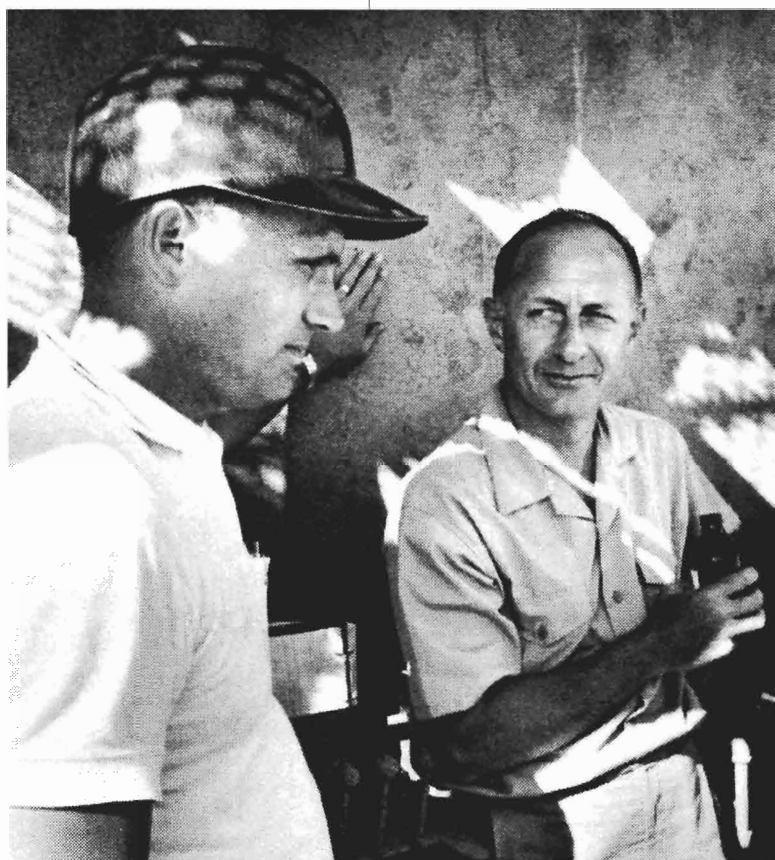
In another example: Vic Anderson and I, many years ago, became involved with the use of hydrophone arrays. It was clear in those days that one could build a delay line and insert hydrophone signals in it in such a way as to concentrate the array's receiving capability in a particular direction. It quickly became clear to us, however, that such systems would be more useful if they could look in more than one direction at a time. We thus began building and using a variety of approaches, with theoretical background from Phil Rudnick. One of these which Vic quickly brought to a high level of development was a digital technique, which we called DIMUS, which is in use in many systems today.

The push that DIMUS gave us into what was then the new world of digital processing paid an additional, almost immediate, dividend by being the springboard for Vic's invention of the now widely known DELTIC correlator, a lovely device for testing how well two signals resemble one another. This device, then, was born of the need to solve some specific problems, but, not accidentally, turned out to be of much broader and quickly recognized usefulness.

Now that I have started on the chain of description of development of research techniques, let me continue with only three more examples. One is my own.

We needed to know the slope of the seafloor on a fine scale in order to assess the performance of sonar systems using bottom bounce paths. I decided that the best thing to do was to tow a precision echosounder close to the deep seafloor, while tracking it with an acoustic navigation system. This indeed did the job, but by allowing for the possibility of using the vehicle to carry other sensors, we laid the foundations for our present Deep Tow system, now the most advanced system in existence for observing the fine-scale properties of the seafloor, and the producer, just last week, of its 12th Ph.D. thesis.

The other major step forward came when we had to consider, in one of Fred Fisher's programs, how to make careful measurements of the



Fred Fisher (left) and Fred Spiess at the launching of FLIP, 1962.

fluctuations of direction of arrival of sound transmitted horizontally through the open sea. The result was the development and construction of FLIP — here again making a solution to a specific problem in such a way as to provide a generally useful tool for a wide variety of future applications.

Just to be sure that all my examples don't have their origins too far in the past, let me cite one final item which will be going out this afternoon for its first major trial on FLIP. We have been interested for many years in studying internal waves in the sea. Others at Scripps have worked on this problem, but FLIP, along with techniques developed by [Eugene] LaFond for use nearshore from the NEL tower, gave us an opportunity to look at the directional properties of this phenomenon in the open ocean. Even with these tools, however, we were frustrated in that the horizontal region over which we could make measurements was too small, and the technique would not work at all in the well mixed upper layers of the ocean. After trying to implement a variety of other approaches, we realized that we could transmit a sonar beam out horizontally from FLIP and analyze the backscattered sound to tell us how the water was moving. Rob Pinkel has been developing a system to do just that, and will be taking it out, leaving today, to give it a first major test. The beauty of the system is that, while its origin came from the internal-wave problem, it will also give us insight into a variety of other aspects of water motion in the upper parts of the sea.

I could go on to Vic Vacquier's magnetometers; to Kaye's scattering study array now mounted on the after end of FLIP; Vic Anderson's RUM, and his latest major project, still under construction — ADA: the Acoustic Development Array, whose outer form you will see alongside the pier when you go to look at our floating facilities later this afternoon.

Looking back on these examples we are naturally pleased with them as innovations, and with the research results which they have produced. I am also proud of their durability: they have been adaptable to new problems as we go along. The key to this is that while we are enmeshed in the problems of today, we keep our eyes on those of tomorrow. Thus when we move to bring some new technique into being, we do it with the idea that it should have potential for answering future questions which at the time may only be vaguely formed. I think this generalist's approach is inherited from our founding director, Carl Eckart, while our pragmatism comes from his successor, Sir Charles Wright.

With this recitation of successes, I don't mean to give the impression

that we have not had occasional disappointments, or even a rare disaster. This is where the essential optimism of those who go to sea to do research must win out. We try to learn from our misfortunes, but not so much as to inhibit our trying again.

In any event, we manage to maintain a pace which you can see for yourselves down at the Marine Facilities pier. FLIP is loaded to go at 4 p.m. The ADA barge has just successfully and deliberately been rolled over yesterday, the R/V *Melville* is there, from which we offloaded our Deep Tow gear just a few weeks ago, after a three-month expedition. Invisible is the *Thomas Washington* in Guam after a long operation under George Shor, and in back of me is our new ONR-funded laboratory building in which you can see a variety of things under construction for the future.

We are a small laboratory — just over a hundred people — and part of the reason we can accomplish what we do is that we have so much good help from a fantastic number of people in our two parent agencies — the University of California and the U. S. Navy — and from our own individual families.

The real motivation for this birthday party then is to bring our supporters and the users of our output here, so that we can give you a little of the flavor of what it is like here on our own ground, and so that we can express our thanks for 30 years of productive interaction in which many of you have played a significant role.

(Vic Anderson then introduced retired members of the Lab. Bill Nierenberg made some comments and introduced RADM Robert Geiger, Chief of Naval Research. Service Awards were made (including 30-year pins to Chris Baldwin and Stan Lai). The event continued with an open house in MPL's new laboratory building, and on board FLIP and R/V *Melville*.)

Address at the 30th Anniversary Observation of the Marine Physical Laboratory

November 11, 1976

Rear Admiral Robert K. Geiger, USN

Dr. Spiess, distinguished guests, ladies and gentlemen:

It is indeed a pleasure to participate in this ceremony observing the 30th anniversary of the Marine Physical Laboratory. For me, as Chief of the Office of Naval Research, this ceremony is of special significance, not only because it recognizes 30 years of productive and imaginative work of MPL, but also because of the interesting parallels that exist between my office and this organization. ONR also marks its 30th anniversary this year. It was formed in 1946 out of the Office of Research and Inventions (ORI).

The Marine Physical Laboratory is the lineal descendant of the University of California's Division of War Research (UCDWR). During World War II, the Division of War Research carried out an extensive program of basic measurements on underwater sound propagation. The work was instrumental and of immediate value in providing information on how to use, most effectively, the gear that the Navy had then. At the same time, the program led to scientific conclusions of long-range value concerning the basic properties of underwater acoustics.

At the end of the war, the Navy was convinced that the continuation of this fundamental research was an indispensable part of a farsighted naval defense program. The impenetrability of the sea to electromagnetic radiation and the relative difficulty to locate underwater objects by sound indicated that the submarine would occupy a prominent position in naval warfare. In this respect it was important to continue research in the fundamental properties of underwater acoustics, which was to remain the principal detection means of pro- and antisubmarine warfare.

The parent of ONR, the Office of Research and Inventions, provided the funds for the continuation of this important work. In 1946, MPL

was formed from the Division of War Research and sponsorship passed from the Office of Research and Inventions to the Bureau of Ships. In 1958 sponsorship of MPL passed to the Office of Naval Research, where it has been since. The similarity and close relationship of MPL and ONR are not limited to their establishment, but, more important, are embodied in their mission and accomplishments.

ONR was established in recognition of the need to continue research vital for future technological advances. ONR became the first government agency with the primary mission to fund basic research performed by the civilian scientific community. ONR's work ranged over the entire area of science that was new and promising: nuclear research, low-temperature physics, solid-state physics, radio astronomy, basic biological studies, computer and information sciences. Most of the 15 nuclear accelerators built after the war at universities were started with ONR support.

ONR's research programs have been the substance for the continuing search for fundamental knowledge in the wide spectrum of scientific fields. This has resulted in the successful development of new hardware and the maintenance of the technology base necessary for future advances. It has resulted in new inventions, since the search for new scientific knowledge often requires instruments and measurement capabilities that do not exist.

This is particularly true in the oceanographic field in which MPL operates and where new instruments must be devised to obtain physical data from the harsh and often unfriendly environment of the sea.

Inventions stimulated by such engineering challenges can lead to new components, new subsystems, or entire new systems for naval uses. They result, through the transfer of technology, in innumerable beneficial applications to the civilian sector. In order to produce these results, imagination and innovation are not just desirable qualities: they are indispensable prerequisites. Innovations do not flourish in a climate of conformism or in organizations where initiative and inventiveness are stifled by the constraints of programmed operations.

Imagination and inventiveness are part, and have been a second nature, of MPL — evident through various expressions that range from the products of the scientific personnel to the demonstrated ability of the support staff to produce unique tools and equipment from an unlikely assortment of surplus material.

The record of MPL is a magnificent affirmation of the idea that the

alliance of the academic world with the Navy can work well to solve Navy problems. Even in comparison with some of the Navy's in-house laboratories, MPL has kept in close contact with the operating Navy's problems and needs. It has been imaginative and in front of the pack. It has been willing to think big and ahead of the pack. FLIP, for instance, which was built for a song, even by the standards of a dozen years ago, was a major departure from past capabilities.

After 30 years of research work, MPL has reason to be proud of its contributions. From the work here at MPL have come new signal-processing techniques which have been used for years in advancing our capabilities to study the ocean sound fields, and are now incorporated in the designs of the newest fleet sonars. These include DELTIC and DIMUS.

The search for knowledge about the sea and its boundaries, and their influence on performance of equipment important to the Navy's capabilities, has led to the development here at MPL of such unique and diverse tools as FLIP, the first truly stable floating platform at sea; RUM, still the only unmanned undersea vehicle capable of performing heavy work at great depths; ORB, a novel support craft for RUM, which has since proven its worth in support of other research work which requires operation of heavy loads through the air-sea interface; the Deep Tow instrument, which provides the greatest variety of scientific information on the seafloor, of great value both to marine geophysics and to naval capabilities.

The Navy recognizes these contributions and the scientific talent which spawned them. Two years ago Dr. Spiess, the Director of MPL, was awarded the Conrad Award, the highest scientific recognition that the Navy can bestow. It is named after Captain Dexter Conrad, one of the principal architects of ONR.

As we at ONR and MPL observe 30 years of service to the Navy and the nation, we must reflect on some of the changes that have taken place in these 30 years. Some of these changes have to do with the attitudes toward research in the military. In the last decade or so, we have observed a tendency from certain sectors of society to regard research in the military, as well as the military as an entity unto itself, at odds with the social well-being. This view was expressed in several ways. One of them was that naval research should concern itself strictly with projects of naval relevance. And the congressional appropriations for Navy research reflected this in a leaner budget.

The fact is that the military — the Navy — is part of society. It repre-

sents society; it projects society's aspirations and character. In research and technology, there is hardly an area which has no bearing to naval needs and vice versa.

In this regard, I am reminded of the late Technical Director of NUC [Navy Undersea Center], Dr. William B. McLean, whose recent death was a great loss to the Navy. As Director of NUC for seven years, and before that Technical Director of the Naval Ordnance Test Center (now Naval Weapons Center) for 13 years, Dr. McLean counted among his innovative ideas the invention of the Sidewinder missile. In a discussion that he had a few years ago with one of my predecessors at ONR, concerning the naval relevance of certain research, he said, "Anyone who can't find a naval application in everything we do lacks imagination."

I am sure that there is no lack of imagination at MPL. Over the past 30 years the work of the Laboratory has returned visible benefits to the Navy, in providing a better understanding of the variables of underwater acoustics. We look forward to your continued success in acquiring this understanding, in the development of novel devices which serve that purpose and lead to new capabilities for the fleet.

Some Recollections from My Years at the Marine Physical Laboratory

Kenneth M. Watson

I was Director of MPL from 1981 until my retirement in 1991. I then continued as Acting Director until 1993. I shall relate here a few miscellaneous highlights of this period.

First, let me describe a bit of background: Prior to coming to MPL I was a professor of physics at UC Berkeley. In 1977 several colleagues and I participated in forming the La Jolla Institute (LJI), a not-for-profit corporation. The other founders of LJI included Margaret Burbidge (of UCSD), Marvin Goldberger (then President of Caltech, now Dean of Natural Science at UCSD), Irwin Oppenheim (of MIT), and Elliot Montroll (of the University of Rochester). One of our principal objectives in creating LJI was to compete for the NSF Theoretical Physics Institute. This was a nationwide competition, won by UC Santa Barbara. Undaunted, we re-submitted our proposal for a Center for Studies of Nonlinear Dynamics to the Office of Naval Research. Admiral Baciocco, Chief of Naval Research at the time, took a direct interest and the center was approved with block funding for a three-year period. Bill Nierenberg gave us the use of the wooden structure T-25, and I came on a two-year leave-of-absence from UC Berkeley to be director of the Center. Admiral Baciocco came for the opening ceremonies in July of 1977.

When it became known that I would be Director of MPL, there was a suggestion from our ONR sponsors that it was unlikely that the LJI Center would be continued beyond its initial three-year period unless it were transferred to SIO/MPL. Both Bill Nierenberg and John Miles (then Vice Chancellor for Academic Affairs at UCSD) saw this as feasible and we anticipated that Henry Abarbanel might become Director of the Center. It turned out, however, that there were in-place interests within LJI who did not wish to give up the Center, and the transfer fell through. ONR did not renew its block funding and eventually LJI withered away. Henry Abarbanel did, however, join the MPL staff and set about creating the Institute for Nonlinear Studies, of which he is Director.

At the time that I came to MPL, it was one of four "University/Navy Laboratories." The other three were the Applied Physics Laboratory at the University of Washington and the two Applied Research Laboratories at Penn State and at the University of Texas. We were coordinated by Carey Smith of NAVSEA. Several times a year we met at one of the laboratories or in Washington, D.C., to discuss our programs.

As one of the four University/Navy laboratories, MPL had 6.2 block funding. This support had been in place for some time at a more-or-less fixed dollar level and was being severely hurt by inflation. In the spring of 1981 Fred Spiess had proposed to Admiral Baciocco that ONR provide a 6.1 Director's Discretionary Fund to the four laboratories. ONR began this program the first year that I was Director. This was, I think, very significant for the health of MPL at this time. With Admiral Baciocco's approval, we were able to use this money to add young researchers to our staff and also to provide "seed" funding for promising new projects.

Research at MPL covers a broad spectrum, including ocean acoustics and acoustic technology, physical oceanography, and geology and geophysics. I think that the Navy connection has provided a sense of coherence to these somewhat diverse fields. Although limited, state funding has been very significant in providing stability and some flexibility.

Many of us recall the NRAC review with less than pleasure. The four laboratories were each given a one-day comprehensive review with the notion that shutting down was an option. As so often happens in these matters, nothing happened.

An important step in developing MPL resources and funding occurred when the Office of Naval Technology asked us to participate in their Hi-gain program. This program had been under discussion for several years, and Ed Frieman, Walter Munk, and I had sat on several reviews of it. It was the very long passive acoustic array that brought MPL into the program. This array had remained unfinished for a number of years due to lack of funds and lack of strong interest in it. John Hildebrand undertook completion of the array, and both he and Bill Hodgkiss participated in its use.

Perhaps the greatest highlight of my time at MPL was the skillful handling of laboratory finances by Pat Jordan through both crises and good periods.

The Swallow Float Project

Greg Edmonds

The project was referred to as the Swallow Float Project in reference to the sensor pressure package, which was a 17-inch glass sphere that could be ballasted to be neutrally buoyant at a desired ocean depth. The sphere would descend to depth and float in the water column indefinitely, a behavior predicted and first demonstrated by Sir John Swallow of the National Institute of Oceanography in England. The buoys were outfitted with a hydrophone, three-axis geophones, a compass, a source transducer, a recorder, batteries, and support electronics to behave as an autonomous DIFAR sensor. A number of these sensors could be deployed as an array in the water column, and some fixed at the seafloor to serve as localization fixtures for the array. Following are a couple of recollections that come to my mind.

We frequently chartered small (inexpensive) boats to serve as deployment platforms off the coast of San Diego just beyond or inside the Channel Islands. We often spent a lot of time wallowing in the trough while ranging to the floats in an effort to track them during an experiment or during recovery. This was a source of considerable discomfort to a few folks (myself included). I was always on the lookout for some new medical technology to reduce the effects of motion sickness and came across an article in the *San Diego Union* (circa 1987). It related the results of a test, where half the guests on a fishing cruise had used transderm patches and the other half had taken ginger capsules prior to the cruise. The result was that both groups experienced little to no motion discomfort on the cruise. This sounded good to me, so I purchased ginger capsules to use on our next Swallow float experiment and convinced Marv Darling to try them also. This was in lieu of the usual remedies. Lee Culver, Marv Darling and I left port in the evening to arrive on station for a first-light deployment the next morning on a charter boat (*Scorpius*). The seas were a bit rough and, no sooner than clearing Point Loma, I began to feel the cold sweats. I decided right away that ginger was not going to cut it and applied my trusty transderm, then hit the rack. I arose about 0500 hours to begin preparations for the deployment. I got Lee Culver out of the rack and advanced to Marv Darling's bunk, announcing it was time to carry out

the experiment. Out of the dark from under a pile of blankets came a woeful “I am sick, I can’t get up, that doggone (expletives deleted) ginger doesn’t work!” Lee Culver and I deployed the instruments and a few hours later Marv showed up looking not so good but improving and able to work the rest of the experiment. That was the only time I ever saw Marv Darling more seasick than myself. Ginger root was never again considered as a motion sickness remedy. Lee Culver went on to get his Ph.D. based upon Swallow float experiment data, and Marv and I lived to do many more experiments.

I often find myself musing over another experiment (adventure). This one involved a number of resources, with deployment and data-gathering tightly scheduled over a two-week period. The exercise was in the Atlantic, 400 miles east of North Carolina. Up to that time we had deployed the Swallow floats only off the coast of southern California. Gerald D’Spain (then a grad student), Marv Darling and I were to carry out our part of the experiment coordinated with a number of other institutions and resources gathered to sail out of Key West, Florida. The Swallow floats had been prepped in San Diego and again checked in Key West. We sailed out of port in July 1990, headed for the experiment site. The floats were equipped with VLF radio beacons and Xenon flashers to aid in finding them when they were recalled to the surface. The first 30-hour deployment of 12 floats vertically sampling about 3000 meters of the water column was pretty routine. All but one float was recovered on schedule. One float (bottom mounted for localizing) inexplicably did not surface and must still be there, waiting! We spent several days refurbishing floats in preparation for the next scheduled deployment. The floats were deployed at a different site with a slightly different geometry, but still covering the water column from 300 meters to 3400 meters with about 2000 meters of lateral spread. We left the area for other exercises and returned about 30 hours later for the recovery. When we acoustically ranged to the floats, we were startled to find them as far away from the deployment site as 20 kilometers. We had only 18 hours allotted to do the recovery, and we started scrambling to recall the floats close to the ship. About an hour after the first float recovery, another revelation! We had floats on the surface that had not been recalled. It seems that the backup galvanic time releases (used to drop away the ballast) were prematurely releasing the floats, and within a few hours all the floats were on the surface and spread out over about 200 square miles of ocean. Fortunately we had the flasher and VHF radio beacons and the ship was equipped with a VHF direc-



Launching of the Swallow Float at Pt. Conception.

tion finder. We steamed off at full speed for the most distant float (20 kilometers), and fortuitously came across a couple of floats on the way that we had not even been able to detect from the deployment site. After recovering those most distant floats, we worked our way back, finally recovering all the floats (with not a little good fortune) in close to our scheduled time. We had been forewarned of the existence of Gulf Stream eddies or gyres of significant proportions, and it seems that we must have deployed in such, since the floats seemed to have moved in different directions and at different velocities over the course of 30 hours. We concluded that experiment with a sigh of relief at not having suffered a serious instrument loss.

They Also Serve Who Only Stand and Wait

Betty Shor

Dan Gibson says that, when he began work at MPL, Finn Outler told him that the “marine” in Marine Physical Laboratory means “we go to sea.” Outler and Eckart and Spiess and others surely said the same thing to many new employees. Those employees then told their wives, who at first probably did not understand. They began to understand when the first sea trip was approaching, when there were decisions to make about house repairs and family questions — just when the husband began working overtime on preparing equipment and was developing a faraway look at the mention of exotic ports (Papeete, Darwin, Antofagasta, Manzanillo, even Lahaina).

Some of us wives were lucky enough to see some of those ports, after a long flight to meet the incoming ship and to take a vacation with the researcher who was then leaving the ship. Antofagasta (Chile) was my first port, in 1957, after a 36-hour flight on a propellor plane from San Diego that was delayed by an engine failure in Peru that even had George worried. That’s the only time I was greeted with roses and champagne in the hotel room. Papeete was another charming port, in 1960, en route home from Australia, when the first jet airplane from Sydney to Los Angeles stopped there for 36 hours to give the pilots a rest after their previous long flight. A trip around the island of Tahiti on a motor scooter was unforgettable; so was Quinn’s Bar.

But most trips to sea by the husbands were — and are — a long dreary waiting time for the wives. Taking care of house repairs and family needs is just that much more difficult. It may seem rough on the wife when the kids are in diapers, but I know that it is rougher when the kids are teenagers.

In the 1950s letters were the only bits of communication, except for a rare message by ham operator. Once I accepted the charges on a late-night phone call from Fort Leavenworth, Kansas — which I knew was a military prison, but I was also sure that the call was from George, who was at the far western side of the Pacific (the ham operator was a prison guard); they had been trying for days to reach a ham in California to make some phone calls. Once I tried to reach the ship by com-

mercial ship-to-shore radio, gave the ship's name (either *Horizon* or *Baird*) and call letters, and the operator insisted that there was no such ship and that the University of California didn't own any vessels. Nick Carter at the Scripps radio station slipped the message through the next day (in spite of the rules).

It did seem as if something about the house always broke down within a few days after George had gone to sea. When home he was usually the handyman, so I had to learn how to get things fixed. He didn't really believe me about such breakdowns until the time I left town before he did. I departed on schedule with the children to visit my parents during his absence. His trip was delayed by ship problems in San Diego, and he had a group of out-of-town students who had arrived and expected to depart promptly. He put three of them up at our house because they couldn't stay aboard ship. Within two days, the plumbing backed up and the washing machine quit. When I heard about it, I just laughed.

In the early years the close association among Scripps Institution people was a great help to the wives, certainly to me. Not so many of the wives worked in the 1950s. Through the institution, they knew other women whose husbands were at sea, and they often got together for coffee or lunches or at least they compared notes by phone. Any information about how the sea trip was going was welcome, especially, of course, if it indicated an earlier arrival home. Sometimes rumors and gossip were rampant. But there was rapport and empathy for lonely wives among the women. The office staff at MPL was always helpful.

My own interest in how well the work at sea was going led to what has become the weekly report from the ship. Starting about the early 1960s, whenever George was chief scientist, he asked each member of the scientific party to provide the name of a spouse or family member who might want to hear about the work. Then he sent back through the Scripps radio station a weekly report that was printed in the SIO Log and a copy was mailed to the spouse or family member, by Gretchen Chambers or by Mildred Rogers in the MPL campus office. Before long this weekly report became a requirement for all Scripps expeditions or any expeditions by chief scientists from other institutions on Scripps ships.

Seeing one's husband off on one of those trips is dismal, as he is caught up immediately in the shipboard activity. Seeing him come in on the ship is a different world: the standing-still time is over, and life resumes. There is such a thing as a second honeymoon. During those

long sea trips, when acquaintances asked me, "How long has George been away?" I answered along the lines of "Twenty-three nights." I could always tell by their reaction the difference between those who understood and those who didn't.

Looking Backward — Looking Forward

Fred Noel Spiess

(Talk at 50th Anniversary celebration of MPL, 11 October 1996)

Introduction

From the beginning of discussions of a fiftieth anniversary celebration for the Marine Physical Laboratory, I have been appalled by the concept of trying to capture in some brief way the content of that entire time, or even of the 44 years of my own professional life that has been, and still is, shared with the other members of the Lab. Our peerless illustrator, Jo Griffith, with support from Chris Baldwin and others, has done a far better summarizing job than I could with her posters capturing the diversity, creativity and personality of these first five decades.

For me this started with my being recruited by Carl Eckart and Roger Revelle. It was not a difficult task for them, since it was immediately clear to me that this was a place in which I could combine my interest in the ocean, my experience from five wartime years as a submarine officer, and my Berkeley A.B. and Ph.D. education in physics. Having done my Ph.D. with Emilio Segrè (subsequently a Nobel Prize winner), I had some lingering guilt about leaving nuclear physics, but this was mitigated by the fact that as eminent a physicist as Carl Eckart was urging me to make this move.

I was fortunate, too, in the fact that Sir Charles Wright took over direction of the Lab shortly after I joined. He was a wonderful person, from whom I learned a great deal about the world in general, and research administration in particular. He had measured gravity in the Antarctic as a member of Scott's ill-fated expedition and strung communication wire among the trenches in World War I. During World War II he was Director of the Admiralty Research Lab in England, and told us stories about the "midnight follies" when Winston Churchill would, in the middle of the night, have some bright idea, and, cigar in hand, would gather the senior research leaders to hear about it and make some responses — scientific and diplomatic. After the war Sir Charles became chief of the Royal Navy Scientific Service, and his last position before retiring from the British research establishment

was in the British Joint Services Mission in Washington, D.C.; thus he knew his way around that area and was well known. He took me on my first research trip to Washington and guided me to the places at which MPL needed to be visible. More important, however, were the insights that he passed along that were useful in considering how to work effectively with the diverse personality types that are likely to be present in any leading research establishment.

Al Focke was the next MPL leader. Being already committed to leading a major research program outside the Lab (the Wigwam nuclear depth charge at-sea test), he was away quite a bit and taught me about delegation of authority by passing much of the day-to-day responsibility along to me. After that came my 22 years (1958-1980) as Director of MPL. During that period I spent most of my time trying both to follow along behind a bunch of hard-charging scientists and at the same time trying to be out in front where one could help open opportunities and smooth the way. My philosophy in this, and I think that of the MPL support staff, is that things should operate in such a way that Vic Anderson, Fred Fisher, or any of the others, would have no excuses for not accomplishing whatever it was we said we were going to do.

In parallel with these administrative concerns I have had the good fortune to be supported by a succession of very capable engineers (Bill Whitney, Maurice McGehee and Tony Boegeman), graduate students, and younger colleagues in maintaining a fruitful research program, in which I am now in the awkward position at age 76 of trying to figure out how, eventually, to extricate myself from the current web of exciting research commitments.

I want to use this brief time, however, not to talk about the research projects in which I have been involved, but rather to emphasize a few areas in which MPL has excelled, and which should be nurtured as we look to the future.

The Navy Connection

I shall start with our Navy linkage. That was very strong in the beginning. MPL had the momentum of its parent University of California Division of War Research (UCDWR) toward short-term Navy relevance, while watching for areas in which our scope could be broadened. We were very much concerned with, and capable of, producing ideas of immediate operational significance as well as with underlying questions about the physics of how the ocean really works.

That the Navy linkages were very close can be illustrated by the manner in which we worked on my first project (inherited from Leonard Liebermann) after joining the Lab in 1952. With a group including graduate students Vic Anderson and Bill Whitney, and engineers Maurice McGehee and Stan Lai, we were working directly with the local submarine force to implement a long-range low-frequency listening capability. The project, like a number of others, had support from the Headquarters of the Submarine Force, Pacific Fleet, and ComSubPac had assigned one of the local Division Commanders to be our liaison, and to see that appropriate support was provided. This level of cooperation included allowing us to install external hydrophones on a series of submarines, including wiring through special fittings into the boat, followed by at-sea operating time to make detection runs on other snorkeling submarines — a real commitment of two submarines for days at a time to this as their primary mission. Our project was successful enough that I was asked to go to active duty and ride one of our boats (USS *Blackfin*) on a three month intelligence patrol in the western Pacific, which was my first research expedition without the support of Bill Whitney or Maurice McGehee. (Our three-year-old son, who had seen me board the shore boat to go out to the nest of submarines in San Diego Bay, was of the opinion that I was out on the “taxi-water” for that period.)

This very close linkage soon disappeared into the more formal and centralized Navy-wide Operational Test and Evaluation Force, but this was paralleled by the establishment of a large number of advisory committees at all levels. As lead members of a small but respected laboratory, Vic Anderson and I spent a great deal of time on the overnight “red-eye” flights to Washington, since we were involved in this kind of activity at all levels from the Long Range Sound Propagation Committee organized by one of the lead project managers in ONR to the National Research Council Committee on Undersea Warfare. These activities mixed together those of us who were doing the work and had ideas about what might be done with the Naval officers who would be the users of what we were learning. I can still recall one committee in which at every meeting the Admiral (Martel), who was in charge of ASW, would give us a Secret-level briefing on where the USSR submarines were and what they were doing. Things of this kind provided a sense of reality and excitement that motivated the generation of ideas and underscored the significance of what we were doing. We were, through these mechanisms, in close touch with the Navy’s problems and the activities of our sister laboratories.

In the late 1970’s there were instructions from above to disband most of this committee structure. In my view this was a mistake, and since,

as Admiral Tobin has indicated, the Navy is going to re-invent ASW, it should also re-establish some similar mechanisms for coupling the Naval officers to the research leaders, and providing means for communication that would leak through the multiple filtering layers of civilian administrators.

In any event, from the MPL viewpoint, even though this is, perhaps, the least important of the areas that we should nurture, it is desirable, for the good of the country, that, even in times of declining Navy research budgets, we devise means for maintaining linkages to the operating Navy.

Support Capabilities

Perhaps the most important area of excellence that characterizes MPL, and that should be maintained at all costs, is one that Dan Gibson has covered very well in his presentation earlier this afternoon: our support capabilities. This excellence has been a part of the Lab's success from the beginning. It is no accident that among the original members of MPL that Carl Eckart brought with him from UCDWR were Finn Outler to see that work proceeded smoothly on all fronts, Chris Baldwin to keep things going in the administrative world, Archie Dunlap to start up our machine shop, and young engineers to collaborate with the group leaders in generating ideas and then followed by translating them into reality and making them work at sea. Built on this foundation, we have continued to maintain what is probably the best group in the University for taking our ideas and presenting them to NSF, NASA, ONR, or wherever, in a timely and appropriate manner, and then providing the necessary follow-up to see that the work is carried forward in an efficient and responsible way.

Providing effective support has become increasingly difficult as our research groups have spread out over the campus, and at the same time the paperwork and ground rules for administering our research programs have become more complex. Nevertheless, timely generation and implementation of new approaches have kept us in a position in which other groups in SIO want to join us in the administrative sense in order to facilitate their own work. This is a trend whose costs and benefits we should balance with care, since it could dilute the attention to our own projects that has been an underpinning of our success.

Seagoing Capability

A capability that has always kept MPL head and shoulders above most of the research entities with which we are usually compared is our

ability to do impressive things at sea with a sense of enjoyment. We go for the outrageous or routine, whatever it takes to meet the ocean's challenges. The atmosphere around the Lab is such that even new recruits without a particularly seagoing bent (e. g. Bill Hodgkiss) eventually become leaders at sea. Even within Scripps we are recognized as a major factor in this arena. We are usually heavily represented on SIO's Marine Operations Committee, as well as on the ship schedule. Here again this goes back into MPL's history, starting with Finn Outler as a major figure in conversion of Navy craft into research vessels, and going through Raitt and Shor's seismic-refraction operations, Vacquier's geomagnetic work, FLIP, RUM, ADA, and Deep Tow, into today's leadership in maintaining and using swath mapping systems, membership on major ship construction and refit committees, wireline re-entry, equipment recovery, etc.

Maintaining this attitude and capability is becoming increasingly important as other organizations and program managers realize that they can stretch their budgets and have more output by assuming that reality can be captured in computer models — what Chief of Naval Research Admiral Gaffney has referred to as “Cubicle Science” — in which computer outputs are called data. The major Navy laboratories, for example, have almost all lost their seagoing capabilities, and must contract out for at-sea work. Recent examples are the San Diego Naval Research and Development Division's SinkEx project, and an incipient Naval Oceanographic Office operation, in which we are taking part because we have the capability to do things that others cannot. Maintaining this aspect of our excellence is becoming increasingly difficult, particularly in the area of engineering talent. We have lost most of our leading engineers to retirement or changes to on-shore lifestyles, and we shall have to rebuild this expertise without having these knowledgeable experts as on-board mentors for the promising younger people who have been attracted to our staff.

Fearless Imagination

This element underlies our approaches over the years to the other items discussed above. We have had no compunction about imagining things that are new and different, while at the same time being achievable and useful. Beyond imagination, however, is a determination to transform the things we imagine into reality. FLIP is on the imaginative side, and so were RUM and ADA. We need to nurture this aspect if we are to stay at the forefront as experimentalists in the world of marine physics.

Imagination and conversion to reality have a place in the administrative world as well, and MPL has its share of contributions there, with such innovations (now commonly accepted) as vacation accrual accounts and re-charge operations having been generated and initially implemented by MPL. We should look forward to continuing to encourage this aspect of our lives as we have in the past.

University Participation

This has not been stressed in other presentations: we are part of a world-renowned university. Our reputation contributes to that renown, but we also benefit from it. Much of what we do is the better from our being, consciously and actively, a part of the University of California. Carl Eckart believed in this at the start and insisted that the Lab be granted three regular faculty billets as part of its establishment. This number has grown to nine active on the present roster. Involvement in graduate education has been an essential element from the beginning and, whenever new thrusts have been made, graduate and postdoctoral students have been brought along to share in the excitement and opportunities to be derived from the new ideas or technology that we have generated. In the context of this anniversary we have been working on assembling a list of all our Ph.D. graduates. We have 65 names by now, and still are remembering others.

Another aspect of this involvement with the University is that we have taken the trouble to help make the larger campus establishment an effective and useful organization. Carl Eckart served as an early Academic Vice Chancellor for UCSD; Vic Anderson served two terms as chair of the upper campus Department of Electrical Engineering and Computer Science; Vic Anderson, Bill Hodgkiss and John Hildebrand all have taught upper campus courses. We have taken part in and even led the activities of the Academic Senate (e. g., my tour as Chair of the Senate, Universitywide, and Faculty Representative on the Board of Regents), as well as serving on general campus administrative committees.

The fact that we play a part, and pay our dues through real participation, has had benefits for the Lab as well. This was particularly evident during the campus troubles of the late 1960's. Because we were well integrated into the larger campus structure, we had a better opportunity to explain to concerned individuals what we were doing, and what it meant to the University and the country. That opportunity was given to us because the University respected us as participants, and thus gave us a good shot at explaining who we were and what we were

doing. The result was that we were involved in a much more rational manner than our counterpart Navy-supported, university-operated laboratories whose connections with their parent academic structure were more remote. The senior staff might well make an organized effort to assure that we continue to be involved at all levels.

Supportive Families

The final important element is the existence of supportive families, particularly spouses. Many of us are away frequently, occasionally for long periods of time at sea. While I was preparing for this occasion, my wife Sally found a pair of letters that I had written in 1966, one to our two older daughters who were away at college, and the other to her at home with the other three children. In diary form I was recounting what I had been doing. It was: out on an expedition in FLIP north of Hawaii, an at-sea transfer, flew to Florida to go out in Aluminaut to test some new sonar with Vic Anderson and Chris Nickels, to Cleveland for a geodesy symposium, then the Long Range Underwater Sound Surveillance Coordinating Committee in New London and finally home. The letter then says "...and I've only been home for one solid week since June" — it was then October. As Bill Kuperman knows, some equivalents of that still exist, and his wife, Gaby, is as supportive as Sally. This sort of thing can only exist fruitfully if both members of the partnership work to provide each other some sympathy and pleasure when they can be together. This is an area in which we must all be concerned — not particularly for the Laboratory's directors — it comes with the territory — but for everyone. Such things as home-front communication while the seagoers are away, and arrangements that explicitly allow for using all that accumulated vacation are important.

These considerations apply not only to those who go to sea, but also to a need for supportive attitudes on the part of Milt Jordan, Bill Kennedy and other spouses of administration and shop people who find themselves working Saturdays and Sundays to be sure that our proposals are out on time or the equipment is ready to meet a sailing date.

Conclusion

Fifty years of success constitute both a blessing and a curse. The blessing comes with the fact that we generate our own self-confidence and a reputation on which to build when we talk with others. The curse is that in our everyday activities the expected standard of perfor-

mance is very high. I know that I have often been far less conscientious than I should have been in congratulating my fellow workers for their great performances. The truth is that great performance is what is expected, and only the super-great incident draws special notice. We need to keep reminding ourselves to look more often at other organizations for comparisons, and, occasionally at least, noting, without too much self-satisfaction, that we do impressive things.

Those of us active today have a challenge to maintain and use our imagination, our seagoing strengths and our role as an important element of the University of California to embark on a next 50 years whose achievements will dwarf those of the half-century just ending.