J.Lamar Worzel

Autobiography

CURRICULUM VITAE

Born: West New Brighton, Staten Island, New York
21 February 1919

Education: B.S, Lehigh University, 1940
M.S. Columbia University, 1948
PhD, Columbia University, 1949

Major Subjects: Undergraduate; Engineering Physics
Graduate: Geophysics, Geology

Professional Experience:

Woods Hole Oceanographic Inst.
Research Associate, 1940-46

Office of Naval Research, U.S.Navy
Consultant, 1950-51

Columbia University
Research Associate, 1948-49
Instructor, 1949-51
Assistant Professor, 1951-52
Associate Professor, 1952-57
Professor, 1957-1972

Lamont Grological Observatory
Founding member 1949
Assistan Director 1952-63
Acting director 1963-64
Associate Director 1964-72

Univ. of Texas, Medical Branch
Deputy Chief, Earth and Planetary Sciences, Division of Marine Medical Institute, 1972-74

Palisades Geophysical Institution
Treasurer, 1970-74
President, 1974-2002

Offices, Committees, Lectureships, etc:

Chairman, Oceanographic Committee, Society of Exploration Geophysics, 1974-76
Chairman, Special Study Group, Gravity at Sea, International Union of Geodesy, 1965-75
Visiting Lecturer, American Geophysical Union, 1965-70
Committee on the Permanence of Ocean Basins, American Geophysical Union, 1955-65
U,S,Navy Deep Water Propagation Committee, 1950-60
Vice President, Society of Exploration Geophysics, 1978-79
U.S.National Committee for the International Geological Correlation Program, 1977-79

Memberships in Scientific Societies:

Pi Mu Epsilon
Tau Beta Pi
American Physical Society, 1946-79
Seismological Society of America, 1946-79
Society of Exploration Geophysics, 1946-life
The Geological Society of America, 1946-life
American Association for the Advancement of Science 1946-1959
American Geophysical Union, 1946-
American Association of Petroleum Geologists, 1974-79
Geological Society of Houston, 1972-79

Honors:

1946-47 Graduate Residence Scholarship, Columbia Univ.
1947-48 James Furman Kemp Scholarship
1963 Guggenheim Foundation Fellowship
1964 U,S,Navy Meritious Public Service Citation
1971 Distinguished Lecturer for Society of Exploration Geophysics
1965-91 Cosmos Club

Autobiography 1990-2001

I b

FOREWORD

Why did I write my autobiography?

1. Because two friends and two of my children urged me to. (Frank Mongelli and Marie Thorpe, Bill and Sandy.)

2. Because I kept getting requests for historical information about early days of geophysics at sea, one facit of Oceanography.

3. Because I thought that some of the things I recounted might correct some of the misconceptions that I have read on occasion.

4. Because I thought that reading of our trials and tribulations and occurrences that stimulated us or hindered us, might someday help others trying to establish new fields of investigation.

5. Because I read that historians had found documents that ordinary people had written gave them insight to earlier times.

6. Because I thought I had an interesting life and I thought it was possible that others would think so too, and might enjoy reading about it.

The field of marine geophysics was almost nonexistent when , as an undergraduate student, I began going to sea with Dr. Ewing in 1937. The concept then, was that the ocean basins were permanent simple bowls that had been receiving the sediments from continental erosion since the formation of the oceans. There were a few exceptions. For instance a ridge had been encountered when laying telegraph cables between the U.S. and Europe. Of course there were oceanic islands such as the Hawaiian Chain. There were only "guesses " about the crust and mantel under the ocean.

The only geophysical techniques for examining the ocean floor were spot echo soundings, and pendulum gravity measurements on submarines. Isostasy was well established and many of the great deeps had been discovered, with their associated large negative gravity anomalies, although their total depth was mostly beyond the range of the depth sounders. Several submarine canyons incised into continental shelves had been found, although it was not known how far into the ocean basins they extended, and their mode of formation was a matter of conjecture.

The principal surface ocean currents were known , but their boundaries poorly determined. The main temperature, and salinity versus depth were determined, but little was known, except in a few particular places, of their variations in the oceanic realm.

Research at sea up to that time had been mostly done by a few national expeditions of a year or less, such as the Discovery Expedition of England, and the Meteor Cruise of Germany and several others. Often these cruises were separated by many years, even decades. Results were studied in every detail and mostly took many years to complete. These were mostly lengthy cruises primarily for temperature, salinity, and spot depth measurements, biological sampling and an occasional bottom sample.

By 1937, the Woods Hole Oceanographic Institution had existed about seven years and had a small permanent staff. Their ship Atlantis spent a large part of of the year in southern waters taking temperature measurements and salinity samples at representative depths. In the summer professors and some students, from several Universities, partially filled the building, we called The Oceanographic and now known as the Bigelow Building, at Woods Hole to carry on their research They usually were lucky to get a two week cruise on Atlantis in the deep ocean near Cape Cod. These cruises were usually single purpose cruises. Once in a while a second group was included if berths were available, to operate on a not to interfere basis. This latter mode was the one on which Dr Ewing and two or three students participated . There were projects on ways to observe ocean currents, sharks, biological sampling, paints to preserve ship's bottoms, and Dr. Ewing's attempts to measure the thickness of sediments in the deep oceans. Observations at sea were mostly made and returned to land for analysis.

Ship's positions and hence observation positions were only accurate to about three miles at times when astronomic slights were made. The position accuracy degenerated in between sights. In bad weather, positions could be known only to within ten or more miles. I've been on Atlantis when we were supposed to meet another ship at sea. Both ships claimed they were on position and neither could see the other even with the increased visibility of the 140 foot high mast of Atlantis. On one occasion it took more than a half a day for the ships to get together.

This was pretty much the situation when I joined the group working with Dr. Ewing. In those depression days there was little money for research, and Dr Ewing, at the urging of a couple of geologists, started to try to find out about the sediments on the continental shelf and later in the deep sea. With their help he had managed to get a small grant from The Geological Society of America. He had worked in the business of exploration for oil during his summers while a student., which is why he had been approached to make these observations. It was hoped that these techniques might be made to work in the oceans.

There was, at that time, no commercial equipment available to be bought, even if he had the money to buy it, and he didn't. He did manage to obtain some obsolete land equipment from friends that were still in the exploration business. It was obvious that this equipment could not be adapted for use at sea, so he decided that he had to build suitable equipment, although he had little experience in such skills. Probably no major research project was ever undertaken with so few funds and so little experience.

Thus the Ewing group had to use all their ingenuity, and the modest machine tools in the staff shop of the physics department, to construct all of the equipment for use on the ocean bottom, keeping expenditures to an absolute minimum. We had to learn the techniques of making pressure cases, or pressure equilibrated cases for equipment, to make mechanical and electronic equipment fit into the small spaces of pressure vessels, and handling explosives safely at sea and to fire near bottom , which had rarely if ever been done.. These all had to operate at almost 0 degrees centigrade and pressures up to 8000 psi. It was managed by making long hours of work and small test experiments drive the learning experiences needed. We never allowed ourselves think that anything we decided to do was impossible. These skills matured and evolved into those of the Lamont Geological Observatory and the Galveston Geophysical Laboratory and a number of generations of students

Oceano dtudies had made great strides and had a major role in the revolution of world geology, by the time I retired in 1979..

First Years

I was born on 21 February 1919 in my father's house on Delafield Avenue in West New Brighton, Staten Island, New York. My mother told me that I had yellow jaundice when I was born. I had a "Muvry and Daddy" and soon called myself John Lamar Peter Rabbit Worzel. Before the age of two I was operated on for a double hernia. The operation was done in the Children's' Hospital in New York City (Manhattan). Mother said she arrived on a visit to find me strapped down on a bed in a ward and other children were putting blocks and things in my mouth. She was outraged and had me removed from the ward.

At the age of about four we moved to 21 Hardin Avenue where the family lived until long after I had my own family.

When we moved to 21 Hardin Avenue the cement driveway had been newly laid and I attempted to ride my tricycle up the hill. My father stopped me after about 8 feet but the fossil record of the tracks are still retained by the concrete of the driveway.

At the age of five, while I was playing on the floor of the kitchen, my mother turned around and found me sprawled on the floor and turning blue (mother's report). It was identified as rheumatic fever and left me with a defective valve (scar tissue on valve) between the left auricle and ventricle of my heart. My tonsils were removed at the age of seven and I had mumps and measles, too, before at the age of nine I had a burst appendix.

My father and I had been going to the chiropractor on Friday evenings when I was about nine. At one of these I reported that my stomach didn't feel well. On Saturday I was kept quiet and the doctor was called. He was busy and didn't get there that day. On Sunday morning I felt worse and they called the doctor again.

They were having a roast of beef for noon dinner that day and I stil remember how good the smells were. Towards the end of their meal the pain became especially intense for me on the living room couch and I started screaming. Dr. Coonley came a short time later, said I had appendicitis and that the appendix had burst and I was in a serious way.

I was bundled off to the Staten Island Hospital immediately and Dr. Coonley operated on me and removed my appendix in the early afternoon. Peritonitis had set in and this was before penicillin. The only remembrance I have is waking up after the operation feeling the need of water. They wouldn't give me any because it was supposed to give a bad reaction to the ether anesthetic they had used. After many demands for water, they finally moistened my lips with a damp wash rag. I survived, but I still remember my need for water was the worst part of the whole experience for me. A few days after the operation the doctor came and said the drain in my wound was not working properly and he would have to reopen the wound as peritonitis had set in. He removed the stitches and tried with his hands to reopen the wound without success. He finally gave up and sent the nurse to get a scalpel. While waiting for the nurse, he pressed the back of his scissors on the skin just beyond the scar and the wound popped open! They cleaned out the wound and the smell was awful. For several days that I can't remember I was very sick. The infection caused a recurrence of my heart problems. I was sent home from the hospital after a two week stay and was forbidden to walk or to even stand on my feet. This lasted about four months. My mother carried me everywhere. I must have been a terrible burden for her.

The appendicitis happened just before Christmas 1928 and the doctor had advised my parents that it was unlikely that I would survive long and in any case I would not live beyond the age of 18.

Of course I was kept out of Arden School for the rest of the year. My parents gave me the best Christmas that a little boy could have that year since they thought it might well be my last.

After lunch each day, I would be bundled up in lots of clothes and blankets, put in a wheel chair and placed on the front porch in a reclining position. Most days I could not nap and I spent a lot of time watching the clouds overhead. Sometimes one or more of the kids in the neighborhood would come up on the porch and talk to me. This was a real event for me.

In May it was decided that I could stand up on my feet and in slow stages I was allowed to start walking again. At first my parents would hardly allow me to do anything. Finally one day I told them I'd rather die than be so restricted. I don't know if that did it, but anyway soon afterwards they let me do progressively more and more.

That summer my parents hired Mr. Warwick, the French teacher at Arden School, which I had been attending, to tutor me so that I could remain with my classmates. I didn't like Mr. Warwick and he didn't like me, but I guess he needed the money. For that reason and because all the other kids were on holiday while I was doing school work, that was a very bad summer. I made the mistake of telling Mr. Warwick that I didn't want to advance beyond my classmates. He assured me that wouldn't happen with lots of irony and he brought it up several times before my classmates in the remaining two years that I was at Arden School, to my great embarrassment.

That fall I went back to school with my class. From 3:30 to 5 PM we were to do our assigned homework, so we had none to do when we went home after 5. Mother or Father picked me up after 5. In the afternoons we had a sports period from 1:30 to 3:30. For most of the year we played soccer. Since I was restricted, they made me goal tender as that required the least activity. One time we went to Brooklyn to play another school. It was an extremely cold winter day and the locker room was unheated. The rest of the team got warmed up by running, but my goal tending allowed me to get colder and colder. When it got time to get dressed to go home, I could not work any of the buttons on my clothes. One of the other fellows buttoned a few buttons for me, enough to keep my clothes on, but I went home mostly unbuttoned.

Another time a ball was kicked at the goal which I caught. The forwards on the other team charged and jumped to block my kick or pass. Instead I ducked between the legs under one jumper and beyond him kicked the ball away.

The coach from the other team said he had never seen that play before.

My father , had a partner and ran a real estate law practice near our home. They also operated a Savings and Loan. My father was a director and Uncle Ted (not actually related) as secretary treasurer spent most of his time in the Savings and Loan. I learned that "Uncle Ted" lived with Mother and Father after they were married for 5 or 6 years. He finally married probably at the advanced age of 35 and set up his own home. While living with us at 21 Hardin Ave. he kept a large vegetable garden in our backyard. After he left this was turned into lawn, often to my dismay since I cut the lawn with a push reel mower after I was big enough to work it.

My father worked 5 1/2 days a week. On Saturday afternoons he would burn the weeks paper collected in a large barrel, clean and occasionally polish the cars and such other chores as he felt needed doing. As soon as I was able, I pitched in and helped him on Saturday afternoon. People must have praised me for it because I felt it was very important for me to work with father on Saturday afternoons. One time when I was about twelve, there was a Circus in town and mother proposed taking me to see it on Saturday afternoon. I declined saying I had to work with Pop. She tried all kind of inducements, but I was adamant. Start of a workaholic I guess.

Pop bought a second car and mother learned to drive while I was immobilized with appendix-heart problems. In 1927 father went on a train trip to the west with a group from the Kiwanis Club. He went to the Grand Canyon, Sequoia, Yosemite, Yellowstone and Glacier National Parks as far as I can remember.

In the summer of 1928 he sent mother and me on a train trip to Grand,Canyon, and Los Angeles to visit Uncle Earl, mother's brother who lived in Burbank. Uncle Earl took mother and me to Sequoia, and Yosemite in his Model A Ford over dirt roads on the way to San Francisco (actually Burlingame) where "Aunt Louise", mother's best friend, when she lived on Staten Island before she got married and moved to Burlingame. On the way home we visited Yellowstone. I got sick to my stomach on a sight seeing tour in Yellowstone, which was attributed to "altitude sickness". This trip was to make up to me the need to go to summer school while others were on vacation. Hence my sister Doris, two years older than I, didn't get to go.

In 1929 Pop bought a lot at Lake Mohawk in Sparta, New Jersey and built a summer home. It was called Te-We-Su for tepee (temporary home) on the western shore. Mother chose the name because of her affiliation as a leader in the Camp Fire Girls. Doris was in her unit. The idea of the summer home was to get me out of the city and involved in outdoor activities in the summer to regain my health.

The great bust in the stock market occurred in the fall of 1929 and the Great Depression began. It did not immediately affect us as most of father's investments were in real estate. By 1931 father was having a hard time too and we returned to become a one-car family again.

There were about 14 families in our neighborhood and about half of the men were out of work by about 1930. The rest mostly had their own businesses and managed to survive the depression years.

Father managed to hold on to our Lake Mohawk summer cottage, but he had to let go of most of the real estate property on Staten Island that he had bought as an investment. He was strapped, but not as badly off as most of our neighbors.

In eighth grade our class was chosen to collect money from our other schoolmates to buy presents for each of the Masters at Arden School. We decided to go to New York where shopping opportunities were greater. This led to going to New York without adult supervision which a number of us took advantage of at the Staten Island Academy where I went to High School. Arden only had eight grades.

HIGH SCHOOL YEARS

Having finished 8th grade, the highest grade at Arden School for Boys in 1932, my parents enrolled me in the Staten Island Academy. On my first day at SIA I was put in the eighth grade. I was highly incensed at being "put back". My father went with me on the second day and met with the headmaster and I was moved to the 9th grade. I don't remember too much of that year other than that I established myself as the class leader in math and well towards the top in the other subjects. In the early part of that year all of the boys in high school were participating in football. Because of my heart problems I was told I could not participate. Again I protested vehemently and my parents went to the school and arranged for me to participate. I also participated in basketball after football was terminated for the season and baseball and tennis in the spring. I was an also-ran in all these sports but tennis. I had taken tennis lessons at Lake Mohawk and played nearly every day during the summer and even won a junior tournament at Lake Mohawk. Consequently, I made the tennis team playing the 3rd singles (of four). We played all of the public High Schools on Staten Island and beat them. A match consisted of four singles matches and one doubles match.

In my sophomore high school year a trophy known as the Phelps Trophy was established to be won by the best High School team of the year. Any school that won the trophy three years in a row was to retire the trophy and keep it. We won it the first year. We won it the second year. The Staten Island Advance, the local newspaper administered the competition. Our number 1 and 2 players Ken Randall and Norris Rockstroh played the doubles match. Most of the other schools also played their #1 and #2 players in the doubles match too. At the start of the third year, my senior year, the rules were changed and players in the singles matches were not allowed to play in the doubles.

Anyway we won all of our matches and came down to the last match. Our #1 and #2 won their matches, our #4 (Larry Radway) lost his match and our doubles team, Carl Etlinger and Ned Harrigan lost their match while my match was still underway. I finally won and we were able to retire the trophy! To my knowledge no other trophy was ever established.

The only football incident I remember was when we played Fieldstone Academy (I think it was), a large private school in New Jersey, our team of 13 players arrived on their field to play their three full teams and about 20 substitutes i.e. about 53 players. This occurred in about mid-season. To say we were overmatched would be an understatement.

Nevertheless the game was played. They started their first team. We had them in check for a while, but towards the end of the first quarter they scored a touchdown. Then they sent in their second team. They scored another touchdown just before the half and they sent in their third team. In the second half they started their first team again. Early in the third period we scored a touchdown. It was now 14 to 6 as we failed to make the point after touchdown. We held them in the third period but early in the fourth period they scored another touchdown and extra point. In came their second team. We were able to keep them from scoring and near the end of the game in came their third team. I believe we were getting tired at that point. The third team scored a touchdown and extra point. The game ended with them winning 28-6. Their coach told our coach that our team was the only team that had been able to score on them that season.

I played half back on offense and right tackle on defense. I just couldn't run fast enough to cover potential pass receivers on defense. I preferred it that way anyway because on defense I could concentrate on trying to tackle the man with the ball.

Before the coach realized how slow I was running, I played right half-back on defense. Today it would be called right corner back. I made one brilliant play. We were playing a team that had lots of razzle-dazzle plays. On the play I am describing, it started towards their right side. One of their players moved well to the left and started waving his hands for the ball. Seeing that I rushed forward and reached him as he was just receiving a lateral pass from the quarterback, I tackled him immediately for a four yard loss. Otherwise I guess I was pathetic in the back field on defense.

The coach several times said he didn't understand it. On punts, my man never got into the back field, but I was always the first one down the field covering the punt yet he knew that I was the second slowest man on the team.

In basketball I was always on the Junior Varsity Team. Toward the end of my senior year the coach told me that I was good enough to play on the Varsity Team, but he could not put me on it because I was the only one on the Junior Varsity who would attempt to shoot baskets and someone had to at least attempt it.

In baseball I was always put in right field. Since the teams we played seldom or never had left handed hitters, the ball was seldom hit to right field. My batting was just as bad. The only play I remember was in my senior year, the baseball team played the teachers. Mr. Cunningham was our science teacher and used to pitch batting practice for our baseball team. Consequently he knew all of our skills or lack of them. He was the pitcher for the faculty team. When I came up to bat, he pitched five balls, two called strikes and three balls to me, trying to get me to strikeout on outside balls. On the last pitch, I figured he knew how poor a hitter I was and that he would throw a perfect fast ball strike down the middle. So I swung as he started to pitch and surprisingly hit the ball at the second baseman, Mr. Botsford, our headmaster. The ball was traveling so fast it went between his legs before he could move and I had a hit!

In my Sophomore year, our English class had the responsibility to get out the school newspaper, known as the "Echo". This was a two page mimeographed paper. Usually each student had to write at least one item for the paper. I took an interest in it and I soon ended up with the job of fitting all the items into a format and typing the stencils and running off about 100 copies. Copies were handed out to all the students during a lunch hour. All the students except seniors were not allowed to leave the campus during lunch. Seniors could go across the street where one could buy candy or sodas or ice cream, and some could sneak a "smoke".

During my Sophomore year I got my chance to try my future occupation - teacher. Mr. Cunningham, our science teacher came down with laryngitis one day and could not talk. He was supposed to conduct a class in Freshman Biology. I had made close connections with him because of my interest in science. He was unable to hold our class in second year biology and was about to cancel the class in First Year Biology. I went to him and told him that I had all my notes from First Year Biology and I could give the lecture for him if he would let me. He agreed and I conducted the class. I don't know what the students thought of it, but I thought it was fun. Mr. Cunningham told me that I had done a good job.

In my Sophomore year my sister, almost two years older, was a senior at SIA. Somehow we were both enrolled in a course on Ancient History. This was bad, because I managed to get better marks than my sister, which didn't help my relations with her at all. Our parents emphasized the point which only made it worse.

In my Junior year the only memory that stands out was that our class had to run the school store. This store had a few supplies like pencils, erasers, paper, pads, etc. and candy bars and ice cream bars. It was open only during lunch hours. Our class had to place the orders for supplies, open them and display them, man the store during lunch hour to make sales, collect the money, count and deposit it, and make out checks to suppliers. Everybody was gung-ho to participate at the beginning of the year, but gradually one or another dropped out. I felt it was important to keep it running and gradually I had all the jobs. I soon got tired of this and the teacher had to step in and assign people to jobs to keep the store functioning.

In my Senior year at SIA, Lawrence Radway came for that year. We soon formed a close comradeship. We lived close together so we traveled the same bus route home. The two of us got the best marks in all of the courses our class took, we played ping-pong at about the same level and I was #3 on the tennis team and he was #4. He especially liked history and english. I especially liked math and science. We held a year long debate of the relative merits of history, English, math and science.

I had been elected editor-in-chief of the "Quill", a monthly magazine mostly made up of stories and articles that had turned up as outstanding in the various English courses of each class. Larry wrote a good many of the items in the Quill, I wrote a few, but had all the work of assembling the articles, ads, etc., paging it, getting it to the printer and distributing it through the school. This included the lower school grades 1 to 8 too.

We had to take regents exams at the end of our senior year. These were New York State exams to establish the capabilities of students of all High

Schools. The regents exams allowed students precisely three hours to complete. I finished the Chemistry exam in one hour and took my papers up to leave. Mr. Cunningham insisted I go back and review my answers. Reluctantly I returned to my seat, but did not review my paper. After a while Mr. Cunningham came by and told me I might as well leave since I wasn't reviewing my work anyway. I got an 89 on the test.

The other regents exam that I remember was in trigonometry. This was my favorite subject in my senior year. I finished that exam in about an hour and turned in my paper. Mr. Tomis, my teacher, asked if I got everything right. I assured him that I had so he let me go. He was greatly disappointed since he had to give me a 98 on the exam. On the exam after completing the problem you had to copy the answer in a special place. On the wrong answer, I had worked the problem correctly and had the proper answer, but in copying the answer in the final place I had transposed two figures. Mr. Tomis was terribly disappointed as he said I was his first student who had worked all the problems correctly.

At the end of the Junior year in High School, towards the end of the year, the class was given a day off from school to pick daisies to grace the stage for the Senior Class Graduation. I had my New Jersey drivers license and my father let me take his car to take a number of classmates "Daisy Picking". We had a lot of fun picking them in the morning and arranging them on the stage in the afternoon..

During my Junior year a holiday came up that our school did not allow us. I think it was Washington's Birthday. A number of us got together, before school started, and complained bitterly that we did not get a holiday. Someone suggested that we take it anyway. Carl Etlinger, Norris Rockstroh, Bill Pott and I decided to do that and walked to the Staten Island Ferry, about a mile away and went to New York. On the ferry we all decided we would go to a burlesque show. In my devious way I pointed out that we would all be quizzed by our parents about what we did in New York and we'd better all have the same story. At that time the aquarium was close to where the S.I. Ferry docked on Manhattan Island so we all walked over there, made a quick pass through and went on to the Burlesque. We arranged to arrive home at the time we would normally return from school. Meanwhile, our parents had been contacted by the headmaster. So, we were all thoroughly quizzed and we all stuck to our aquarium story. It was only years later that our parents learned of our real escapade.

That evening our fathers were called to the school and the principal told them that he could not let the incident pass, but that in years to come this would be one of our greatest memories of our school years. We were suspended for a week. We were not allowed to have any of our school books. A week at home, alone with no one to play with, seemed an eternity. When we returned to school we were informed that when the other students were occupied in sports, we would have to complete the missed school work before we could rejoin our sports activities. We were put in a room, but we were not supervised. We had two hours each school day to catch up on the missed school work. At first we talked and fooled around and didn't make much progress. Finally, I realized it was going to be a long time before I would return to the basketball court, so I buckled down and finished in about two weeks. The others took over a month to finish as they continued to talk and fool around a lot. We never tried it again.

Finally graduation time arrived. It was customary for the Senior Class to go somewhere for fun instead of to our classes on the last day of school. Our class chose Mr. Cunningham and his wife as our supervisors and went to our family's summer cottage at Lake Mohawk for the day, where we swam and used my motorboat and canoe and had some drinks. We returned to our homes about 4 P.M., got dressed and went to the Meadowbrook Club in Montclair, N.J. for dinner and dancing. They had big bands there every night.

The following night was our graduation night. After graduation four of us graduates got in my father's car and drove around the block in St. George, where our school was located, several times blowing our horn loudly. Then we adjourned to one of the graduates home where we had a big party which included drinking. That was the last time I ever saw my classmates (13 of them), except for Larry Radway during Christmas vacations, while we were still in college.

COLLEGE YEARS

My father allowed me to choose my own college. He took me to Lehigh, one of.my choices, to see the place. I went on my own to Columbia which would have accepted me, but I did not like it because of its location in the city. I also considered MIT, but did not ever visit it as it required one year of high school Physics. Our high school didn't have the equipment for the lab work and didn't have a Physics course. Probably if I had written them they would have let me in conditionally subject to passing a Freshman Physics course. That idea did not occur to me. So I finally decided on Lehigh and Mechanical Engineering. Joe and Bruce Boyle, close friends at Lake Mohawk and I hung around together most of the time. Joe also decided to go to Lehigh for Business Administration. Someone in Lambda Chi Alpha fraternity knew Joe and recommended him to the Chapter at Lehigh. Joe and I went to Lehigh together and stayed at LXA for rush week. Joe was picked to join LXA but I was not, so at the end of rush week I moved to a boarding house on Montclair Avenue. I did get a bid from one fraternity, but it was across town from the campus so I turned it down.

In the rooming house, I had a room on the third floor. Al Vine, a Physics graduate student, and Ted Brown, a senior at Lehigh had rooms on the second floor. Naturally, we saw a lot of each other and we all had meal tickets at the diner at the foot of Montclair Avenue. A meal ticket cost $5 and allowed you to choose $5.50 worth of food from their menu. A meal ticket would normally last us about one week, three meals a day.

Shortly after the school term started, one of the lab assistants in the Physics department didn't show up. This left them with too few lab assistants. Al Vine who had lived in Garretsville, Ohio knew that Norman Webster, his classmate at Hiram who lived in Cleveland , had not found a job and was looking for one. He contacted Norm and persuaded him to become a graduate student and lab assistant (employee) in the Physics department. Naturally, he came to live in the same boarding house that we did.

I had signed up as a Mechanical Engineer since I believed that I had been good at taking things like clocks apart and understanding how they worked. My classes and studies didn't keep me fully occupied so I went to my faculty advisor and asked if there was anything I could do in my spare time. He arranged for me to "help out" in the machine shop of the Mechanical Engineering Department. On my first day I went to the machine shop and reported to the foreman. He assigned me the job of refacing some instruments which needed new round glass faces. He then left me on my own, Since I had no idea how one cut a round disc out of a piece of glass, I dithered around a couple of hours and left without accomplishing anything. I never went back there.

I then joined the Photography Club to absorb my spare time. They taught me about film, emulsions, film development and photo enlargement. I bought an Argus camera (35 mm) that had just come on the market and started taking pictures around the campus. As part of this experience we mixed all of our chemicals from the components.

Towards the end of October, Al Vine came to my room and told me that he was working with "Doc" Ewing, who had recently completed a trip on a submarine to measure gravity. To complete the work he needed to make some base measurements and required a 35 mm. camera to photograph a part of the observation. It would require some modifications to the camera. They had been all over Bethlehem and could not find any Argus cameras in stock. Would I consider selling mine to the project? After a little agonizing I agreed that cameras promised to the local stores in a week or 10 days would satisfy my needs and I would sell my camera for the cost of a replacement. It was $15 as I recall it. This was my first "contact" with Doc Ewing.

In my first semester at Lehigh I failed Freshman English. It came about this way. As part of the English course we had to write essays and another part required reading short stories to learn good presentations in English. One of the stories involved an English teacher who went mad. In our discussion of the story, our teacher asked why the man went mad. As a wise guy, I quickly piped up "because he was an English teacher". This did not endear me to my teacher, and afterwards nothing I wrote, satisfied him. Result, I failed the course. Apparently this fact was reported back to the Staten Island Academy, because I received a letter from the Headmaster, Mr. Botsford, deploring the fact that a former editor of the "Quill" could fail an English course and assuring me that he had confidence that I would overcome this difficulty. That was the only course that I ever failed! It probably kept me from making Phi Beta Kappa as I missed out on that by .01 of a point in my senior year.

Photography ceased to take up all of my spare time so I volunteered to help in the research Vine and Webster were doing with Ewing in the Spring. Since they were preparing a paper on making Seismic Measurements at Sea, I was put to work drafting illustrations. I could do this because I was well along in the drafting course required of all engineering students. Don Crary was also drafting illustrations for that paper. He was paid from Work Progress Administration (WPA) funds to help him with his school expenses. I wasn't paid. So many of my fellow students could not go to college without such help that I felt that I could not apply for it since it wasn't necessary for me. I never asked my father about this; I often wonder it he would have felt the same way.

Each Friday, as a freshman, each student was scheduled for a one hour visit (with a group of about thirty) to each academic department. The department would make a presentation of what a person specializing in that department would study and what type of jobs he would qualify for after he earned his degree. The Physics department gave a presentation that appealed to me. The Physics program was considered the hardest one to complete. I liked that challenge, so I transferred from a Mechanical Engineering program to an Engineering Physics program.

In the fall of my Freshman year 1936, in early October, I think, on a Friday evening about 9 P.M., Al Vine came to my room and told me that Ewing, Vine and Webster and a former student were going to New Jersey to meet George Woollard to shoot seismic profiles across the Coastal Plain of New Jersey. The former student had not shown up and they were shorthanded and would I like me to come along to dig shot holes. This consisted of using a hand auger that would dig a 6" diameter hole to depths of 10 to 40 feet. I had a Saturday morning class and I would have to cut it to go along. After talking it over with Vine for about an hour, I decided I could cut my class and would go along. I guess this minor decision settled the rest of my professional career.

On that weekend, we left Bethlehem about 5 A.M., drove to Princeton, N.J. in Floosey Belle (Dr. Ewing's 1934 Ford car). Floosey Belle was a product of the depression. She had two front bucket seats and a removable back seat. There were two side doors and a door at the back so that she could be used as a delivery truck when the back seat was removed. As a passenger car seating five when the back seat was in place. Three of us would sit in front, and one in back sharing the space with the oscillograph, amplifiers, cables and geophones that would be used to record the sound waves. There was no back seat in the car then. The hand augers and extension pipes and a reel of two conductor wire were strapped on the outside.

At Princeton, George Woollard would join us with his Model "A" filled with cases of dynamite and we drove to the location for making the observations. On that first occasion we were shooting the Barnegat Bay line. This consisted of about eight locations from a point near Princeton, N.J. , at the fall line of the Coastal Plain, to Barnegat Bay at the shore line at the New Jersey Coast, a distance of about 40 miles.

At a station we would park Floosey Belle alongside a secondary road and place the geophones (6) about 200 feet apart on each side of Floosey Belle on about a Southeast Northwest line. Then we would string a two conductor cable along the road to the shot point, which varied from about 100 feet beyond the last geophone to a distance of up to a mile increasing to about 8 miles at Barnegat Bay. Spacing and distance of shot points were determined by the results of previous shots. We dug shot holes along the road as near as possible to the fence line, as we could, so that if we were accosted by the landholder we would aver we weren't on his land we were on the road right of way, and to government objection that we weren't on the road right of way we were on private land. When the shot was loaded we would telephone Ewing at Floosey Belle that we were ready. When he had warmed up the amplifiers and the geophones were quiet Ewing would tell us to shoot. He would hand crank photo paper through the oscillograph until the sounds had all arrived at the geophones.

The shot instant was transmitted along the telephone wires to the oscillograph. Timing the various sound waves to arrive at the geophones would allow us to calculate the speed of sound in the various nearly horizontal layers beneath us and the thickness of each layer. After the recording was completed, Ewing would take the photo paper out of the oscillograph camera in a light tight box and put the box in "Minnie's drawers" which was a black mantel covering a box containing a can of developer, a can of water and a can of hypo. By winding the paper back and forth in each can appropriately the record would be developed and it could be examined. Ewing made hasty picks of the arrival times and decided the distance needed for the next shot. Sometimes we would have to make up to 10 successive shots.

At one station, when the shot distance was over one mile, our connection to Floosey Belle failed. By backtracking along the line we found a section of several hundred feet had been cut out of our line and had disappeared. This took us a while to repair and precipitated the decision to use radio connections instead of phone lines as soon as we could.

The State Geologist of New Jersey who was financing our work would occasionally visit us in the field and help out. He never inquired about the legality of handling dynamite or radios that were unlicensed and we never thought to burden him with such details.

Bob McCurdy, who was an amateur radio nut, was recruited to join the group to design and build radios and keep them working. He was a regular in the remainder of the land work. After that first weekend, I too became a regular in the work.

Usually my classes would be over by noon Friday. I would then go to the lab and join whoever was working to get the equipment ready and loaded on Floosey Belle. We'd quit when we were loaded, get some sleep and return to leave for New Jersey (usually about 5 A.M.) in time to go to work at daylight. As we moved our work closer to the coast we'd have to leave earlier. We would work until dark, eating sandwiches and drinking milk at noon without interrupting the work. After dark we'd find a rooming house, nearby, get some supper, wash the records thoroughly, usually in the bathtub at the rooming house, read the records carefully, plot the data on graph paper and make calculations of the layer velocities and thicknesses, make plans for the next day and go to bed at midnight. We would ask to be called about 5 A.M. so we could get breakfast and be in the field to go to work at daylight. Sunday night at dark, we would find a diner, get a meal and drive to Bethlehem and unload the equipment. I'd often get back to my room at 2 A.M. and would have to get up at 6:30 to get to my first class at 7:45.

During the week, we would often have to make repairs to our gear. This sometimes caused a shortage of time for classes and all. At that time I was averaging less than 6 hours per night of sleep. We always had to remove the gear from Floosey Belle on our return because Ewing had to use the car for his family during the week.

On one occasion, early in the morning, I had the rumble seat of my Ford coupe, a second hand car my mother had persuaded my father to buy for me, filled with dynamite. While crossing the central square of the town of Silverton, N.J. a bread truck on a cross street ran into the rear fender of the car. The road the bread truck was on was about 100 ft. wide, parallel to the beach. We were on the main road heading away from the beach. Even though it was before 7 o'clock in the morning, a group of about 10 people gathered around. As far as I could tell, only my fender was crumpled.

One onlooker claimed he was a body shop worker and insisted on my opening the rumble seat so that he could see how much damage had been done. I refused saying that our rumble seat was chockablock full with camping gear and that I was not going to unload it all so that he could take a look. Ultimately I prevailed and we got away unscathed. The bread company paid for my repairs about $20 as I recall.

Assisting in the seismic work I learned how to test continuity in electric lines, test for voltage in a circuit, how to dig auger holes in sands and clays, how to handle and fire explosives, the rudiments of radios, how to make a short but accurate radio report, how to lift and carry and many other things. It paid off because in my laboratories in physics and various engineering courses, I would accomplish the three hour lab in an hour or two, while most of the other students would complain that they did not have enough time to complete their report.

The seismic work on the Barnegat Bay line (finished in the fall of 1937) was such a success that we made a Cape May line in the spring of 1938. This consisted of 12 stations from the fall line near the Delaware River to Cape May, New Jersey a distance of 60 miles. In the papers about these profiles Ewing developed the analysis of reversed seismic stations for determining true velocities for the Barnegat Bay line and the system of least squares fitting for the reversed profiles on the Cape May line.. The seismic apparatus developed for making records on the sea floor in the summer of 1937 and 1938 was used for these profiles. All of the equipment was built by Ewing, Vine and Webster. I helped on some of the building of the geophones.

Since I had been spending so much time involved in the research program in New Jersey and helping build instruments, (this was where I first learned how to use machine tools under the tutelage of Al Vine) my grades suffered in my Sophomore year. They fell to a 2.4 average for the year from the nearly 3.0 average I maintained in my college career. (4.0 was max).

Towards the end of my Sophomore year, our whole class in the engineering school was given a general test to determine our progress in general terms. I placed 17th in our class of about 350 to my surprise, the surprise of the Physics Dept. faculty and my peers. I attributed it to the many things. I had learned in the research program in which I was participating.

The first attempt to determine the structure of the ocean floor in deep water was carried out in 1937 and after some improvements in 1938. The equipment was constructed in 1936, 1937 by Ewing, Vine and Webster. Basically it was a robot that would be lowered onto the ocean floor, proceed with its program of operation and then was recovered. It was lowered on a one half inch diameter wire rope from RV. Atlantis. The equipment consisted of a main instrument chamber (Oscillograph) cable connected to four geophones spaced 100 ft. apart. Beyond the last geophone three explosive bombs were attached to the cable beyond the outermost geophone at 1,000 ft. intervals. The object was to lay all of this equipment on the bottom in line. Since the geophones were affected by motion, the 500 lb. oscillograph was attached to a large lead weight (about 1,000 pounds) by 50 feet of chain. In turn the lead weight was attached to the 1/2 inch wire rope that lowered all of this equipment from R.V. Atlantis, about three miles above on the sea surface.

When it was believed that the farthest bomb was nearing bottom, the ship would get under way at slow speed to try to lay all the gear in a line on the bottom as the wire rope was paid out. After the large weight reached bottom the ship was stopped and wire rope paid out slowly trying to avoid tugging on the weight due to ship's drift and wave motions. After the bombs fired the equipment was recovered by reeling in the wire rope.

Since no one had ever fired explosives at the ocean floor where the pressure approached 8,000 pounds per square inch, and the temperature was close to 0°C, Dr. Ewing had been advised that the bombs would have to be in pressure and watertight cases as TNT would not explode at the pressures and temperature at the sea floor. Cylinders were constructed to hold 1 pound, 3 pounds and 10 pounds of TNT. These cases were filled at sea by melting the TNT on the deck of the Atlantis in a device called "Vines Still", by steam piped from the engine room. An electrically fired detonator was included in the charge and two leads through the cover allowed the necessary circuits to be completed. Naturally, no such connections had been made up to that time so these too had to be constructed.

The wires to each bomb were soldered to the "lead ins" . A lead in had a tapered head so that the water pressure would not push it into the case. It was insulated from the casing head by bekelite washers and the connections were protected from the sea water by a small cap, which surrounded the lead ins, filled with melted tar. These proved messy to use and expensive to build (especially since the whole project was financed by the Geological Society of America with grants of about $5,000 per year to build all of the equipment, buy explosives, and all of the cable for electrical connections. These original bombs were exploded in the order from the farthest to the nearest since the electric cable would be severed by each bomb.

Geophones were not available commercially at that time so the four geophones (actually six were constructed) that had a large alnico magnet about four inches in outside diameter and three inches in inside diameter. A center pole piece about one inch in diameter was attached to an iron disk that spanned one end of the alnico magnet. A pole piece 1/2" thick of iron was added on the opposite end of the alnico magnet leaving an annulus about 1/2" diameter from the central pole. Thus a radial magnetic field existed between the latter pole piece and the central post. On a bakelite form a coil of about five thousand turns was constructed which would fit in the radial field without touching either side. This coil was supported by a thin flat piece of spring metal to the frame that supported the magnet. An aluminum case surrounded the whole construction and was filled with motor oil to provide suitable damping. A sylphon bellows was attached to the case so that the pressure was equalized between the ocean outside and the motor oil inside. The geophone had to be oriented so that the cylindrical case axis was vertical.

When a vertical motion from a sound wave would move the geophone case, the inertia of the coil would tend to keep the coil from moving. The magnetic field was moved so that the wires of the coil were cutting the magnetic field, generating a small electric current. Two lead ins similar to the ones on the main case were connected to the electric cable conducting the current to the amplifiers in the oscillograph.

These amplifiers, which at that time used vacuum tubes, had to be very small to fit in the main case. Regular flashlight batteries powered the filaments of the vacuum tubes and 4 small 45 volt "B" batteries supplied the plate voltage. The amplified signals were fed to a series of D'Arsonval galvanometers which had mirrors attached to their moving coils. The nine galvanometer bank had to be constructed too, as none were available of such small size. The whole bank was about 6 inches long, 4 inches wide and about 5 inches tall and contained nine separate galvanometers. Each of the four geophones had two galvanometers hooked to it each with a different gain to cover the possible wide range of signals that might be received. The ninth galvanometer received a signal from the firing of each bomb, the shot instant.

Each galvanometer element was 1/4" square and about 3 inches long. A twenty turn coil of wire was wound on I beam shaped piece of aluminum about 3/4" long. Each end of the coil was attached to a flat gold wire about 1" long. The bottom gold wire was attached to the frame as a ground connection. The top gold ribbon was attached to a .030" rod about I" long. This rod was fed through an insulator inserted in the end of the top of the frame, a small soldering lug (also had to be manufactured) then to a small about 3/4" compression spring, through a washer, retained by a smaller rod passed at right angles through the galvanometer rod. A small mirror about 1/8" by 1/4" was glued just above the coil to the flat gold wire. The spring kept the suspension appropriately tight. The small rod at the top allowed the suspension to be turned so that the mirror could be aimed. The solder lug along with the grounded lead allowed the amplified signal to be fed to each galvanometer.

As the amplified signal from the geophone was passed through the galvanometer, the coil would twist in the magnetic field supplied by a U shaped alnico magnet. The aluminum coil form in the magnetic field provided eddy current dumping so the galvanometers wouldn't oscillate back and forth from an impinging signal. The twist, caused the light beam from a line filament bulb, reflected from the mirror to move back and forth along a slit opening in the photographic camera, thus recording the up and down motion of the ocean floor from the seismic waves from the bombs disturbing it to be recorded as a wavy line on the moving paper record in the camera. I believe the galvanometer bank described above was the largest number of galvanometers, nine, constructed as the smallest galvanometer bank that existed at that time.

To provide timing for the photo record, a tuning fork was equipped with a slit attached to each leg of the fork with a line filament bulb located so that each time the fork vibrated the slits would line up and allow light to fall on the camera slit. A suitable driving circuit to cause the fork to vibrate had to be provided. Thus lines were put on the photo record at 1/100 th second intervals. The paper in the camera was driven at such a speed that the fork lines were about 3/16 inches apart. Thus times could be estimated on the record to about 1/1,000 sec.

The camera slit was provided with a cylindrical lens so that the fork illumination of the slit would make very fine lines and the vertical linesreflected from the galvanometer mirrors would become sharp dots on the photo paper. The photo paper was about five inches wide. The record then consisted of one galvanometer which recorded the shot instant, eight galvanometers which made wavy lines following the motion of the geophones with black timing lines across the whole record at 1/100 th second intervals.

The camera of course had to be built to shield the photo paper from all light except what came through the slit. A small electric motor operated the camera mechanism.

A timer that could be set to close a switch at a chosen five minute interval, from one to twelve hours after it was set, started the operation of the equipment. When the timer switch closed it started a motor which drove a commutator. The commutator was constructed to provide current to the amplifiers shortly after it was activated, two minutes later to light the lights and start the camera. 10 seconds after the camera was started the shot the farthest away was fired, after another 10 seconds the second shot was fired, after another 10 seconds the third shot was fired, after another 10 seconds the camera was stopped and the lights, fork, amplifiers, and camera were shut off.

After the shots were heard on board ship, the wire rope was hauled in and the equipment recovered. After all the gear was on deck the oscillograph case had to be opened, the camera recovered and taken below and the record developed. Four attempts to make records were made in the summer of 1937 with this gear. This was only one program of several on a two week cruise on Atlantis. By the time the gear was made ready, the pressure case closed, the new bomb wires and bombs connected and all attached to the wire rope more than an hour would have passed. By the time the gear was put over the side, and the equipment lowered to bottom, three to four more hours would have elapsed.

Usually the timer had to be set so that there was more than adequate time to get the gear on bottom, so there would have to be a wait of a half hour or so before the bombs would go off. Recovering the gear took a good four hours of hoisting and an hour to get the various pieces of apparatus on board. Thus an operation was usually started at daylight and wouldn't be completed till nearly dark.

None of the attempts to make records were successful in the summer of 1937 because of the ship tugging on the wire despite everyone's best efforts to avoid this.

Underwater Photography

On our trips on Atlantis for seismic measurements, it would take about a day and a half to cross the continental shelf each way going and coming. Since we did not have too much to do at those times, Ewing and Vine had built an underwater camera. The ship would stop for about fifteen minutes about each three hours and we'd lower the camera to bottom, take a couple of pictures and recover the camera and the ship would resume its course and speed..

The camera was an Argus camera which was mounted on an aluminum head looking out of a small glass porthole. The aluminum head was fitted on a large pyrex glass test tube which we called a pyrex penis. The glass test tube was about 6" in diameter, about 3/8 inch thick and about four feet long. The end of the tube was ground flat and the aluminum head was held against the end of the tube by large elastic bands, cut from an automobile tire inner tube, strapped to the tube. The watertight seal was made by stretching a piece of inner tube about 1 1/2" wide across the seam between the tube and the head. Batteries were included within the tube which would wind the film one space and fire flashbulbs. Electric leads synchronized to the camera were fed through the aluminum head to the trigger which fired the camera on bottom contact and another sent to a small flashbulb which was enclosed in a automobile gasoline glass filter bulb to protect it from the water pressure. The camera was lifted off bottom and held suspended off bottom for 30 seconds to allow a motor to advance the film, then was lowered a second time to take a second picture. The pyrex penis provided enough flotation to bring the camera back to the surface. The trigger weight caused the camera to sink to bottom and was,dropped after the second picture was taken. A light "cod line" was attached to the pole to which the camera and flashbulbs were connected so the camera could be pulled back to the ship without having to maneuver the ship to the camera.

About 200 pictures were taken on the continental shelf and on George's Bank before the camera was lost on a lowering to 1,000 fathoms. We knew it was marginal whether the pyrex penis would stand the 3,000 pounds per square inch pressure at that depth, but we decided to take a chance to get such a picture at that depth. Only a small piece of the pole was recovered. We believe the pyrex penis imploded and shattered the pole. This was the second camera that Ewing et al had made. The first had taken one rather poor picture in 2,900 fathoms before it was lost. These were the forerunners of numerous cameras we made over the years with which we took thousands of pictures at all ocean depths up to 3,000 fathoms.

Since the bombs for the cable connected OBS were expensive and time consuming to make, an experiment was conducted in 1937 that successfully fired powdered TNT enclosed in a weather balloon (a heavy rubber balloon) The powdered TNT would form a solid block under pressure with air surrounding it. . Thus the experts were wrong. TNT would fire under temperature and pressure conditions on the ocean floor. In the summer of 1938 bombs were made this way which also avoided the somewhat dangerous and very messy step of casting TNT on the ship. To avoid breaking the electric firing wires passing by the explosion, at each bomb the wires were attached to a willow stick about 10 ft. long which was bent into a curve with the bomb stretched between the ends within the nearly half circle that the willow stick made. It would fire without damaging the electric cable passing by on the curved willow.

In 1938 one observation gave some data. It showed a layer velocity of 5,700 ft. per second which was interpreted as sediment and showed a minimum thickness of 600 feet. Again we were plagued by the ship tugging on the instruments. Sometimes the equipment was laid in a jumbled pile, probably due to unknown currents affecting the ship while the equipment was being laid on bottom. Sometimes there would be damage to cables and instruments from being dragged on the bottom. On one occasion one of the bombs got caught in a loop of cable and was found near the instrument case. Fortunately it had not fired. It was considered likely that it was only a matter of time before a bomb would destroy or blow off the oscillograph. In any event, it became obvious the whole string of bombs and instruments were not being laid out in line as desired. Therefore, it was decided that another way to do the experiment had to be found.

The hurricane of 1938:

We returned from sea in September 1938 and we were making 12 knots under sail on Atlantis, perhaps her fastest under sail. It turned out we were riding the forefront of the e hurricane that hit New England. We landed at about 9 A.M. and since I was going to be late for class, Webster and I loaded my 1936 Ford Coupe (with rumble seat) with gear and left Woods Hole for Bethlehem. We actually left about noon. By the time we got to Buzzards Bay the hurricane was hitting the Mainland full force. Someone had stepped on Webster's glasses and broke them that morning so he could not drive and I had to do all the driving.

As the wind picked up signs were being detached from their poles and were blowing across the road. Webster and I would watch to see any signs ahead that were about to blow into the road and I'd speed up or slow down in order to avoid it. By Providence, R.I. most of the traffic lights were not functioning, but since there were few cars out we got through all right.

The section of road to New London did not have many trees close so we did not have much trouble other than the veering of the car in the winds. From New London on we were on US 1 which goes close to the shore. There were a lot of trees down, many across the road. We'd come up to one, find several cars stopped, we'd pass them and find a way around or over the trees and the cars that had stopped would follow us. In some places, power lines were down and sparks were jumping from the broken cables to nearby trees, or sign posts. We knew that our tires insulated us, so we'd pass stopped cars and drive over the wires and pretty soon some of the cars that had been stopped would catch up to us and pass us having followed our path across the live wires, only to be stopped by the next one. Pretty soon we started to find low places where the sea had crossed the highway. If we could tell the water depth was less than 3 ft. we would slowly drive through them and continue on our way. Finally, we were stopped behind a line of about thirty cars near Madison, Connecticut. We,walked ahead and realized the water was too deep so we returned to our car. It was then about 10 P.M. We decided we were going to be there for a long time and that we'd need something to eat. Fortunately a Dugan's Bakery truck was stopped nearby so we went to him and persuaded him to sell us a couple. of coffee cakes. This started a rush and pretty soon the Dugan's truck was all sold out.

Meanwhile cars had been accumulating behind us for at least a mile. At that time Route 1 was only a two lane road with narrow shoulders. Some wise guy in back decided to pass all the stopped cars by driving in the left lane. A lot of others followed and we SOON had at least a mile of two lanes filled with cars up to the flooded section.

Unfortunately on the far side of the flooded section the cars had done the same thing. At about 3 A.M. when the water had fallen enough for cars to get through, about three cars from our lane would move ahead, about three cars from the outside lane would move in behind them and on the other side of the water the same thing was taking place so our six cars could move ahead about 6 car lengths and their six cars could move ahead about 6 car lengths and this had to be repeated over and over again. The result was that it was 6 A.M. before we got through the water and past the car jam. After we got underway again, after about one hour, we passed out of the area, messed up by the hurricane and were able to stop and get some breakfast and coffee. I surely needed it. The rest of our trip to Bethlehem was routine.

Dr. Ewing and Al Vine left Woods Hole about 2 P.M. in Floosey Belle during the beginning of the hurricane; then Dr. Ewing had to stop for about 1/2 hour in Plymouth on business and then they drove farther inland towards Bethlehem. They also experienced flying signs, downed trees and power cables on the ground spitting sparks, but they were less numerous than the ones that we experienced.

They reached a river where the bridge had washed out so they could not proceed further. A man with a flashlight stopped them and told them of the washout. They asked if there was another way across the river. He said yes, that about 10 miles north there was another bridge, but he did not know whether or not it was passable. They turned north and were faced with a maze of roads and they had no idea which ones to follow. Nevertheless they made choices and came to a man with a flashlight who told them that the bridge ahead was washed out. They asked about an alternate crossing and he said that about an hour previously he had sent a couple of fellows north and they had not returned so that he guessed the bridge above was open. Then Ewing and Vine realized they had circled back and this was the same man! They eventually returned to Bethlehem too.

Free Floating Instruments:

In the fall of 1938 Prof Ewing received a Guggenheim Fellowship which allowed him to take leave off his academic duties for a year to concentrate on his research project. He had decided to build a separate oscillograph for each geophone and a separate firing unit for each bomb synchronized by suitable timers. A former student of Dr. Ewing's that had taken a job at the Hamilton Watch Company had invited us to come to visit and bring the crystal chronometer, which had been invented and built by Dr. Marrison of Bell Labs, to compare with Hamilton's "Shortt Clock" their standard by which they rated all their watches. We had this instrument because of the submarine gravity work that Ewing had done on the submarine USS Barracuda in 1935. The Shortt Clock was supposed to be accurate to about a part per million as was the Crystal Chronometer. All the tests we made by varying voltages over acceptable ranges and other things showed no variation between their clock and ours until 4:30 when a big change occurred which ended at 5:15. This coincided with the factory making watches shutting down for the night affecting the "Shortt Clock" which returned to its excellent operation again after 5:15. The Hamilton Watch Company was surprised and took immediate steps to correct the problem.

The signal from the Shortt Clock was piped throughout the factory so watches could be rated at many locations. The shut down (and probably the start up in the morning) changed the load on the Shortt Clock and affected its rate and affected the rating of some of the manufactured watches. Hamilton immediately went to work on the problem and were able to correct it quickly. In appreciation of our participation they gave us a dozen Hamilton Watches (men's pocket variety) to use in coordinating our new seismic instruments. The Hamilton Watches had to be modified by removing the hands and replacing them with commutator disks. Feelers to ride on the commutator disks had to be made to mount on the watch faces with insulated leads attached to the outside so that outside contacts could be made. The watch bezel and crystal face was retained to protect the mechanism from external effects.

Al Vine made a protective case in which to keep the watches with suitable circuits so that each watch would light a button when the hour and minute hands contacted their feelers. The second could be written down by the button and the button pushed to turn off the light so that the others could be similarly marked. Before each use the seven best synchronized watches would be used for that day's experiment.

In the fall of 1938 we all got busy building individual oscillographs, which all had to have the same parts and functions as the previous multiple unit. These galvanometers were built as units of three instead of the larger unit of nine galvanometers used in the former instrument.

We built four oscillographs and constructed aluminum pressure cases to house them. Under the pressure of 8,000 pounds per square inch and the temperature of near 0o C the insides of these cases shrunk a little and the oscillographs inside had to have frames slightly smaller so that this constriction did not damage the instruments. We also had to build five bomb shooters. These too had to have aluminum pressure cases, a timing watch, batteries and a relay to switch on the firing current for the bombs.

To bring each of these units back to the surface we had to build floats. These were made from about 10 ft. sections of heavy neoprene hose (about 10" inside diameter, filler hose for large tank ships) with aluminum heads to seal each end. One end had a vent so they could be filled with gasoline. Beneath the instrument chamber we attached a release mechanism and cast iron weights of sufficient weight to cause the unit to sink rapidly to the ocean bottom. The weights were made as disks of cast iron about 10" in diameter and about 1" thick. Each disk weighed 10 pounds in seawater.

The release mechanism consisted of a metal can with a diaphragm with a 1" hole in it. A small bag with a block of solid salt was retained above the diaphragm. The weights were attached to the salt bag. When the salt in the bag dissolved, the weights were released and the gasoline float would return the instrument to the surface.

Smaller geophones were made housed in aluminum cases. These were suspended off to the side of the oscillograph cases by a triangular frame of bamboo. Each cavity in the bamboo had to have a hole drilled into it so it would flood with seawater otherwise the frame would be collapsed and destroyed by the water pressure. The geophones had to be deployed to the side so they would not tangle with the sinking weights and so their cables were slack when the weights were on bottom.

The size of the block of salt determined how long the instrument remained on bottom. Of course we had to make extensive laboratory tests to find out the time it took for enough salt to dissolve that the bag would be pulled through the hole in the bottom of the release to leave the sink weight on bottom and allow the instruments to float back to the surface. A deflecting shield had to be attached below the release to prevent the rapid solution of the salt in the bag by the motion of the unit while sinking to bottom.

Each oscillograph had to have a camera (using 35 mm photographic paper) with a cylindrical lens, a light source, a turning fork for making time lines, an amplifier, a three galvanometer bank, two sets of batteries, and a starting watch.

There were no known gasket seals for the instrument heads available at that time. So we had to design and test our own. We also had to test the instrument cases to make sure they would not collapse at a pressure of 10,000 pounds per square inch. To test them we obtained a 14 inch in outside diameter naval shell and sank it sharp end down in a hole in the ground floor room of our laboratory in the physics department. The base of the shell had a very heavy plug to seal the explosive powder in the shell. We could test cylinders up to eight inches in diameter and up to three foot length in the cavity of the shell. The shell was made available to us by Bethlehem Steel Co. as excess Navy property. We mounted a 1 ton hydraulic automobile jack on top of the shell after drilling a hole into the bottom of the jack that mated with a hole in the shell plug. The jack piston was plugged so that it could not move and pumping the jack caused oil to pass into the shell. With sufficient pumping we could raise the pressure up to 10,000 pounds per square inch. A copper gasket clamped between the base of the jack and the shell plug prevented leakage between them.

A former student of Dr. Ewing's, then working at Johns Manville, made a cylindrical ring of rubbery asbestos made in a circle which served as a gasket between the plug and the shell. Probably this was the first O ring seal. The threads on the shell plug were left handed so that the rotation of the shell leaving the gun barrel would not cause it to unscrew. This feature caused a humorous incident which will be related later.

The cylindrical instrument heads had to be made with two steps with only .001 inch radial clearance in each step. This was because the external pressure on the whole head so compressed the neoprene rubber of the gasket that it would squeeze the neoprene into the clearance spaces if they were any larger. These gaskets were made of 1/8 thick sheet neoprene with a 1/2 inch wide strip of circular neoprene.

All of this equipment and tests had to be completed by Thanksgiving because Dr. Ewing had tickets to Bermuda where he was to meet Dr. Woollard to test the system in the deep water easily reached from Bermuda. His wife and son were going to Bermuda with him. The Monarch of Bermuda was sailing about 1 P.M. on the Saturday after Thanksgiving. The equipment that Ewing was to take with him was not completed as Thanksgiving approached. So all of us tried to complete it in time. Vine and Webster and I had been helping to get the gear ready all of the time when we were not occupied in classes. Starting Wednesday noon we worked continuously until about 9 A.M. Saturday without stopping other than to eat occasionally. At 9 A.M. we loaded all of the equipment in Floosey Belle, Webster's Packard touring sedan and my Ford rumble seat coupe and started for New York.

It had snowed about 6" deep on Thanksgiving day, catching the authorities by surprise. Since little snow removing equipment was ready the snow had been packed into an icy consistency on the roads by the traffic. We didn't get under way for New York until about 11 A.M. and on dry roads it would take 2 hours to make the trip. Being short of time, we all drove about 70 m/Hr. most of the way on the icy roads. At some traffic lights where cars were stopped, Dr. Ewing would try to stop and would go into a skid turning around and round and would pass the stopped cars often going backwards in a skid, I would pass them on their other side in a full skid and Webster would come last also skidding passing on one or the other side. Without stopping Ewing would regain control and start East again with us following. Fortunately, we never hit anyone or each other even though there were red lights for our direction of travel.

Despite our efforts the Monarch of Bermuda, with Ewing's wife and son on board while Ewing still held their tickets, was slipping her lines and tooting her horn to warn of her moving away from the dock when we arrived. Doc did his best to get the authorities to hold the ship with no success. He later told us that in desperation he was trying to decide whether to shed his coat or not while he tried to haul himself on board hand over hand with the one line still connected to the dock, when that line was thrown to the dock from the ship.

Ewing arranged with the shipping line about the tickets for his wife son and for himself to take the trip leaving one week later with all of the equipment. I had arranged with my folks on Staten Island to have Vine and Webster and I to have a belated Thanksgiving Dinner with them on Saturday night after the ship had sailed. I tried to persuade Doc to join us, but he would not and returned to Bethlehem. Mother served her usual super Thanksgiving Dinner for us. She and father later told me it was funny to watch the three of us. We'd fall asleep while we were eating and would wake with a start when we'd start all over and try again. Finally, she decided there was no use and got us to go to bed without completing our meal. We had a warmed over Thanksgiving meal on Sunday noon. She was a good sport and never complained of our failure to appreciate her hard work Saturday. I must say we all did more than justice to it on Sunday.

We returned to Lehigh Sunday afternoon and worked as much as possible for the following week on the gear. It probably worked out for the best since the gear was far enough along when he left the following week that Ewing and Woollard, who was joining him in Bermuda, could finish completing the gear with just hand tools. Ewing and Woollard deployed the oscillograph units and bomb shooters five or six times during the winter and spring but did not get any usable records.

They found out that surface currents would cause their deployment of the various units to be in a different line than that which they had followed in deploying them. It was always hard to find them after they surfaced even though each one had a 6 ft. pole with a red flag on the top. Once they found one, it was usually fairly easy to find the others. When Ewing returned in late spring with the above information we added two more buoys to the line. One that we deployed shortly after the last instrument that would go to bottom, stay on bottom about one half as long as our instruments and another that would make the trip to bottom and return immediately. We thought this would make it easier to find the line of equipment.

Operation of the Free Floating Instruments

The mode of operation of the free floating instruments was to steam at full speed along a chosen course line. A bomb shooter and bomb would be put over, one thousand feet later another bomb shooter and bomb would be released, one thousand feet later a third bomb shooter and bomb would be released, five hundred feet later an oscillograph and geophone would be released, at five hundred feet intervals the other three oscillograph and geophones were released, five hundred feet beyond the last oscillograph the float to remain on bottom a half hour was released and five hundred feet beyond the float to go to bottom and immediately return to the surface was released.

Then the ship was hove-to to await the two hours it took the units to reach bottom. If all went well we would then hear the bombs fire. About 2 hours after recovering the buoy returning directly from the bottom, we would start looking for the flags of the other buoys. After the last buoy was over the side the ship would drift away from the site for about three hours under the influence of the surface currents and the wind. Our greatest efforts to site the buoy flags were naturally aimed upwind. If no buoy had been sited by an hour after we expected the bottomed units to surface, we would sail slowly upwind for a couple of miles that we believed we had drifted since deploying the instruments. If one was spotted then, we'd institute a search in the area. After a buoy was found, we'd pick it up, identify which unit it was and then search harder along the line of travel we used while deploying them and or its reverse.

On Atlantis, Vine and I would climb up to the spreaders, 100 ft. above the deck and using binoculars search around the ship for the buoys. Webster and Ewing and the ship's officers would search from the top of the deck houses. The spreaders were round, about six inches in diameter, and about six feet long. A stay from the masthead, a wire rope about 3/4 inch in diameter, would pass the end of the spreader and be attached to the rail near deck level. This made a very good handhold to steady one on the spreader. At first we must have bent the wire stays with the strength of our grip to maintain us on the spreader. After the first two hours, we got quite accustomed to it and we became quite casual with our grip.

Sometimes it would take us most of the remaining daylight hours to find the line of buoys and we'd be recovering them well into the dusk. Most times we would find them while there was ample daylight. Of the about thirty times that we deployed this group of instruments, we only lost one instrument. This was a bomb shooter. The bomb failed to fire and was too heavy for the float to bring the equipment back to the surface. We had a lot of misfires in the summer of 1938. We attributed this to the detonators having an air space at their top end. We believed the pressure was collapsing the detonator, damaging the circuit and causing the misfires.

We decided that if we put the detonator inside a brass tube, the powdered TNT would pack around the detonator forming a seal that would prevent the detonator collapse. We put together a small bomb with a detonator inside such a tube and put it in our pressure vessel to test it. At a pressure of about 4,000 PSI there was a loud thump and the pump on the pressure vessel stopped working. Vine and I were examining it to see what was the matter, when Ewing, who had been working in his office, came running in and asked what had happened. I told him. He asked what the pressure was. I looked and told him about 6,000 PSI. He asked what it had been when I last looked, and I said 4,000 PSI. He told me to open the pressure release valve and a spout of burnt oil spurted out. He then got us all to leave the lab posthaste.

We went to the far end of the hallway and discussed what was going on. We then realized that the campus policeman was due by soon and he often looked in our lab window, which was on the ground floor, and visited with those of us working there. Feeling that seeing the oil and cinders spouting from our pressure vessel he might come in, we decided we'd better pull down the window shades. I ran back into the lab, pulled down the window shades and jumped out the window rather than pass close to the pressure vessel again. After about one half hour, we returned to the lab to find the pressure released. We opened the pressure vessel and found a mess of cinders, oil and water emulsified, a spot on the ceiling where the vent spot hit, thoroughly soiled with oil and cinders and an oily emulsion dripping off all of the furniture, ceiling, walls, etc. When we recovered the remains from the pressure vessel we found the tube in which we had enclosed the detonator had collapsed causing a secondary explosion, that is the TNT to bum instead of explode and the detonator exploded apparently from the fire. This mishap turned into a great boon later on in our work at WHOI

The lab was a real mess, with burnt motor oil, cinders, and oil and water emulsion coating everything in the lab. This happened about 10 p.m. Vine and I spent all night cleaning the externals of the lab so that it didn't look much worse than usual by the time the department staff started to arrive the next morning. It took us about three days to complete cleaning the lab. We had a rack of cubby holes, just larger than a cigar box, with small parts stored in cigar boxes for later use. We found emulsified oil and water and cinders inside small cardboard boxes which were in the cigar boxes which were in the cubby holes. It is still a mystery to me how the mess could have so thoroughly distributed itself. The spot on the ceiling was just beneath the spot of the chair of the department heads secretary sat during the day on the floor above. Needless to say, we did not repeat the experiment. Instead Dr. Ewing got his friends in DuPont to build us detonators that had no air spaces in them which we used after that.

In the late fall of 1939 the engine of my 1936 Ford was using large quantities of oil and running badly. It needed new pistons, new main bearings and probably more. Instead I bought a replacement engine for about $75 and over one weekend Ed Uhl and I took out the old engine and put in the new one. Neither of us had done this before, so we did it by the seat of our pants. I rented the use of a large garage around the corner from our apartment. Included was a frame on rollers with a chain fall capable of lifting one ton. We had to take out the whole radiator and surrounding coverings leaving the front end open. We removed the bolts securing the engine to the frame, and those securing the engine to the transmission. After taking the engine weight on the rolling frame we had to pull the main engine shaft spline from the clutch. This meant lifting and sliding forward about one quarter inch at a time for at least three inches. When the engine was free we lifted it high and rolled the frame forward until we could lower the engine onto the floor. Then we had to remove everything, the generator, starter, the exhaust and intake manifolds, and the cylinder heads from the old engine and install them on the new engine block. Replacing the new engine was even harder than removing the old one as aligning the spline was so difficult. We finally finished about 5 p.m. Sunday after working till midnight Saturday and starting at 7 a.m. again Sunday stopping only to eat and sleep. To our surprise when we put in oil and started the engine, everything worked!

At Lehigh, everyone was required to take two years of Reserve Officer Training Course (ROTC). In the spring of my Sophomore year we were assigned to learn to disassemble and reassemble the BAR (Browning Automatic Rifle). One also had to name each part as this was done. We were given a month to learn to do this and to practice. Since I didn't want to have to take the course over I practiced this chore repeatedly. When the results of the test were announced, I had had the best time to accomplish the task and no errors in naming the parts. Towards the end of the ROTC course, we were all asked to sign up for summer camp of two weeks and the advanced course for the remaining two years. I decided that I did not want to go on as it mostly required memorization which I was not good at, and the summer field camp would interfere with my summer work with Dr. Ewing. In the last week of classes, they read off the names of those that had signed on for the advanced course. The list was correct except my name was included. I protested and I was asked to see the instructor after class. When I did, he did his best to convince me that I would have a great future in the ROTC and later in the Army if I so chose. I emphatically declined. I believe this mistake was the result of my success with the BAR.

In the spring term of 1939 1 was taking an Electrical Engineering Course in DC and AC electric motors. Our Professor would come into class and ask if we had read our assignment. He would then ask if anyone had any questions. Usually there were none, so he would send us to the blackboard and have each of us work a different problem given at the end of the chapter of our text. After some time he would ask each of us in turn to explain our problem and our solution. After about two weeks of this, I made sure I had a question each day. After another two weeks he would lean against the door jamb on the outside of the room and then gradually rotate into the class room and start his routine. He added a new twist though, he would say does anyone but Worzel have a question. When there was none he would then say what is yours Worzel. After answering the question very succinctly, he would proceed as before.

Normally in college I wore corduroy trousers and a suede jacket. One day I was going home for a weekend so I appeared in his class in a suit and tie. He asked me who I was going to bury. I, of course, answered I am leaving for home right after class. During the class it started to pour rain outside causing everyone to turn and look out the windows. Without thinking I spoke loudly and said, "Stop". The rain stopped abruptly and class resumed. Pretty soon it started to rain again. The Professor turned to me and said, "Let's see you do that again". I just smiled.

On one exam he took off five points because I had given the answer as 25.2 rather than 25.23. 1 went to him and pointed out that the data he had given us was only given to 1 part in 300 and that we had been taught in the Physics Department that we could not get any answer more accurately than the data we had. He reluctantly backed off and took only 2 points off my grade instead of 5 and refused to go any farther.

Towards the end of the term I was elected to the Newtonian Society (honorary mathematics). At the dinner in honor of the new members I happened to be seated next to the Professor described above. Towards the end of the meal he turned to me and said that he had been teaching the course I was taking with him for five years, and that I was the only student who had fought back at him. He said that he commended me for it.

During the summer Dr. Ewing was able to allow us $15 per week for expenses. Finding it hard to live on that, working 18 hour days, seven days a week and with our hearty appetites, even with $5 meal tickets, we appropriated a ten foot by 14 foot tarpaulin and about six blankets from our lab supplies. We doubled over the 14 foot length making a bed 10 feet wide and 7 feet long and put our blankets inside. We doubled it over and rolled it up making a roll about 5 foot wide and about 2 feet in diameter. This we stowed in the rumble seat of my Ford coupe during the day. About 11 or 12, after work, at night we'd drive out in the countryside, find an empty field and unroll our bed. On good nights we'd throw back the tarpaulin on top and crawl into the blankets at the level that gave us our desired warmth using the other blankets as our mattress. On rainy nights we'd pull up the tarp over our heads and sleep warm and dry. Normally we got up 6 to 6:30, went to the diner and had breakfast, and got to the lab no later than 7:30.

One night we worked later than usual and were very tired so we laid our tarp-bed on the campus near the Physics Department and went to sleep. We believed our usual wakeup would have us long gone before any campus life would begin. We overslept and woke up to find one of the campus cops, our friend, standing over us. The campus was alive with people moving about. Vine and I, in our underwear, fought to get dressed in our cocoon. The cop stood by watching us and laughing at our contortions.

Pacific Cruise??

In the spring or early summer of 1939 Dr. Ewing was approached by a Professor of the University of Virginia about making a year-long cruise in the Pacific Ocean. He and his students would be making biological measurements and samples and we were to make seismic refraction measurements, ocean bottom photographs, and ocean bottom gravity measurements using pendulums. The cruise was to leave in September on a 300 ft. Coast Guard Cutter. As I remember, it was named the Alexander Hamilton. We had the camera, just needing a pressure case for deep deployment, the free floating seismographs, and a set of Gulf pendulums. We believed we could make a pressure case for the pendulums once we were at sea as well as a means to cause it to operate on reaching bottom. Dr. Ewing agreed that he, Al Vine and I would go and do the geophysics program.

We still made our two week cruise on Atlantis, which we were allotted, figuring we could get the gear to operate better. The bombs did work, but our oscillographs failed to operate even though they would operate perfectly in the laboratory on Atlantis just before a lowering and just afterwards. We were frustrated but had to get on with getting ready for our Pacific cruise. A freight car was obtained in Washington, D.C. to carry all of the expedition gear to the West Coast where we were to board the ship. In early September I was sent to Washington, with Floosey Belle fully loaded with most of our gear to be loaded on the freight car. It was raining and the window on the driver's side was broken so it could not close. The result, I was getting wet continuously throughout the drive to Washington. I got there and loaded all of the gear on the freight car and started back to Bethlehem. It was about 6 p.m. by that time. I was starting to shiver since I was continually getting wet. On top of that, with no load in Floosey Belle, when I hit a large bump, and the roads had plenty of them then, a shock absorber on the back would overturn and lock up. If you continued to drive, the shock absorber would break, and we had no money for repairs. Each time this happened I'd have to get out in the rain, jack up the back of the car, return the shock absorber to its proper position and then lower the jack. Of course it was getting colder and colder as the night progressed. I had to do this operation about four times on the five hour trip back to Bethlehem.

When I arrived, I was shaking uncontrollably. I quickly changed into dry clothes, took a 100 cc drink of 180 proof laboratory alcohol, unrolled our tarp-bed on the lab floor and crawled into the middle of all the blankets. I continued to shake and shiver for over an hour. I recovered and had no serious effects from the experience, although it must have been the first stage of hypothermia.

About a week later we were informed that President Roosevelt who had wanted the trip to be made, had canceled it ostensibly because of the need for the Hamilton in the Atlantic with the war under way in Europe. After Pearl Harbor, we often joked that the trip was planned so the Japanese would have subjects on which to commit atrocities. In all likelihood, it was planned as a legitimate means for surveillance in the Pacific.

Back to Normal?

Ed (Whitey) Uhl and Bill Pohlman two other students in my Engineering Physics class had attended ROTC summer camp. The three of us had become close pals and I persuaded them to join us in the lab between the end of their camp and the beginning of the new semester. Since none of us had much money, we found a room in the attic of the Physics building which had a bench, a table and some chairs in it and fitted with gas fittings. We got a couple of Bunsen burners and had our lunches and dinners there. We had no refrigerator so one of us would go to a delicatessen and get some rolls and cold cuts or cheese and some milk and have sandwiches for lunch. For dinner we got some meat, a couple of potatoes and a can of vegetables. Sometimes Webster and Vine would eat with us and on rare occasions even Dr. Ewing. We then found out that Dr. Ewing and Crary used to eat there in previous years, but not as regularly as we did.

When our trip was canceled and the fall term approached, Uhl, Pohlman and I rented an apartment across town and cooked all our own meals, cleaned our apartment and studied together. This was our last year before graduation. About once a week Dr. Ewing, Vine and I would go to a public park and grill steaks, pork chops, or lamb chops and sit around the camp fire awhile. The park had fireplaces and always had wood available.

Just before Thanksgiving the apartment owners notified all of the tenants that they would have to relocate since they were going to renovate the apartment building. We quickly located another apartment that a householder had made in the upstairs of their house. We got Vine, Webster and Hersey to help us with the move and we arranged to make it on Thanksgiving day. We bought a fifteen pound turkey, stuffed it with a four pound meat loaf and had it cooked in the ovens of a bread company a few blocks away. We cooked about a peck of potatoes and about four packages of frozen peas and a large pot of onions on the stove in our new apartment. We bought two pumpkin pies from the bread company. We got all the furniture moved and in place in our new (?) apartment by about 3 p.m. The six of us sat down at our kitchen table extended by two card tables and ate everything that had been cooked for our Thanksgiving dinner. There were no leftovers.

When graduation came in June 1940, Uhl and Pohlman who had stayed in the advanced ROTC program accepted their Second Lieutenant appointments and became officers in the U.S. Army. Hersey and Vine joined me and we reserved the apartment for the fall term as I had received a tuition scholarship from Lehigh U. and was accepted as a graduate student. However, this never happened as you will see in the succeeding pages.

One humorous event that occurred in the summer of 1940 happened as follows. We had a cylinder which we could put in the presses the Civil Engineers maintained in Fritz Lab. I was running a pressure test on some new device there, while Vine was running another pressure test in the naval shell in our laboratory. Ed Douglas, a former student of Dr. Ewing's, then an engineer at Johns Mannville, was visiting and assisting Vine. Vine came over to Fritz Lab and asked me if I had any ideas, because the head was jammed on the naval shell-pressure vessel. I had used it most and had had the most experience. With great bravado, I told him that I could open the vessel all by myself. When we got to our lab I found they had an 8 ft. lever of 2" X 2" steel on the head of the pressure vessel, attached to a six fold block and fall and had attempted to pull the rope on the block and fall through the window attached to Floosey Belle. I had them disconnect their Rube Goldberg rig, reversed the lever on the head of the pressure vessel, leaned against it and opened the pressure vessel. They had forgotten the threads on the pressure vessel were left handed to prevent the plug from unscrewing when the shell was fired from a gun barrel which imparted a right-handed rotation to the shell. Needless to say, I never let either Vine or Douglas forget this incident for many years.

We went to Woods Hole in early July since we had been allotted space on Atlantis in the last two weeks in July to continue our efforts to make seismic measurements on the bottom of the deep sea. Again on the four attempts to use the free-floating gear we did not achieve successful results. We were able to recover all of our equipment showing that the technique for placing the gear on bottom and recovering it worked successfully. The bombs fired properly, but the oscillographs failed to pull the photographic paper through. We were discouraged, but determined to persevere until we succeeded.

Columbus Iselin, the new director of the Woods Hole Oceanographic Institution, took pity on us and gave us an additional opportunity to go to sea on Atlantis for the last week of August and the first week of September. We jumped at the chance. On that cruise, again we were not successful until the last lowering when one oscillograph pulled the photo paper through and we got one record of the four shots. This happened because of a mistake that I made. It was my job to test each oscillograph for proper operation just before the start of the deployment. In each case I used a used set of batteries that we had for some time for these tests. After each successful test I removed the batteries and put brand new ones in each instrument. The mistake I made, was that I forgot to change batteries in one instrument, and that one worked!

The new replacement batteries were Burgess, while my batteries for testing were Eveready. We finally concluded that the Burgess batteries could not deliver as much current as Eveready batteries at the near 0. C temperature at the ocean floor. Tests soon proved that although both kinds could provide ample current at surface temperatures, the Burgess batteries could only provide a lesser current at 0° C. Thus a mistake provided a solution to a major problem for us!

On our return to shore, we now knew how to proceed to get more positive results in the future. Unfortunately, our available time at sea was gone for that year. Next door to the WHOI was a service station, garage. It had been set up in the will of a benefactor as a means of providing for a faithful chauffeur that had served him for a number of years. As a part of the garage a machine shop containing a 10' foot LeBlond Lathe, a large Milwaukee Miller, and large size drill press had been provided. The garage hardly ever used any of these machines in the approximately five or more years of its existence. The water level had risen about twelve feet in Woods Hole during the 1938 hurricane and these machines had stood in about three feet of seawater for a number of hours. Nothing had been done to protect these machines after the water level returned to more normal levels and the residual salt in the machines had caused severe rusting of the mechanisms.

Vine became aware of this history and offered the owner several hundred dollars for these machines. His offer was accepted. We had wanted to get a number of our own machines as those in the staff shop at Lehigh U. were inadequate for some of our work and it took so long to wait for the professional machinist to get to our jobs and to accomplish them. This was, of course, because our jobs had to wait until our turn came up for all the jobs in the Physics Department, and because the machinist only worked eight hours a day five days a week while we could work twelve or more hours a day seven days a week to get our jobs done. Vine of course had to persuade Dr. Ewing that this expenditure would save money for the project and get the work done more quickly, which he did.

On our return from sea, Vine and I removed all of the delicate parts of these machines, such as gear boxes and lead screws and packed them into Floosey Belle to transport them back to Lehigh. When Floosey Belle was loaded, her springs were depressed so that the rubber bumpers rested on the axle housing. I guess I should say overloaded. As the lead screw for the 10' LeBlond lathe was placed on top extending into the front passenger seat, Vine said there that's done and slammed the back door. There was a tinkle of glass and about 1" of the lead screw stuck out through a small hole in the "shatterproof' glass.

Vine arranged for a dump truck to load the major parts of these machine tools and start for Lehigh. Meanwhile Vine and I drove Floosey Belle and our load carefully to Lehigh. I say carefully, but I probably should say super carefully. Floosey Belle was so overloaded if we hit a bump at any speed, we could expect the axle's housing housing to break and we would be stranded. So, we had to drive slowly, coming to almost a complete stop to negotiate any bumps in the roads. The roads at that time, having suffered through the depression too, had an extremely large supply of bumps for us to negotiate.

1940 and World War 2

Ewing stayed on in Woods Hole as he had business to conduct with Columbus Iselin. He kept my Ford roadster, which he was to use when he returned to Lehigh later. The dump truck arrived at Lehigh about a day after we did and we had the machines placed in our laboratory. They nearly completely filled our lab leaving only a small amount of room to get around them. Vine and I set to work taking all of the lower part of the machines apart, cleaning all of the rust off all the parts, oiling and greasing them and reassembling them. We had nearly completed this work about a week later when Vine received a call from Dr. Ewing telling us that there was going to be a program of research instituted for the looming war effort and that WHOI was going to participate. He had obtained a leave of absence from Lehigh University and he was planning to stay at WHOI and participate since he felt that the U.S. would inevitably be involved in the War. He further stated that Columbus Iselin assured him that Vine and I could participate if we wished.

We discussed this proposal at length. Vine had essentially finished his course work for his Ph.D. and had planned to sit for his orals in the coming school year. I had the tuition scholarship and acceptance in graduate school at Lehigh starting in about a week. We decided that we would join Ewing at WHOI. Vine called him and told him of our decision, and he told us to pack up all our equipment and return to WHOI at once. We finished putting the machine tools together in another day and hired a dump truck to take them and the other bulky items from our lab to WHOI.

The smaller and more delicate items and our personal belongings, we packed in Floosey Belle. This took another two days. We worried that we would have trouble getting our shell pressure vessel out of the floor since we had cemented it in. We placed a tripod over it and with a chain fall hoisted it. Because we had coated it with tar to prevent corrosion, we had no trouble lifting it, the tar actually acting as a lubricant. When we had it above the level of the floor, we covered the hole with a piece of 2" by 10" wood and started lowering. Then we got into trouble. As the shell started to lay on its side, a horizontal force was applied to our tripod. We hadn't planned on that. If we continued to lower, the shell and tripod would crash to the floor. We solved that problem by adding some ropes from the top of the tripod to some of the plumbing nearby to hold the top of the tripod. Then we started to lower again. As we were at the most crucial point, when we did not know whether we would have a disaster or not, Professor Bailey came in and asked Vine if he had seen the wastebasket in the student laboratory, since he could not find it. We stopped in this precarious position long enough for Vine to convince Prof Bailey he had not seen the wastebasket and didn't know where it was. After he left, we looked at each other, laughed, and returned to our delicate task. Fortunately everything held together and we finally got the shell on its side on the floor. We didn't even rip out the plumbing which would have caused a flood.

The shell was loaded on the dump truck with the machine tools. While we were packing Vine was called to President Williams office. He was told that any items in our equipment owned by Lehigh University would have to be left behind. This did not make any sense, since with no one having any interest in them but us, they would only be dissipated and lost. Vine tried to argue this to President Williams. He was told that Lehigh University did not loan equipment to eleomosynary institutions as WHOI, the Boy Scouts, Girl Scouts or the U.S. Government. We unloaded all of the specialized equipment for our work belonging to Lehigh University and left it all in the Lab. We still felt that we would have kept it intact through the war years.

Hersey became a grad student after we left and he never found any of the equipment we left behind, and we never saw any of it again. As we approached Woods Hole, traveling through Cape Cod, Vine and I talked about the future and what it might hold for us. We looked at the flat land, the stunted trees and the brush and felt we would find the change from the mountains and valleys and tall trees of Pennsylvania wasn't altogether advantageous.

When the dump truck arrived with our equipment at WHOI we had the machine tools moved into a basement room of the only building at the Oceanographic as we were soon to call it. It was of course ironic that these tools had made the trip to Lehigh University and back to be moved just next door to their original location. Thus the first machine shop at the Woods Hole Oceanographic Institution (WHOI) was established.

Ewing and Columbus spent the first week or two looking over data of sound transmissions which had been made between the U.S. Semmes (a destroyer) and various vessels in the southern North Atlantic, Caribbean and the Pacific Ocean. Temperature and salinity data had also been acquired to a hundred or more feet of depth. They became convinced that the results showed that the refraction effects of the variations within the water column would explain the disparite results. . So it was decided to develop the physics of the sounds transmitted nearly horizontally near the surface of the ocean and the oceanography that caused the variation within the water column. It was thought these would be an important guide to the use of echo ranging in submarine detection.

By the time this discussion was made, Vine and I had set up the machines in the basement shop and made them operational. Ewing's and Iselin's study had convinced them that the temperature effects in the ocean near the surface was the major factor of concern. In the previous couple of years Athelston Spilhaus had designed an instrument he had called a bathythermograph to make a measurement of temperature and take water samples which could be analyzed for salinity at various depths. This instrument was lowered on a wire rope to a prescribed depth where it would be held until the temperature mechanism had equilibrated and a sampler attached had taken a water sample. It was then lowered and the measurements and samples were taken at eight or ten depth stations. It was decided that to be useful to the U.S. Navy, the salinity measurement could be dispensed with in most cases and temperature depth measurements would need to be made from ships under way. Vine and Ewing worked on redesigning the bathythermograph (BT) to have sufficient speed of response to accurately measure the temperature as the instrument was in free fall to a depth of 100 fins (600 ft). The instrument was also required to have a pressure measuring element to record the depth.

Part of the time Ewing and Iselin worked on the text of a report on the oceanographic effects and the physics of sound transmission in the top 100 fms. of the Ocean. I was set to work to locate, in the literature, all the temperature depth and salinity measurement, that had been made in all of the ocean by the various cruises of the oceanographic expeditions that had been made by such ships as Fram, Discovery, Meteor and a number of others. These data would be used to try to establish the Atlantic-wide variability of the surficial layers. By mid October I had found all of the data that there was in Iselin's personal library and the library shared by WHOI and the Marine Biological Laboratory which was across the street. Ewing and Iselin had by then laid out the format of the paper and divided the work of its production according to their expertise. Iselin was working on the text of the oceanographic portion and Ewing was working on the text of the physics of the sound transmission.

I was then set to work to make the computations for the figures to accompany Ewing's portion. For instance for one figure I had to calculate 3000 points of sound velocity for a range of temperature from 30° F to 85° F and a range of salinities for a range of 32 to 37 parts per thousand. One evening, after supper, (about 7 p.m.) I sat down to plot the points I had calculated on my Marchand desk calculator. Ewing came by and saw what I was doing and sat down to read the points to me while I did the plotting. We worked on it all night and finally finished the plotting about 6 a.m. the next morning. As we finished Ewing turned to me and said "Joe, if you had not finished plotting these points by yourself by midnight, I would have given you hell for taking so long". There was a similar graph for water density to plot and a number of other figures.

Then I was set to calculate the ray paths for the sound for the water conditions of each of the sound transmission runs (with the sound intensity measurements) of all of the various Semmes transmission experiments. I also had to plot the ray paths for all of the examples that Ewing had used in his text. There were 27 figures in all with about four of them requiring such extensive work as described above. At this time we'd usually get to work about 8 a.m. and work until 10 or 12 p.m. with an hour off for lunch and an hour for dinner. There were no restaurants open in Woods Hole then, so we had to prepare our meals ourselves in Al Woodcock's house which he made available for us. He was a bachelor and had a six room house. At that time he was spending a lot of time at sea so we often had the house to ourselves.

As the fall turned chilly I decided that I had better drain the radiator of my 1936 Ford, thoroughly cleanse it with radiator cleaner and refill it with Prestone for the winter. Thinking to be helpful, I asked Dr. Ewing if he wanted me to winterize Floosey Belle for him since it would not be much additional work doing two at once. I also knew that he would probably let it go to the last minute otherwise. He gratefully agreed.

I spent about two hours on a Saturday afternoon doing the deed. On the following Monday Dr. Ewing had trouble with his car. There had been a blinding flash of light when he started it and then nothing. It was completely dead. He called the garage and had them tow it away to fix it. He told me that I had ruined his car. I declared that draining and cleaning the radiator and filling with antifreeze had nothing whatever to do with the electrical system. Nevertheless, he still claimed it was my fault.

When he got the car back he was told that one of the brushes in the generator had gotten totally used up and the brush holder pushed by the spring shorted to the commutator and completely wrecking the generator and burning through one of the ignition wires. The generator and the wire had to be replaced. Ewing still insisted it was my fault. Knowing that it was his own fault because he never gave his cars any maintenance, driving them until they failed and then having the minimum repairs made, I made up my mind that I would never again work on Dr. Ewing's car. Only once, thirty one years later, was that vow broken. Ewing had to catch a plane in fifteen minutes in Galveston, there was serious trouble with his car lights, and his wife had to make a trip that night. I fixed them enough to help her out and told her to get it properly fixed the next day.

Ewing & Vine would work on the design of the BT a few hours, then Ewing would check on my progress and work on the physics text a few hours, usually generating additional figures for me to calculate and plot, while Allyn was drawing the design of the part of the BT he and Ewing had planned. At least daily Ewing and Iselin got together and discussed their progress and any ideas either had had that might be included in the other's manuscript.

By the end of October I started to make fair copies of all the figures I had plotted suitable for reproduction and the text was being typed by Iselin's secretary. Ewing and I proofread all of the text and figures for the section we had worked on and Iselin proofread his section.

About the beginning of November Vine and Ewing had finished the design of the BT and acquired the commercially produced parts needed to make the first ten instruments and I was set to work in the machine shop to construct them. Vine showed me how to lay out the parts accurately and supervised my work as I progressed. By mid December we completed the first ten instruments and Vine and I put our heads together to build a winch suitable to lower them. We decided to use a constant speed 110V D.C. motor, since ships at that time normally had such power sources available. On the shaft of the motor we built a V belt pulley that was split so that one side could spread apart from the other allowing the V belt to slide directly on the shaft of the motor. The Vbelt drove a winch drum which contained 1000 ft. of 3/32" stainless steel aircraft cable. We had to work long hours to complete our winch before the New Year because I was slated to go on a cruise to Georges Banks on Atlantis to test out the BT's.

The cruise to Georges Banks was for a biological sampling mission with Dean Bumpus in charge. The two operations worked smoothly together as

I needed to make observations while the ship was under way, and the biological program required the ship to stop for a couple of hours on prescribed stations. Although Bumpus was senior to me, I was made chief scientist, at age 21, because our work had priority.

On January 1, 1941 the N.D.R.C. (National Defense Research Committee) was established and WHOI received the first (war) contract of Division 6 (underwater sound). The manual, Sound Transmission in Sea Water, had been completed and the first copies delivered before then. The first ten BT's had been completed and a prototype BT winch had been completed before we had a contract. Neither Ewing, Vine or I had received any pay since September and we shared what money we had to buy food until our money ran out. We had not been able to pay Woodcock the rent on his house we had agreed on since October. Columbus Iselin managed to get a grant of $1,000 in November to help us out until there was an N.D.R.C. At the end of January we received our first pay from our N.D.R.C. contract. Mine was to be $1,500 per year.

At the end of January, on receipt of his first salary check, Allyn called Adelaide and proposed marriage. She accepted and soon joined our group at WHOI.

It is interesting to note that Sound Channel transmission of sound was described in the manual called "Sound Transmission in Sea Water" and became a very important project itself later in the war. This will be described later as I played a part in that project. It is also interesting that this manual, written before there was an N.D.R.C. and distributed as the first act of this first contract of Division 6 (Underwater Sound) of N.D.R.C., became the U.S. Navy's Underwater Sound Manual until finally superseded fifteen or twenty years after the conclusion of WWII.

On the cruise to Georges Banks I made a large number of BT lowerings while the ship was underway at full speed , 7 knots. On my second lowering I lost the BT, the only one I ever lost in the thousands of BT lowerings I made during WWII. It happened this way. I had lowered the BT once. When I recovered it, it was hanging about 3 feet below the outboard block (nautical for pulley). I released the backstay so I could rotate the boom forward to the rail where I could reach the BT. Because the winch was mounted forward of the boom, as the boom moved forward the BT was lowered somewhat more. When it was by the rail, the BT was about 6 ft. below the boom and out of reach. I brought in about 3 ft. more wire rope and recovered the BT. On the second lowering I put over the BT at the rail and returned the boom to its outboard position. Since I had forgotten the change in the wire rope length, by this maneuver the BT wire jammed in the outboard block. Before I could secure the backstay and reach the winch, the wire rope fatigued from the BT swinging back and forth due to the ship's rolling and pitching and the BT went to the bottom for keeps. From that unplanned experiment we learned to fasten the base of the boom just above the winch,a little higher than the rail, and the stays to the rail fore and aft. With this arrangement these attachments acted as a hinge and the boom could be raised until the boom could be swung to the raill and the BT could be recovered.

Our BT winch also had some lessons to give us. With the motor running, even with the V belt pulley pulled apart, when paying out the wire rope the V belt didn't slip well on the motor shaft that was rotating in the opposite direction. As a consequence there was a braking action on the paying out of the wire rope, slowing down the free fall of the BT in the water. Faced with this problem, I realized that the motor shaft was steel and the V belt was rubberized. The outboard bearing on some ships were steel rotating in wet rubber making a low friction bearing called a Cutlass Bearing. So on each lowering I would dip up a pail of seawater and douse the motor shaft and V belt in seawater to make it slip better. Captain MacMurray, the ship's Captain said this was the funniest thing he had seen a scientist do yet, i.e. throw a bucket of seawater over the winch before each use. It worked though. Soon after our return from the cruise we placed a ball bearing on the motor shaft so that when the Vpulley was separated the belt was running on a fairly frictionless ball bearing. This design lasted a couple of years until a cheaper design for a suitable clutch was developed.

The winch was operated by turning on the electric motor, by moving the operating lever to its center position the wire rope paid out freely. When sufficient time for the BT to reach the desired depth was reached, the lever was pulled forward which braked the winch drum stopping the wire pay out. By quickly moving the operating lever all the way away the clutch was operated and the wire rope and the BT were recovered. This winch was used for other instruments in WWII such as an underway bottom sampler, and to lower underwater cameras. BT's were lowered from ships making 20 knots to depths of 100 fms (600 ft.) with wire ropes of no more than 1000 ft. in length throughout WWII and for about 15 years afterwards.

At the completion of this first cruise using a BT I spent about a week reading the temperature depth points off the smoked slide record made in the BT. These were compared to temperatures read from water bottles

(Nausea bottles) some of which were observed at biological stations by Bumpus. Columbus was satisfied with the accuracy of the BT measurements.

Since each BT could not be readily made absolutely identical, it was necessary to build a calibration chamber to produce a temperature depth template, for each BT, on which to insert the BT slide to read off the pertinent data. I was set to work to build this chamber. It consisted of a cylinder about six inches in diameter enclosed by another cylinder about 10 inches in diameter. Water of uniform temperature was circulated in the annulus thus constructed. A small propeller in the central chamber driven by an external motor assured the water surrounding the BT was well mixed. A standard thermometer inserted in a well on the top cover was used to determine the temperature of the contents of the inner cylinder. To change the pressure of the inner cylinder, oil was pumped from a hand hydraulic pump into a small cylinder used as a separator. From the top of the separator a copper tube allowed water to flow into the calibration chamber as the pressure was raised. A pressure gauge in the line allowed the pressure to be determined.

The chamber was operated by introducing a BT into the chamber and closing the top. When the temperature stabilized the pressure was increased to that of seawater at 600 ft. and then reduced back to 0. Then another temperature would be established in the outer chamber by introducing warm or cold water from a nearby sink and the process repeated a number of times. These temperature points were established on one axis of the slide, the pressure points on a second axis, at right angles. We soon determined that the pressure scale was linear so that it was only necessary to record the 0 and maximum pressure at each temperature to calibrate the depth scale. The temperature was slightly non linear so that each instrument had to have a number of temperature points observed. With this data a temperature and depth grid was drawn, photographed and glued into a slide holder that was a duplicate of the one in the BT. A magnifying glass was mounted so that the BT slide could be superimposed on the calibration grid and the temperature and depth established for each BT lowering.

When I first started BT calibrations with this equipment, we found the lines that were supposed to be isothermal were not duplicated between increasing the pressure and decreasing the pressure. Since this did not make sense I tried everything I could think of to determine the cause. Finally I turned to Dr. Ewing for help and for a week we tried everything and could not discover the cause. Dr. Ewing was convinced that I was too impatient and did not wait enough time for the temperature to equilibrate. I was sure that the temperature was equilibrated. As the week went on the debate and discussion became more and more acrimonious

Finally I waited an hour after each temperature change for the temperature to equilibrate. Still the problem remained. Late one night, in despair I was just sitting and looking at the apparatus when it dawned on me what was causing the problem. When we were increasing the pressure, we were pumping water, at room temperature in the separater, into the pressure vessel which would cause a small change in the temperature. When lowering the pressure we removed liquid from the pressure chamber thus not disturbing the temperature. Once the cause of the problem was established the solution was simple. We raised the pressure while changing the temperature of the chamber. When the temperature equilibrated we would lower the pressure establishing the isotherm and repeat the process. We soon established that the temperature equilibrated in the chamber five minutes after the changed temperature was reached, and the calibration of an instrument required only a half to three quarters of an hour. Additional people were being added to the staff and shop people were soon building BT's. One of them took the calibration of the BT's and I was moved to other work.

In this period Vine ordered large quantities of materials needed to build BT's, office supplies, graph paper in sheets and rolls, and many other things that were needed to carry out our work and to expand it into other areas. It was a time when lots of tasks were allocated and we got a large share. The result was that we started to receive shipments nearly every day. Vine and I enjoyed opening these deliveries and allocating things appropriately. It was sort of like having Christmas every day after the years of making do with what we had, if possible, and only buying supplies that were absolutely essential.

We received one shipment of a dozen bottles of Harlem Oil. Neither Vine nor I knew what it was, what it was used for or who ordered it. We put it aside assuming someone would start asking about it before long. Shortly afterwards Atlantis came in and docked. When Capt. MacMurray came by, Vine asked him if he had ordered Harlem Oil. He said yes. Vine then asked him what it was used for. Capt. MacMuray said "Saving lives", took his carton and left.

Later in the day Capt. MacMurray stopped by and explained it was stored in the lifeboat so that if survivors had to leave the ship in the lifeboat they could coat themselves in "Harlem Oil" and prevent serious problems from sunburn. Thus our education was enhanced.

In early March A.P. Crary was persuaded to come to Woods Hole to join us in the "war research". He had been a graduate student of Dr. Ewing at Lehigh, leaving with a Master's Degree in 1935. He and Dr. Ewing had had some wonderful adventures which "Doc" loved to tell about, often with a few embellishments. For instance, on the first refraction measurements at sea, on the continental shelf off the east coast, Crary was sent out in the whaleboat to fire dynamite charges for sound sources. The Captain was concerned that Crary, who had never gone to sea before, would become so seasick that it would be dangerous for him to handle explosives. After the first day when they returned to the Atlantis, the two sailors operating the whaleboat were just barely able to do their job while Crary was fine, and chewing a wad of tobacco when they were picked up.

Another time, after working on the equipment for several days without sleep, they started for Norfolk, Virginia, each driving a car. Crary carrying the dynamite was in the lead. He got into a small accident and was stopped on the side of the road. When Ewing came along he saw the accident, but being sleepy he drove on. About five miles later it occurred to him that it looked like Crary's car in the accident. He turned around and went back and talked Crary free and they went on to Virginia and refraction measurements on the land end of one of the continental shelf refraction lines.

On another occasion, they were working on land in Virginia on a very hot day. Crary returned to the recording truck where Ewing was developing the film of the shot which had just been fired. Hot and tired Crary hoisted the gallon jug that contained the hypo, saying I just can't wait for a drink, Ewing said he was just about to tell him to hold it when he took his first mouthful. Crary said Ewing just sat there and watched him. Anyway, Crary blew the acid hypo out of his mouth as an explosive jet.

The instruments they were using on the early trials of seismic refraction measurements on the continental shelf were not too sensitive. Captain MacMurray of Atlantis had ruled that no more than two sticks of dynamite (1 lb) could be fired at a time from the whaleboat. Ewing decided that a larger charge was needed so when the whaleboat came within hailing distance the next time he leaned over the rail of Atlantis and shouted "two more sticks". At the same time he made motions of his hands outside the rail holding out all ten fingers. Crary got the message and fired ten sticks, with no harm to the whaleboat and good results on Ewing's recording gear.

On another occasion, a steam shovel had been lost in a coal mine when the explosive charge dislodged more coal than expected and the steam shovel was completely buried. The quarry owned only the one steam shovel and had no idea where it was buried and wanted to remove the least coal possible by hand to recover their equipment. Ewing and Crary had agreed to try to locate the buried shovel magnetically on a Saturday. Crary had gone to a formal party on Friday night in a tuxedo. Somehow he got involved and finally left the party in time to join Ewing at 6 A.M. still in his tuxedo. They went out to the coal mine and ran traverses across the coal rubble, located the boom of the steam shovel using a dip needle. When the miners started to dig they found the top of the boom just two feet beneath the top of the rubble, at that spot. This is probably the only time a member of a geophysical field party worked in a tuxedo.

Crary arrived in Woods Hole about 3 P.M. That evening he ate supper with our group in Woodcock's house. He and Doc and I drank until midnight with Crary and Ewing taking turns recalling interesting adventures. The next morning, when I cleaned up I counted twenty empty bottles that had varying amounts of liquor in them before the previous evening. The next day was a slow day.

A new project was started. Dr. King of Bell Labs had had the idea that using explosive charges as sound sources, reflections from ships could be detected at greater ranges than the conventional echo ranging gear in use in 1941. Woods Hole had been chosen to test out this idea, cooperating with Dr. King. Crary was put in charge of the work at WHOI and I was assigned to assist him. The usual experiment would be set up using two yachts, borrowed for the occasion, one as a shooting vessel and the other as the sound receiving vessel and Atlantis as the reflection target. Part of the concept was that there would be a single shooting vessel and a number of receiving vessels to protect convoys of merchant vessels. This meant that echoes would have to be obtained at a number of different angles. Hence our experiments would involve having the shooting vessel and receiving vessel assume a lot of different angles relative to the target.

Dr. King usually brought an electronics expert from Bell Labs with him. They would design and build amplifiers that had various characteristics and bring them along to test alongside our usual geophysical type amplifiers which we had matched with hydrophones that could be lowered into the water at various depths. Unfortunately, Dr. King was not a very good sailor and would remain seasick all the time we were carrying out experiments. After four or five days of this, he would suddenly decide he had to return to Bell Labs, usually before we had completed any of the experiments planned for that cruise. We would return to Woods Hole, Dr. King would go back to Bell Labs and we would set up another set of experiments for about a month later. Crary and I would work on the records, trying to get as much as possible from the uncompleted experiments, and readying our equipment for the next attempt.

In the late spring 1941, Woodcock asked us to make other arrangements for housing since it had become too crowded. Crary, and a couple of others had moved in and only Vine had moved out. The rectory of the Methodist Church, a house of about fifteen rooms, was empty as the single rector had found more suitable lodging for himself. We persuaded the Church fathers to rent the rectory to Ewing, Worzel, Crary and McCurdy. This became a boarding house for about fifteen single men at a time working at WHOI for the duration of the war. The personnel was always changing as people went to sea or on other assignments and other people were added to the staff. Meals were served at night, each person in residence taking a turn at fixing the meal. This lead to some very strange "concoctions". Most of the members were drinkers and some merry times ensued occasionally, not too often though, because everyone was very busy. It soon became known that if you wanted a drink, day or night, the rectory was the place to go. In the early part of the war there were no bars open in the winter in Woods Hole. Later on the Rendezvous was opened, across the street from WHOI, and it took up this duty.

Throughout WWII, about once a year, the Rectory as it was known, would hold a party. It would start about 5 P.M. on Friday and go on continuously until about 5 P.M. on Sunday. People would stop in, stay a while and join the fun, and then would leave only to be replaced by others. Everyone hated having sea duty when a Rectory party was to be held. Fortunately the Rectory was well separated from the Church and other neighbors so the noise of these parties disturbed no one. People living at the Rectory when they became tired would retire to their room, have a sleep and then rejoin the party. Everybody working at WHOI would stop in sometime on the weekend. In June Crary went out to sea to test some of the recording gear, assigning me some work in the lab. After he had been at sea a couple of days on a Friday afternoon, a telegram was phoned to me in Crary's office. It was from his sisters, Marion and Dorothy saying they were arriving, to spend a week in Woods Hole with him, on the New Bedford steamer late on the following Sunday afternoon. Crary was not expected to return until the following Tuesday afternoon. I met his sisters at the steamer dock, brought them to the Rectory. Knowing that they did not have very much money, I established them in Crary's room in the Rectory. When he returned on Tuesday he arranged a room for them next door for the remainder of the week. The younger sister, Dorothy and I became quite well acquainted in that week. We hit it off well. We made a couple of cruises in the summer. All were inconclusive because Dr. King would decide he had to return to New York before any of the experiments were completed. We were becoming frustrated. Towards the end of the summer a cruise was planned and Dr. Ewing planned to go along. I was to go on the Blue Dolphin, about a 90 ft. schooner to do the shooting. The Atlantis was the target, and Ewing was to be on another ship with a set of recording gear and Crary on the Asterias also with recording gear. Dr. King was to be on the Blue Dolphin. Before departing Ewing and Crary got me aside and told me, I was not only to do the shooting but to make sure that Dr. King remained seasick all the time so that he could not interfere with the planned operations. I went across the street and bought a box of cigars.

When we got to sea, whenever Dr. King appeared on deck, I would get a cigar out, light it and get upwind of Dr. King. In short order, he would retire to his bunk. After three days Dr. King insisted he had to go back to New York. So I was shifted to the Asterias with all the explosives, Crary moved on to the ship with Ewing, taking his recording gear with him. Dr. King went back to Woods Hole on the Blue Dolphin. We stayed out two weeks and completed a set of experiments for the first time. The experiments were carried out more than 100 miles offshore where the water depth exceeded 2000 fms. to avoid any complicating factors from bottom echoes. This was quite a trip for the 40 ft. long Asterias. We were stretching her fuel and food capacity and size. At the end of the experiment we all returned to Woods Hole independently since we all had different speed capabilities.

Stan Poole was the Captain of the Asterias. He had three hands to help operate the vessel and myself to handle the explosives. This was the fall of the year, and swordfish season off Cape Cod. Stan had brought his swordfish harpoon and despite the fact the Asterias had no pulpit he was determined to try to get a swordfish on our way in. In the daylight hours, he and I would stand on the cross trees of the mast about 30 ft. above the water looking for a swordfish fin while the others manned the helm. Finally Stan spotted a swordfish fin and talked the helmsman into its vicinity. Then he jumped down from the mast and picked up his harpoon and moved to the bow of the vessel. From there he guided the helmsman close to the swordfish. Luckily the fish did not veer off and Stan was able to harpoon him. Soon the six thread line (rope about 5/16 inch in diameter) was sizzling over the side as the fish left our vicinity. Stan had a tub of six thread line. He rushed to the stern and had two hands and myself help him launch the Dory and then he and I and the tub of line quickly got into the Dory. As soon as we were safely on the Dory, he started snubbing the line. After awhile, he was able to stop the line and the fish towed us in the Dory awhile. Then he was able to recover some of the line and warned me to get up on the thwarts (seats) because swordfish had been known to charge the boat and stick its sword through the bottom. It was to avoid any possible damage to Asterias that we had transferred to the Dory. After the fish took out more line several times and Stan had recovered it several times, and we had been towed about some with Asterias following a couple of hundred feet away, the fish tired and Stan was able to get him to the surface. He then got the Asterias to come alongside and he got a line around the fish's tail. We then transferred back to the Asterias. We turned the boom over the side and hoisted the fish on board.

When we returned to Woods Hole, Stan put in to the fish market (Cahoon's) and sold the fish for a couple of hundred dollars. Although he had done everything in catching the fish, he insisted in dividing the money five ways, as he said that was the way all the fishermen did it. I got together with the other three and we agreed that we had done almost nothing to help catch the fish and Stan had done everything. We tried to give him the money, but he would not take it, so we bought him a very expensive home entertainment center, a standup radio phonograph with the money we had received and presented it to him. He accepted that. Stan became a very good fhend of mine and taught me almost all I know of ship handling, splicing, handling heavy equipment with marginal gear and all sorts of nautical lore.

On one occasion a 40 ft. fishing boat had been hired on which I was deployed to shoot explosives. It was hard to see how the vessel functioned. Jury rigged repairs had been made to the engine and the electric wiring was draped around largely not fastened in place with spaces of bare wire showing when insulation had departed this earth. The general appearance of the vessel can only be described as dilapidated. On our return to Woods Hole, a fog had descended on us. I had had little small boat experience at the time so I was dependent on the owner and his helper's skill in navigating us up Vineyard Sound and into Woods Hole

Harbor. That is, until we had to abruptly change course to miss the channel marker for Woods Hole which was on the Buzzards Bay side! Apparently, we had lucked through the Broadway Channel before almost colliding with the main channel marker. We had survived the large boulders which caused the devil's cauldron with the current running as high as 5 knots towards Buzzards Bay at the time of our passage. This unhinged the owner so he quickly anchored in the embayment where the channel widens out and turns towards Buzzards Bay. The tide tables indicated we had made this dangerous passage in the fog at the height of the current from Woods Hole Harbor and Vineyard Sound into Buzzards Bay. The three of us gathered around and studied the chart. We were all amazed that we had survived this dangerous passage. As slack current approached I tried to get the owner to up anchor and proceed to the WHOI dock. He would have no part of going through the Woods Hole Channel again in the fog. I argued since we knew where we were, and that our dock was only about a half mile away, that at slack water we could make the passage easily. The owner was not having any of it. Finally I argued that when the current turned there was no assurance our anchor would hold and that we risked another passage across the dangerous rocks and narrow channels at the whim of the reversed current where we were anchored. The entrance to Buzzards Bay was wide and the currents never exceeded about 2 knots in that section, so I finally convinced him to raise his anchor and move out into Buzzards Bay where currents could not be expected to exceed about 1/2 knot. He finally agreed and at slack water we moved into Buzzards Bay and anchored again well clear of the Woods Hole Channel entrance. We spent the night there, a little hungry as we had carried only sandwiches for lunch as we expected to return to WHOI well before dark. When daylight came, the fog had dissipated and we easily returned to the WHOI dock. We never used that vessel in our work again. In retrospect, this was the poorest vessel and the poorest equipped vessel I ever went to sea on in my nearly fifty years of seafaring.

One thing we discovered in this work was the "bubble pulse". That is, the recordings did not show a single pulse from an explosion underwater but several. This puzzled us and we had long discussions about what was causing the effect. One possibility was that there was not complete combustion of the explosives and some of the gaseous products would make an additional explosion. Our instruments showed that sometimes the second pressure wave seemed to be greater than the first. To test the "second" explosion hypothesis, I made a pressure chamber about 4" inside clothes and went to work on Monday morning, again with no sleep. I slept well that night!

About a week later I wrote a letter to Dorothy and asked her to marry me, offering to ask her on bended knee next time I saw her if she chose. She answered that she had thought that was settled and that we only had to set the date! When I wrote my parents I told them my plans, my mother insisted I invite Dorothy to Staten Island to introduce her to parents and to buy her an engagement ring. Mother had an arrangement to buy jewelry from a New York dealer. When we visited this merchant, mother went along and insisted that I buy a bigger and more expensive ring than Dorothy and I would have otherwise. At the time it seemed a lot of money, but neither of us has regretted it in the intervening years. We set the date for the wedding as 22 November 1941 at Dorothy's parents' farm in Canton, N.Y. The wedding and reception was held in the parlor and dining room of their Farm House. All of her close relatives were there and my parents and Doris and Ralph came to support me. Ralph was my best man and Marion Dorothy's siste was matron of honor, I had been the best man at Ralph's wedding to my sister, Doris, at Lake Mohawk a couple of years earlier. Dorothy was matron of honor and I was best man at Marion's wedding to Wayne Flagg in Woods Hole shortly after the end of WWII.

Before my wedding day Dad Crary took me aside and told me that I didn't have to go through with it if I wasn't sure. I told him I was sure. After the reception Dorothy and I got in my car and discovered that Marion and Albert had decorated my car to make sure everyone knew that we were newlyweds. They had just paved the road that passed the Crary farm for the first time just before the wedding and the shoulders were quite soft. About a mile down the road I pulled off the road to undecorate our car as we felt the notoriety was undeserved. I nearly got stuck in the soft shoulder, after clearing the car, but fortunately made it out. Of course, the car had a good coat of mud by that time. We went to Niagara Falls and spent a couple of days there. We had always heard of newlyweds going there on their honeymoon, but neither of us knew anyone that had done so, therefore we determined we would do it. Later we spent a couple of days with my parents at their home on Staten Island and then made our way to our new home in Woods Hole. The wedding came close to not happening. Just beforehand, I was driving through the Adirondack Mountains, on my way to Canton. I came over the top of a mountain making 70 MPH, touched my brakes to slow down to find I didn't have any. I was going too fast to downshift to slow down so all I could do was steer down the steep slope I was descending. It was a one lane road in each direction and I used every inch of roadway on both sides descending the steep slope. I don't know what speed the car got up to on that slope, I was too busy trying to steer to keep on the roadway. Fortunately, no one was coming up the other side so I could use the full width of both lanes on the curves. When I got stopped at the bottom I took a deep breath, got out of the car and took a walk to calm myself. I got the brakes fixed and told no one of my experience until many years later.

While on our way back to Woods Hole, I got stopped for going too fast in Providence. The police officer gave me a stiff lecture and asked me if I thought I should speed since my mother was in the car with me. Dorothy claims this never happened but that the police officer let me go since he took pity on my pretty bride that was sitting next to me. In the intervening fifty four years, we have never been able to agree on the details of this event.

In early December we set up housekeeping in a house on Glendon Road that I had rented. We had some pots and pans, some dishes, a kitchen table and two chairs, an outdoor chaise lounge and a double bed for furnishings in our two story seven room house. Within a year, Dorothy changed that, even though it was still sparsely furnished. On the Sunday, following our return, I was working in the lab in the middle of the afternoon when someone came in and told me that the Japanese had bombed Pearl Harbor. I went into Ewing's office next door where Ewing and Vine were working and told them. We got a portable radio and listened to the broadcasts of the news. We discussed the event at length between us and wondered how the Japanese could have made such a serious mistake. After about an hour, we decided wed better get back to work that what we were doing had now become even more important to pursue as fast as we could.

If this had happened about two weeks earlier, we probably would have postponed the wedding because of the big upheaval in everybody's life. I often wonder if that had happened, whether we would have married when things settled down a little. Just before our wedding it was decided that the explosives echo ranging didn't show much promise so it was dropped. I was assigned to a professor of Queens University to investigate methods to increase the range of photography in the ocean. The professor would come to Woods Hole, outline a number of tests for me to complete and return to his teaching. On his next visit, he would review the results of my work, set up another series of tests and leave me to it again.

In the basement of WHOI there were tanks about three feet deep lining the walls of the room except for the doorway entrance. These had been used for various biological work in the ten year life of WHOI before the start of WWII. We first made a port about halfway between the bottom and the water surface. This was fixed in concrete and was of plate glass about a foot in diameter. This port had to be cleaned before each day's use as biological material would start to grow on it. Water was pumped into the tank from Woods Hole Harbor and circulation of the water from the harbor would be maintained to keep the water from stagnating I had to make a wooden target with lines of black of various widths and several geometric shapes on a white background. This target was placed at various distances from the port and with a camera on a stand just outside the port pictures would be made. Pictures were taken at various target distances, using all of the available 35 mm film types including infrared film and various developing systems to extend the distance at which photos could be taken in seawater. The transparency of the harbor water varied somewhat so a control had to be found.

Someone had been measuring the transparency of seawater as a measure of biological life in the water and had made a transparency meter. We made a copy of the instrument to keep track of the quality of the transparency of the water used in our tests. It consisted of a small light bulb at one end, a photo cell a meter away at the other end, with circular diaphragms along the length which kept almost all the surrounding scattered light from entering the instrument light path. This work went on for about six months, but did not keep me occupied full ime. I was also sent to various harbors between New York and Boston to make Seiche Disk readings during daylight and transparency meter readings both in daylight and in dark. The Seiche Disk was a white disk 20 cm. in diameter, which was lowered into the ocean and the depth at which it disappeared from sight was recorded. These measurements had been made in many of the world's oceans as a measure of biological life in the near surface waters. It had not yet been established how the Seiche Disk reading compared to the transparency meter readings. It was surprising to find the the Seiche Disk reading was very sensitive. The disappearance would occur in only a few inches of depth change. The visability range was about 1 ft. in Boston Harbor to 600 ft. in the Sargasso Sea, an oceanic desert.

I guess it is not surprising that we learned that the bigger the harbor the less the transparency, since all kinds of dumping of wastes were condoned in all of the harbors of the world at that time. Offshore of the harbors, beyond the harbor mouth, we found transparencies of about 20 ft. along the Northeast Coast. This meant we could take photographs at 10 to 12 ft. in these areas. This shorter photo length resulted from the scattered illumination of the water by necessary lights so that pictures could be taken. This work was supported because the Bureau of Ordnance hoped that photography would help to locate mines in mine fields and allow divers to take pictures to identify details of a mine to make it safer for divers to disarm them.

Soon after we started, magnetic mines became prevalent and we were asked to try to locate them with photography. Vine quickly made a multi-shot non magnetic camera by making all of the parts of brass or aluminum and using a twisted elastic band for power to wind the camera. I was sent on the Anton Dohnn to Chesapeake Bay where at Indian Head, Maryland they had a marine mine operating unit. I tested and retested the camera on the trip to the Chesapeake Bay. The night before the first tests were to be made, we were at anchor near Indian Head. With nothing much to do we all were playing poker. At 9 P.M. I excused myself to make a final test of all the gear to make sure that all was in readiness. I checked the camera only to have it all fly apart. One of the soldered parts had failed and this overstressed a number of others so it all came apart. I was dismayed. The only thing I could do was to get the soldering iron out to resolder all the parts that had separated this amounted to about 10 parts. Then I had to completely disassemble the camera so I could reinstall the repaired pans. By about 1 A.M. I finally got it all put together and tested it and found it worked as well as it had before.

At 6 A.M. a Navy ship joined us and took us to a field of acoustic mines, unarmed of course, and set us to work with our camera. We worked hard all day and found it very discouraging. We had little luck at finding mines by photography. We could take pictures of mines without activating the acoustic mechanism, when we were led to a known mine. I felt the exercise had been a total failure. Nevertheless, we received the best letter of commendation we received throughout WWII for this work. However, they decided to use other methods to deal with magnetic mines.

The tests in the water tank did not turn up any way to improve the distance we could photograph in seawater, although I must have made hundereds of tests and printed thousands of pictures. It was discouraging. Nevertheless the Navy felt that there was some use in photographing the bottom, for divers to take photographs, and to photograph damage to ships so that repairs could realistically be planned, before the ship was raised in dry dock. So we were set to work to make a general purpose underwater camera. It was decided that the new camera should be able to take multiple shots. The only camera motorized to take multiple shots and with a built in synchronizer was a 35 mm camera known as a Robot manufactured in Germany. Some were available in photo stores in the U.S., probably from stock obtained before the war.

This camera took a square negative of 24 X 24 mm. It was small and compact. Any equipment put in the ocean inevitably leaks. These underwater cameras were no exception. The Robots cost over $300 so we could not afford to lose them. When one got wet because of a pebble under the head gasket, I quickly put the camera in freshwater to flush out the saltwater. Afterwards it was dried thoroughly in an oven. The camera worked for a while then the wind up spring failed. It was impossible to get all the saltwater out of it, so it rusted and soon broke. I took the spring out, measured it and bought some spring stock to replace it. Since the original width had been measured in millimeters, it could not be duplicated in English measure. We got the spring manufacturers to "slit" spring material of the right width. After cutting the length desired, the ends had to be annealed so that they could be machined to work with the attachments on the camera. I then had to wind the spring in a suitable coil to fit its space in the camera. It was finally installed and worked properly. I had to do this once more on our cameras during the war years. Inevitably, the Navy had similar problems with the 10 units we had supplied them. They contacted us and asked how to get them repaired. It was decided that we could do it for them. Thus, I had to repair four or five Robot cameras for the Navy. We built a brass case intended to be used in water depths of 100 fms or less. A half inch thick piece of plate glass about 2" in diameter was used to make a porthole in the side of the cylindrical pressure case, Two lids which included the structure to hold the camera were made. One that had terminals so the camera could be fired electrically from an external switch which operated a solenoid. The other which could be operated mechanically to operate the camera by an external trigger. The mechanical head could be used with the camera mounted on a pole to be operated by relieving the strain of a trigger weight, for instance by touching bottom. Alternately, the camera could be operated by a thumb mechanism mounted on the camera head. This was planned for the use of divers. We also made a plate that a diver could hang from around his neck, with straps around his body to restrain it tightly along his front. A mounting, including a handgrip on each side, was made to attach the camera case to this plate on the divers suit. Both heads had a lead in to bring the connection from the synchronized flash terminal on the camera to the flashbulb which was on a separate structure from the camera. This made it possible to mount the flashbulb on the "pogo stick" or for a diver to hold or clamp the flash in a suitable place for illumination of the picture.

A "Kodatron", a newly available commercial electronic flash unit was used for some pictures, but many were made with photoflash bulbs of peanut or sometimes larger design. We took many pictures of the ocean floor, useful to people anchoring mines. We were soon called on to see if we could photograph a German submarine sunk offshore near Morehead City in North Carolina. There were strong currents due to nearby Cape Lookout in the area. The Navy had been trying for a month to locate a submarine salvage vessel over the wreck so they could send divers down to recover papers from the submarine without success. Their problem was that because of submarine activity they did not want to keep the vessel at sea at night. Leaving Morehead City at dawn, travelling to the site, finding the wreck, anchoring with multiple anchors to locate the vessel over the wreck took so much time, that they would have to recover the anchors and return to Morehead City by dark leaving inadequate time for the diving operation.

They hadn't even been able to make sure they were diving on the submarine, or whether it might be some other wreck. When we arrived, a British ship with echo ranging gear was assigned as our protection. We arrived at the wreck location near Morehead City at 10 A.M. We had our British escort move away about a half mile from the wreck, locate it with their echo location gear and keep the ship's bow aimed at the wreck. We then positioned the Anton Dohnn about a mile ahead of the escort, lined up on her masts and moved forward slowly until the wreck showed up on our echo sounder. We placed a buoy about 100 ft. to one side of the wreck as a surface mark for us to maneuver near. With our buoy and our compass we were able to move around over the wreck. When our sounder indicated we were over the wreck, we would quickly lower the camera in the "pogo stick" mode and take a photo. By noon we had about a dozen negatives developed which positively identified the wreck as a German submarine. We printed pictures and delivered them to the submarine salvage vessel and returned to Morehead City. They thanked us and sent us on our way. We later heard that with our buoy as a guide they had been able to anchor and get divers down to the wreck. Whether they obtained the papers they wanted or not, we never found out we had no "need to know"..

With this success the Fifth Naval District, headquartered in New York, requested us to locate and identify the wrecks sunk off the east coast between Montauk Point and Cape Hatteras. The Germans had sunk so many vessels, and they had three or four positions marked on the chart for each sinking, that it had become nearly impossible to sail vessels in the area inside the 30 fm. curve. It was too dangerous to send them into the waters deeper than the 30 fms curve, because of submarine activity. There were a number of positions for each wreck because survivors of the wreck would give a couple of positions, aircraft which had been sent to the area to help locate survivors, would give a different one, and vessels picking up survivors might give another one or two. Partially this was the result of inaccuracy in the navigational methods extant, partially to the confusion associated from the sinking and attempts to recover survivors.

Since wrecks were tall enough to present a danger to surface ship, traffic had to be routed to avoid each wreck position. The 5th N.D. felt that if the wrecks could be located and identified, the false positions could be eliminated, and that a depth drag could ascertain whether anything on the wreck was tall enough to present a danger. If this could be achieved, they could again route coastal shipping into the safer areas. An escort was provided to us and we set to work. From 1942 to 1944 sometimes Ewing and sometimes I was sent as chief scientist and Rusty Tirey and John Ewing, one or the other or both as photographic assistants. In that period we were able to photograph and identify 33 wrecks and to eliminate at least three times that number of "false positions". The escort vessel made depth drags of those locations we identified and was also able to remove about half of the existing wrecks as presenting no danger.

One of the things that amazed us, was that we would lower our tethered camera to a wreck which usually was a jumble of equipment and wire ropes in total disarray and only a couple of times find ourselves entangled in the wreckage. With careful manipulation we always were able to free ourselves and continue.

In this program two other wreck surveys provided noteworthy situations. The first was off Manasquan, N.J. A blimp with a new MAD, a magnetic anomaly detector, was believed to have detected a submarine and sunk it in about fifteen fathoms of water. I was sent in Anton Dohnn to investigate. When we got there we found our escort vessel to protect us from submarine assault was a Coast Guard motorized open lifeboat, about 40 ft. long with a Chief with a pistol as its sole armament. While we searched for and photographed the wreck, they would anchor nearby and so far as we could tell, all on board would go to sleep. We obtained only photographs of a cargo vessel in total disarray and with so much marine growth that it had to have been sunk more than ten years earlier. We could not find any other wreck in the vicinity.

That night I went into a local bar and found out from local fishermen that they had been fishing near that "wreck reef' for years and that it was the wreck of the Mohawk which had been in a collision in the early thirties and had sunk there. I sent the Anton Dohnn back to Woods Hole and I went overland to New York City and found the newspaper accounts of the wreck of the Mohawk, which included a position very close to where we had found the wreck as the place where it had sunk. We reported this and I'm sure the people on the blimp and the people who developed the MAD were not happy with the result.

The second case resulted from a phone call on a Saturday afternoon. Most of the vessels' crews and staff had already left for the weekend. The call was taken by Ewing and relayed to Iselin, at home on Martha's Vineyard. It seems the General Green, a Coast Guard cutter, had detected a "submarine" and depth charged it near Nantucket Shoals. They wanted us to come photograph and identify it. It was decided that we would do it. We found the Captain and Chief Engineer of the Anton Dohnn, who agreed to come immediately. The first mate, cook, and crew could not be found. Ewing decided that he, and John Ewing and' Rusy Tirey and I could substitute for the missing crew and do the photographic work too. Doc became the interim cook, I became the interim first mate and John and Rusty became interim crew. We took off about 3 P.M. from Woods Hole. We arrived near the General Green early Sunday morning. Dr. Ewing went aboard the General Green and learned about the situation and indoctrinated them on our mode of working, On his return, we maneuvered over the "wreck" and took pictures.

They showed a wreck which had been sunk a long time that we identified as the wreck of the Nantucket Shoals Lightship which had been sunk in the early thirties. It seems a trans Atlantic steamer being guided by the underwater bell (lightships had underwater bells as sound sources to help ships when fog conditions prevented their light to serve) had sliced the Lightship in half sinking it. The people on General Green were not happy with this result and decided to drop their remaining ten depth charges on it to make sure, that if it were really a German sub it would never sail again. These were added to the 40 depth charges they had previously showered on the wreck. This last salvo, knocked out their magnetic compass, joining the non functioning gyrocompass that previous depth charging had knocked out. Without any functioning compass, they radioed us and asked if we would lead them back to Woods Hole. Their base was in Little Harbor in Woods Hole. We of course agreed, and set off Sunday night for Woods Hole.

Eddie Atheam, our Captain, had sailed with us despite having a bad case of the flu. He decided, that he couldn't manage any longer and turned over the Captain's chores to me. I had been sailing with him for a number of months and I guess he figured I had learned enough to serve. We made it back to Woods Hole late Monday morning completing another "wreck survey".

There was so little space for people on Anton Dohnn, so that Rusty, John and I had had to serve as sailors on our trips along the East Coast from Portland, Maine to Beaufort, N.C. as well as underwater photographers. On one occasion, Capt. Atheam had decided to go outside from Cape May to the entrance of Chesapeake Bay to save the time necessary to go up the Delaware River to cross the canal, just south of Philadelphia, to the headwaters of Chesapeake Bay, and back down to the Norfolk area. As we neared the mouth of the Chesapeake Bay, we noticed a picket ship trying to get our attention. We finally decided we werre in the middle of a mine field. Not knowing where the mines were, Captain Atheam decided we might as well continue as to turn around and traverse back through the part of the mine field that we had already negotiated. We made it without setting off any mines. It was probably because Anton Dohnn's draft was less than eight feet and she was constructed of wood. If any of the mines were magnetic, our engine was apparently too small a chunk of iron to trigger them.

This was typical. We were asked to move back and forth along the east coast but we could not get charts of "war dangers" because we had no "need to know". Somehow we felt we did have a "need to know" but that changed nothing. In early 1943 the Bureau of Ordnance contacted us and advised that they had developed a device called a hydrofoil. It was a section shaped like an airplane wing. It was designed to be towed by a wire rope, with the "lift" area facing down so that some of the drag on the device from towing would be counteracted by a downward force reducing the wire angle needed to tow it. They wanted us to fit it with a camera so they could take up to twenty pictures on each tow and to demonstrate its capabilities. We were advised towing instructions would be included. We received the equipment but no instructions.

We cut out a central section and mounted the camera behind a plexiglass cover so the water flow was not interrupted. In an adjoining compartment we initially mounted a switch and an arrangement of 20 flashbulbs. Later on we replaced this by a Kodatron electronic flash. When we were ready to try it out we called to get the missing towing instructions, only to find there were none. It had never been towed. We made a wire bridle with a towing fixture that could be adjusted back and forth on the bridle on each side. After some experiences such as broaching too or heading rapidly for the bottom, we found a towing arrangement that would keep the wire angle at 45° and the hydrofoil would tow stably. We soon found out that even though the wire angle was quite stable, and our sounder gave us accurate depth information it was nearly impossible to tow the device at a known distance from the bottom so the pictures could be properly focussed. I built a small hydrophone from four Rochelle salt crystals and we mounted it alongside the camera. By using the sound from our sounder that went directly to the hydrofoil and its reflection from the bottom, and the wire angle, we were able to determine the distance from the bottom accurately. Displaying the signals on an oscilloscope, adjusted to an appropriate sweep spread, we could make 0.1 inch on the oscillograph screen represent 1 foot off bottom. The operator could then advise the winch man to pull in or let out wire rope until the proper distance from bottom could be achieved. We towed it at about four knots and found the water depth did not change so rapidly we could not keep up with it. We had a bundle of three two wire cables connected between the hydrofoil and the ship's laboratory. One pair for the hydrophone, one pair to fire the camera and one pair for the Kodatron flash. We took a number of pictures in Vineyard Sound and Buzzards Bay. We wrote a report explaining the system including how to tow the hydrofoil and sent it to the Bureau of Ordnance. When we had no reply, we parked the hydrofoil and went on with our other work.

About six months later our contracting officer called and asked when we would have any results to report. Columbus replied that we had sent them a report about 6 months previously and gave them the date it had been sent to them. About a day later they called and apologized since they found our report in their files. They requested that WHOI send someone to Fort Lauderdale to help them outfit their ship to tow the hydrofoil and teach their staff how to use it. I was chosen to go, so I packed a camera and hydrophone and left. We called them in Fort Lauderdale and advised them to obtain a Kodatron Flash unit and some two wire rubber covered cable.

I went to Boston and with suitable priority established by Bureau of

Ordnance got a berth on the train for Fort Lauderdale. The train left an about 8 P.M. and I went into the men's room to smoke a pipe before settling down for the night. Other smokers joined me so that we had a couple of other civilians and a GI there. Some conversation started and one man, about fifty years old began to talk about slackers that avoided the draft refusing to serve their country. Since I was the only civilian aged 24 in the group, it was obvious this talk was aimed at me. I decided I would ignore him. This seemed to infuriate him and he became more and more pointed and critical. Finally the G.I. asked him what he did. He replied that he was a salesman for ladies lingerie. The G.I. then told him to mind his own business and to shut up. He knew, as that man should have, that I would not be there unless I was there in some capacity for the government. I shortly finished my pipe and went to my berth.

I spent about 10 days at Fort Lauderdale outfitting and adjusting their hydrofoil and teaching them how to use it. We never learned whether they used it or if so, what for. "Need to know" you know. After the hydrofoil we were asked to see if we could use the new television as a means to hunt mines. We had to build a small pressure case for a small camera, and one for a much bigger camera. The latter was about 12" X 18" and about 18" tall! The small one was so grainy we couldn't identify anything on the bottom with the opacity of the water added to its discrimination problems. The large one we could see the bottom only from a distance of five feet, in water that we could take photographs from a distance of 10 to 12 feet. We found the results unsatisfactory even while we were drifting, because something would come into view and before we could get a good look at it, it was gone, We wrote the report of our results and heard no more about television.

In the fall of 1943 we received a task that we were to call our "fine print job" in a renewal of our contract. It said that we were required to find out the transmission characteristics at all frequencies for sounds in water depths of 10 and 20 fathoms over an array of bottoms made up of all ranges of geological conditions. Since this was impossible as it was a request to understand all there was to know about underwater sound in those water depths, Doc Ewing went down to the people who wanted to know the information to see if he could get at what they really wanted to know. After talking to them a while, he gave them the answer to their problem. Apparently they didn't believe him, since they insisted we make measurements to show them.

Apparently the Germans were blowing up whole mine fields of acoustic mines by setting a charge off in the water at a distance from the field. The low frequency ground wave which would set each mine off, since they were similar frequencies to be found in ship's sounds before the higher frequencies of the water waves could trip the safety device that was in each mine to activate a device to shut the firing source off when subjected to a nearby explosion. It was decided that we would make records of explosions at various distances in water depths of 10 and 20 fathoms at locations representing five different geological situations. Situations were chosen, near Solomon's Island in Maryland, in Chesapeake Bay for a thick mud bottom, near Jacksonville, Florida for a thick sandy bottom, the Virgin Island, for a volcanic rock bottom, near Barbados for a coral bottom over a thick sandy section, and near Trinidad which was an extremely thick section of mixed sandy and mud layers. The Saluda from Miami, Fla, a former yacht made available for the use of the Navy, was made available for a recording vessel and a Cost Guard Cutter was made available as a shooting vessel as well as a protection from enemy submarines.

Ben Snavely, a former Lehigh Professor when I was a student, and Gail White were made available from the Naval Ordnance Laboratory to help us acquire and set up the instruments on the receiving vessel. Gail White and John C. Owen, who Doc recruited from an oil company went to sea to help Doc on the receiving vessel, and I was assigned to fire the charges from the other vessel. I helped as much as I could to set up the receiving vessel. Besides systems of timing it was also being set up to acquire amplitude and energy data. One set of equipment for this latter purpose recorded on 35 mm film. The film lengths were 1000 ft, so we had to store it so it could be developed on our return. This was difficult because it was hard to set the gain so that it did not overmodulate. Since space was limited on Saluda, Doc set me to building a buoy to connect a seismic receptor to the receiving ship by radio. I had very little to work with as this had not been part of the planned instrumentation.

I got a five gallon can, cut an eight inch hole in the top and soldered a brass ring around the hole to make a fastening for the instrument head. I managed to get a small FM radio transmitter and a geophone together. With Gail White's help (he was our primary electronics man) we designed an amplifier and modulation system to transmit the acoustic waves by radio. Before I could get it all put together the rest of the gear was ready so we had to sail. I never had time enough to get back to my acoustic buoy, so it was never finished.

We fired seismic refraction stations at up to 30 to 40 miles length at Solomon's Island and Jacksonville without any difficulties, after overcoming start up glitches which are inevitable when setting up complex equipment on shipboard for the first time. Only one noteworthy incident occurred at Jacksonville. While at the 20 fm station while we were firing our explosive charges, the sonar picked up a target nearby that was moving. Since there were no visible vessels except the ones belonging to our party it was concluded that a German submarine had been attracted by our shots. We terminated our work and made an attack on the target. The Captain sent a radio message to the shore base alerting them to our detection and attack. The base obviously did not believe us. We made several more attack runs setting patterns of depth charges. The Captain kept sending messages to the base suggesting assistance with the target with no success. As nightfall was approaching, our attacks on the target was taking us farther and farther away from Saluda.

Finally, the Captain broke off the attacks to return close to Saluda since his primary mission was to provide protection for her. The next day a submarine sank a freighter about thirty miles north of us. Then when it was too late, the base began to believe we indeed had a submarine target. I guess the submarine figured we weren't worth a torpedo.

At Jacksonville, it was decided, by the powers that be, to send Saluda to the Virgin Islands without escort. So I was moved on board Saluda for the trip. Saluda could not carry enough fuel for the trip, but since it was expected to be mostly downwind and Saluda was a good sailor, a two masted schooner of 120 ft, it was expected to use sails without much loss of time. The weather did not cooperate and we had head winds for almost the first week. As a result the Captain had to use the engines more than expected and we ran low on fuel before we got to Saint Thomas. So we sailed about half of the way and took longer than expected.

The crew on Saluda had all sailed for many years and many of them had been fishermen. One afternoon while sailing along at about 6 knots, some of them convinced Doc to use the BT winch to troll for Sailfish as we passed a few along the way. A hook was attached to the 5/32 BT wire, breaking stream 1000 pounds, and a flying fish that had sailed onto our deck was used as bait. Within an hour, a Sailfish had taken the bait. Doc put the BT winch in gear and pulled it alongside. It was brought on board. Then the fisherman on board advised Sailfish were inedible that it was only good for a good tussle. There was no tussle with the BT winch, the fish never had a chance. The fish was unhooked and to Doc's disgust released. He had expected a good fish dinner. That was the end of fishing on the way.

Finally, we approached St. Thomas and the wind chose to die. The Captain had saved some fuel to motor into harbor, so he started the engine. We were approaching the island from the north and the harbor was on the south side of the island. Before we got in sight of the island we saw a mirage of the island up in the sky. This was the only time in my 42 years of going to sea that I had such an experience. We ran out of fuel before we had made it around the island. We measured the fuel tanks and found there was about a foot of fuel left. The intake had been raised above the bottom to avoid any possibility of encountering any water that might get into the tanks by leakage or condensation. The chief engineer and I pumped some fuel from the tanks into buckets. We then unhooked the fuel line from the fuel tanks and placed the end into one of our buckets. Diesels do not use all the fuel pumped to them so they have a fuel return line that returns unused fuel to the tanks. We also unhooked this line and arranged for it to flow back into our bucket. Then I held the bucket and made sure the fuel lines remained in it while the engineer babied the engine to run. We finally made it into the St. Thomas fuel dock and refueled. We replaced the fuel lines and were thankful we did not have to use our bucket technique as a regular thing. Another Coast Guard Cutter was waiting for us there and I transferred my equipment and myself to it. The next day Saluda went about revictualling and getting ready for sea. The second day was Christmas and all shoreside operations ceased. Doc and I spent Christmas eve at the bar of the Officers Club as our celebration. On Christmas day we worked on the seismic records we had already taken.

The second day after Christmas we were able to commence work on the observations near the Virgin Islands. One day, after just firing a depth charge, I went to the Officer's mess where dinner was being served. The Captain's seat at the table was under the cover of the ladder from the deck which prevented seawater from coming below decks. As I entered, I noticed the Captain's soup plate was splintered and soup was splashed all over him. He, angrily, asked me why I was blowing up his ship holding a 3/4 inch bolt that had been broken off in his hand to show me. I replied, that the explosion could not possibly have done that. It turned out that in the last overhaul, the hatch companionway had been removed by shearing the bolts off so some equipment could be brought below decks. It had been replaced and somehow a sheared bolt got left sitting on the ledge on the inside of the companionway. The explosion dislodged it and of course it had to land in the Captain's soup. It soon transpired that the Captain's anger had been feigned and I was again persona grata on board. We all had a good laugh about the incident, the Captain went and changed and we completed our dinner.

From the Virgin Islands we went to Barbados. There was a lot of coral on the bottom and our instruments on Saluda became tangled in the coral and they could not be recovered. Since there were no spares it looked like we would have to await new geophones, hydrophones and cables . It turned out there was a group of divers in Trinidad and it was decided to fly them to Barbados to see if they could free our equipment from the coral. They successfully followed our cables to the bottom and cleared everything from the coral. Consequently, we lost only two days instead of at least two weeks.

The Coast Guard Cutter I was on developed engine trouble and limped into Port of Spain in Trinidad. My gear and I were transferred to a 100 ft subchaser. It had pancake diesel engines with side exhausts on either side so the exhaust could be directed to the leeward side. Our work in the Gulf of Paria, an area between Trinidad and some other islands and the South American continent, went without incident. Our last location was to be off the mouth of the Orinoco River in the Atlantic Ocean,where the sediments were expected to be especially thick. We sailed to our station and commenced our work. When I needed to fire some of the depth charges the ship carried, they would not explode. After three attempts I inquired about their history. I found out that they had been reworked by the Ordnance Department in Trinidad. I decided that they must have been assembled improperly since they would not fire, so I asked if they had any engineering drawings of them on board. They did, but the Ammunition Officer became offended that I would possibly think the Ordnance Department could make any mistake, or even more, that I would possibly think I could determine the problem from the drawings. I pointed out to the Captain, that if we could not make the charges work we would have to return to Trinidad, have the problem corrected and return to finish our work and that this would mean at least an extra two weeks or possibly even three weeks. The Captain overruled the Ammunition Officer and I received the engineering drawings.

After studying them for about an hour one of the sailors and I took one of the depth charges apart to examine its works. We did find a problem that prevented the charge from arming. We fixed it and made a test run which was successful. The Captain then had all (about 30) of his charges disassembled and properly assembled. We had no more failures. Nevertheless, the Ammunition Officer refused to speak to me again for the rest of the cruise.

While we were working, a storm developed in our area which became worse and worse. Because of our work we would often have to reverse course frequently and the engineers didn't keep up with the changes of the exhaust. The result was the exhaust frequently crossed our deck where I was working and the saltwater in the exhaust would end up on the deck. This made the deck very slippery and my clothes became thoroughly soaked in saltwater. When I would get out of them at night to sleep, they were stiff. They would stand up on the deck as if they were still filled with my body. Since I saw no reason to get another set of clothes similarly afflicted, I wore those clothes for the three days needed to complete our work.

The storm got worse and worse. I finally had to lash myself to the deck, with the various explosive components within arm reach to continue to fire the necessary shots. The Captain finally came to me and asked if I didn't think we should quit and seek shelter. I replied that as long as Doc could operate the receiving gear in the storm, I could shoot the explosives. On his ship, Doc told me later, he was replying to his Captain that as long as I could shoot, he could record the shots. Since no one wanted to extend the work another three weeks, the time to return to Trinidad to refuel and revictual and return to finish the work, we were allowed to continue. We finally finished after three days working in the storm, as Doc later used to say in the lea of the land, Africa, we headed back to Trinidad with all our data in hand. The storm abated and we had a beautiful day our last day at sea.

The subchaser had a gun on the afterdeck which they had used on a training exercise before I came on board. Due to repair work, the safety switch that prevented the barrel from lowering too far had not been installed properly and they had shot away a piece of the rail. In the same practice, one of the hand fired Thompson Machine Guns had a bullet lodge in the barrel, and the next shot caused an explosion and the end of the barrel was split and splayed out in jagged pieces. The officers asked if I would put on my disreputable salt stiffened clothes so that when we came into base they could claim I was a German prisoner under guard and that the machine gun and rail damage had resulted from a fight with a submarine. They thought it would be a good joke, so I went along with it. Shortly after we arrived at the Section Base, where the ship was based, the Captain was called to the Commandant's Office. He received a thorough dressing down for this "joke". I threw my salt crusted clothes away, never even attempting to save them.

When we returned to Woods Hole Doc and I went to work immediately to interpret the data we had acquired. I was sent to the Naval Ordnance Laboratory to analyze the sound level films that had been recorded on Saluda and returned for photo processing. When I had completed their processing, I took the processed back to Woods Hole to work with Doc on their interpretation. We soon found the early records had been over modulated and had to be discarded. Farther study showed none of these tapes could be used so we had to rely on the calibrations of our records made on shipboard for amplitude and energy measurements. The work we had done on shipboard and ports along the way showed us that the water waves showed sound dispersion. The highest frequencies travelled through the water fastest and were received first on the records. Lower frequencies travelled slower and the waterborne waves were recorded with the highest frequencies we could record first progressing to lower frequencies terminating in a frequency similar to those transrnitted in the ocean bottom. Thus an impulsive signal became stretched out to several tenths of seconds. The duration appeared to relate to the total distance of the shot. Close shots had a short duration and long ones had a longer duration.

We reported this result to a group working on underwater sound theory at Columbia University, sending a couple of records as an illustration. We soon received a letter from Chaim L. Pekeris stating that he had written a theory about this phenomenon about a year earlier, but since he could not find any data to test it he filed it. He asked us to send him copies of records at all of our stations so he could make a thorough test. We did and he showed the dispersion was a function of the ocean bottom geology, the water depth and the distance between the shot and the receiver. These results later led Ewing and Press and later a number of associates to explain the dispersion of earthquake surface waves.

When our report was completed it was clear that the ground waves from an explosion at some distance could cause acoustic mines to fire before the water waves could arrive to enable the preventive mechanism to work. Despite the fact we did not "need to know" we later found out that they redesigned the acoustic mines to avoid this difficulty.

While we were in Barbados refueling and revictualling, we had been invited to go to the USO Club for an evening. When we got there we found out that they were having a dance. One of the young ladies persuaded me to dance with her. At the end of our dance they announced that they were about to have a waltz contest. The contestants were to waltz and a committee would retire contestants one by one who were poorer at waltzing. The girl I was with insisted I should participate with her since she did not wish to miss it. We survived the those cuts, but the other couple won. All I can say is that the girl I was with must have been a wonderful dancer.

In Sound Transmission in Sea Water, Doc Ewing had mentioned the existence of sound channels in the ocean. These were places where negative velocity gradients overlay positive velocity gradients. Negative velocity gradients usually occur where the temperature decreases as depth increases. Positive velocity gradients usually occur where the temperature decreases only slightly or not at all as depth increases. In these latter cases the effect of increasing pressure increases the sound velocity. A sound channel therefore occurs when a region of decreasing sound velocity with increasing depth overlies a region of increasing sound velocity with increasing depth. The axis is the depth where the change from the former changes to the latter. Sound channels occur in the ocean because of the normal temperature depth relationships. The main sound channel occurs because of the heated near surface waters that have absorbed the sun's heat which overlies the normal cold ocean waters at depths greater than the sun's heat penetrates. Near surface sound channels occur because of either seasonal heating of the surface waters or diurnal heating of surficial waters.

In July, 1943 Ewing wrote a memorandum to the Bureau of Ships, U.S. Navy Department proposing the main sound channel as a long distance sound transmitter which could be used to locate airplanes downed in the ocean. Any survivors could drop a small bomb to the sound channel axis and three widely separated receiving stations could triangulate the received signals to locate the source. During WWII a number of planes went down over the deep ocean due to engine failure or enemy action. Many times passengers and crew would survive for periods up to a month. Often, they would not be found in time. A bomb located in the aircraft would even fire automatically when the aircraft sank. Aviators mixed the idea since they did not want a bomb on their aircraft because of passenger safety. This was ridiculous since the aircraft of those times carried thousands of gallons of high octane gasoline, which was much more hazardous. Navy

Laboratories responded that the greatest distance that the largest bomb (I believe 5000 pounds of TNT) could be heard was about 30 miles.

Nevertheless, Roger Revelle, then in the Bureau of Ships, authorized the investigation of the main sound channel as a sound transmission experiment. A Navy Destroyer as a firing vessel, the Saluda as a listening vessel, and an escort vessel to protect Saluda were authorized.

We built fifteen "infernal machines" to fire charges at a depth of 4800 feet, the Sound Channel axis. We called them "infernal machines" because they were fired electrically from batteries using a boundon tube as a pressure elememnt to close the switch. With such a device it was always conceivable that an accidental closure of the switch could cause an unintentional firing. Such an event might endanger personnel. To prevent this we brought two leads through the bomb case. A 100 ft electric cable would allow the bomb to be lowered 100 ft into the ocean before the circuit to the batteries was connected.

Not knowing how efficient the sound channel was, we decided arbitrarily to make the charges four pounds of TNT. We later found by extrapolation that this size charge could be heard halfway around the world. Nevertheless this was the size charge we used in all of our sound channel work to be described below. Later, after WWII, because of the extensive sound channel data in existence, when the Navy decided that they needed a standard charge for all sound intensity measurements in the ocean, this charge size was chosen as a standard. Such is the nature of things, that standards are sometimes chosen arbitrarily.

The experimental group moved into the Atlantic to the Northeast of the Bahamas. Because of the excellent advice Gail White had provided for our electronics work previously, despite the fact he was chronically seasick, Ewing persuaded him to sail on Saluda in this same capacity. After hydrophones at 1000 ft and 4000 ft were deployed and working, the destroyer was requested to sail to the northeast firing bombs at fifty mile intervals. Before the first firing occurred the electric generator failed on the destroyer and she had to be detached to return to base for repairs.

In order not to waste all of the effort, Ewing arranged for the escort to set off for a distance of about 50 miles firing a surface charge, a hand grenade at the surface and a four pound charge at a depth of 600 ft. at five mile intervals. Ewing and I analyzed the results of this work and wrote a report about this work that concluded that low frequency sounds could be transmitted very long distances. The Bureau of

Ships refused to allow us to distribute this report because it would upset their sound doctrine that they had had so much trouble to set up. This work, nevertheless, was a major part of the basis of my later work with Sub Dev Grp 2 which evolved into the SOSUS system well after WWII.

After a while, the aborted experiment was reinstated and got underway in the same area. This time the firing ship made its way to 1000 miles firing charges at 50 mile intervals. The incoming sounds were put on a loud speaker as well as being recorded on a photographic oscillograph. The sounds were still so loud at 1000 miles that the cook in the galley towards the bow of the ship could identify the sound channel sounds coming from the loud speaker near the stem of Saluda. These sounds were unique. Although started as a single abrupt sound, they arrived in groups of four. Many groups arrived, each group having a shorter interval from the previous group until the intervals became so short the groups were coming in on top of another. Each succeeding group was louder than the preceding one making a crescendo at the end when the sound quit abruptly (later found to be within less than .003 seconds). The number of groups and the duration of time of arrival of groups increased as the separation distance between the ships increased.

The signals being received were unique and loud so when the last shot at 1000 miles had been fired, Ewing requested that the range be farther expanded. The Captain of the subchaser refused because his orders only authorized 1000 miles, which had previously been thought farther than the sounds could be heard. Our later analysis indicated by extrapolation we could have had heard this size bomb at 12,000 miles, halfway around the world, if deep ocean path existed between source and receiver.

Because of the outstanding results of these tests we were authorized to establish a deep water receiver connected by cable to a land receiver. We also recommended that submarines carry a Sofar bomb, which could be released from their signal tube if they were under attack by the enemy. Planes could then be sent to the attack location to drive off planes or vessels pressing the attack. This was in response to the news that Japanese were attacking our submarines trying to interdict their merchant marine support.

The operational response was that since an impulsive signal was drawn out to about 10 sec.s in length for each 1000 miles of path, a coded signal would take too long a time to send to be of any use. This was of course nonsense, since a single signal was so distinctive that it could not be confused with any other signal in the ocean. Furthermore, the abrupt cutoff would allow location from three suitably located listening stations to determine the location of the source to better than one mile.

Nevertheless, it was decided to set up a land based station. we chose Eleuthera Island in the Bahamas to be close enough to deep water to establish a sound channel hydrophone cable connected to shore. Since Eleuthera was surrounded by coral, we had a 1,000 ft. armored cable and 20,000 ft. of demolition cable suitable for such an installation be made available. McCurdy was set to work to construct suitable amplifiers, recording equipment, and obtain loud speakers for a permanent land base. We requested the Bureau of Ordnance, of the U.S. Navy to develop bombs suitable to equip Navy ships and airplanes to carry and drop for planned tests. Jim Peoples was selected to be supervisor of the Eleuthera Station and to go there and arrange for a suitable land based station and living quarters for the expected station staff.

I was sent to Nassau, Bahamas, to connect the 1,000 ft. lengths of demolition cord together to make a 20,000 ft. connecting cable for the hydrophone. Nassau was the only nearby harbor suitable for Valour, the former yacht turned over to the Navy for various duties, thus relieving fighting ships to fight. Valour was about a 100 ft. two masted schooner with an auxiliary diesel engine which could drive her at about 8 knots.

Demolition cable was made up of two stramds. Each strand consisted of thirty or so wires about half steel and half copper individual wires twisted togetheer which were about one eighth of an inch in diameter. These were rubber covered and then covered with a woven fabric cover to provide some protection to the rubber from chafing and cutting. These two strands were twisted together and a filler of cotton fibers were added to make the package into a circular section. The outside was then covered with a tough neoprene jacket so that the whole cable formed a circular section about 5/16 inch in diameter.

The connections of these 1,000 ft. lengths of cable into a 20,000 ft. length was difficult. Each end had to have the neoprene jacket cut back about three inches, and all of the insulation cleaned off the strands for about two inches. This was all such tough material that this was a severe chore. Then the going got tough. The wires had to be connected and soldered to make a strong mechanical and electrical connection. This meant separating the copper conductors from the steel strength members, linking the steel wires by doubling them back on themselves with each one linked to the other. Then the copper wires from each side had to be wound around the linked steel members and the whole connection soldered together for good electrical connection. Each joint then was coated with rubber cement and at least a double layer of rubber tape. The two joints were then pressed together and covered from one neoprene jacket to the other with a double layer of rubber tape to make the whole joint watertight. Finally, a covering of friction tape was wound on to give mechanical protection.

As each cable union was completed, it was put over the side into the saltwater to assure that the joint was watertight. In the end we had a 1,000 ft. pile of demolition cable, the end fed over the side so the joint could be tested in saltwater and another pile of demolition cable and so on. With 10 unions in the water on each side. The ship appeared to be pretty much a derelict much to the Captain's dismay.

After "soaking" all unions for a least 24 hours with no leaks detected, I had to repile all 20,000 feet of the cable on deck in one pile with the hydrophone attached to the bottom end .

Then I was flown to Eleuthera to lay the armored cable from shore to the 30 fm. water depth. Since I had no previous experience with armored cable, I studied carefully the brochure included that described and pictured the technique for connecting two armored cables. Since I was connecting demolition cable to an armored cable I had to devise a modification of their technique.

When I felt I understood how to proceed we coiled the armored cable in the well deck of the Coast Guard Buoy Boat which had been provided by the Navy for our station use. The Buoy Boat, normally kept at Half Sound, was moved to Quarter Sound which was near to our station location on the Atlantic side of Eleuthera. Then we waited for the weather. While waiting I decided that a good look at the bottom near where we were bringing the cable ashore would be helpful. We took the Buoy Boat offshore and launched the skiff, where with a water glass I could view the cable in water depths up to about 20 fins. One of the locals sculled the boat while I scanned the bottom. About 50 feet from where the waves were breaking, he declined to enter the wave break zone. I felt I needed to see that area so I told him to return to the Buoy Boat. I lashed the water glass alongside the dinghy. I started to scull to the area myself This amazed our local, who had thought we had no skill in handling small boats. I scanned the cable through the breaker zone to a water depth of five feet, where we had dug a trench that would allow us to bring the cable ashore underground, avoiding the zone of waves breaking against the shore. Finally, the weather became calmer and we brought the Buoy Boat offshore from Quarter

Sound and anchored near enough to shore that we could carry the cable end in the skiff to where a heaving line could reach the shore. A heaving line was attached to the armored cable and with a number of locals adding their Norwegian Steam to that of our shore staff, they pulled enough armored cable ashore to reach our recording shack. The armored cable was about 1 1/2" in diameter and the outside was completely encased in steel wires about one eighth inch in diameter wound around the core of 1" 2 conductors and cord stuffing.

When this was completed most of our Norwegian Steam engines (about six of us) moved onto the Buoy Boat and paid out the armored cable as the Buoy Boat moved offshore to about the 30 fathom curve. The end of the armored cable was attached to a buoy which we moored there. On our return to shore we radioed to Valour to move from Half Sound on the eastern side of Eleuthera to the offshore region in the Atlantic within a few miles of our buoyed armored cable.

When the weather cooperated, Valour came near to the buoyed armored cable and anchored. We came out in the Buoy Boat and obtained the top end of our 20,000 ft demolition cable and picked up the armored cable, again with Norwegian Steam until there was enough on board to make the connection. We didn't have to moor the Buoy Boat as the armored cable provided all the mooring we needed. In the well deck of the Buoy Boat I made the connection between the armored cable and the demolition cable. This took several hours to make the mechanical and insulated cable connections. When finished we threw the end of the armored cable over the side in the appropriate direction.

Several of us moved onto the Valour. The Captain wanted to move slowly to sea paying out the cable onto bottom as we went. I insisted instead that he go to sea along our route at full speed, with us paying out the cable as we went. The cable settling laterally through the water didn't pull the cable out faster than we were progressing over the bottom. I had three people supervising the cable coming off the pile near the bow to make sure it did not tangle. Two people were stationed along the deck to keep it from hanging up on any of the deck fixtures and I paid out the cable off the stem making sure it cleared the rail. When we reached the hydrophone, I tossed it into the water and let it sink. On shore, McCurdy and Peoples monitored the speakers. About a half hour after the hydrophone was put into the water the shore party radioed it had reached bottom and had become suitably quiet, i.e. no more "water noise". Valour returned us to the Buoy Boat. When we reached shore we asked Valour to sail to the northeast firing 4 pound charges at 20 miles, 50 miles, and each 50 miles until we lost our signal or our radio contact failed. We were jubilant that all was going well. About 24 hours after Valour left, the signal failed and a short circuit was evident between our signal leads. Valour was recalled from about 180 miles away.

Since that day was well advanced, we waited for the next day . The next day we went out in the buoy boat and grappled the end of the armored cable. We buoyed it, and started to underrun the demolition cable. When we got to about the 50 fathom curve, we found all of the insulation off the demolition cable, the copper wires all gone, and the steel wires polished clean. We continued to under run the cable until we found the cable intact. At this point, we cut the cable and tested it beyond the break. It tested ok, so we insulated the end and buoyed it. Then we returned to the shore end, and cut the demolition cable off the armored cable. The shore then reported that the armored cable tested o.k., so we buoyed it and went ashore to decide what to do.

The group got together to discuss the situation. We decided there must be an outcrop at about the 50 fm. depth, or coral from shallow water had piled up on this area of the slope and provided enough sharp edges to do the damage we had found. This meant of course that the cable must not have been unsupported for a ways, and the current moving the cable back and forth did the damage. We therefore decide to run the armored cable farther offshore until we could connect to the remaining demolition cable. That meant that I would have to go back to the manual and refresh my mind on how to splice two armored cables together. I did that after dinner.

The next day I had to build a device, described in the manual to allow me to protect the splice of the two armored cables. This took only about an hour. Meanwhile others loaded the buoy boat with a length of armored cable . When we had finished that, the day was too far gone and we decided to wait for the next day. We turned too early to make the splice of the armored cables. This took most of the morning. We then laid the armored cable out till we could get the demolition cable on board with sufficient slack to make the splice. We cut the armored cable off. This was a hard job because of the armored wires and we had no way to immobilize the cable except by holding it. Occasionally the hack saw stuck on one of the armor wires and the holders could not hold it still. When this happened we had to increase the number of holders until we could get started again with our hack saw. The cut off was quite ragged, but it sufficed. I immediately turned too to make the splice. This took quite a long time and it was well after dark when we finished. The radio to shor told us that all was well, so we eased the armored cable over the side supported by a doubled up rope until it reached bottom. Again the shore reported that all was well, and we went ashore.

Dr Ewing arrived and I was sent home with instructions to get some of the bombs from the Bureau of Ordnance and send them down so that experiments could get underway. When I called the Bureau, I found they hadn't even started yet! I passed the news to Eleuthera. I was instructed to get Ed Douglass to build some more bombs with the improvements he had made since we used them earlier. He made about ten in a hurry and I shipped them to Eleuthera. Meanwhile Ed started another group of ten. None of them would fire! Dr Ewing returned, bringing one of the bombs with him.

I was sent to find out why they wouldn't fire. Since they could not go off unless they were under pressure, and we couldn't attempt to fire one in the pressure vessel, we had installed in the basement of WHOI because of the explosion possibility, I had to build a small pressure vessel capable of pressures of 3000 pounds. It had to be large enough that a bomb could be placed in it. I found out that 1/8th inch copper tube would stand those pressure, so I got a hydraulic jack and attached 100 feet of the copper tube to it. When I was ready, I was transported to Elizabeth Island across the harbor to put the bomb under pressure. We deemed the 100 feet of copper tube enough for me to safely put the bomb under pressure, and since I was on a lonely part of the Island, no one else would be bothered if it went off. I ran the pressure up to 3000 psi and the bomb did not fire., although it was supposed to fire at 2400 psi.

Then I had to open the pressure vessel and extract the bomb. Of course I waited one half hour hopefully to protect me from a hang fire. When I had extracted the bomb, I took it apart and confirmed that it could not possibly go off, so I called WHOI on the radio and asked to be picked up. This was probably the most dangerous thing I did in WWII. Meanwhile a fairly dense fog had come in so they advised me that they could not pick me up. They said they would try to arrange with a farm on the Island to feed me and for a bed for the night, so they would call me back soon. When they called back, they said they had made arrangements and gave me directions how to get there. It was difficult because of the fog, but I finally found the farm. I had left all my equipment back in the field I was using.

In the morning the boat from WHOI came over to pick me up. I thanked my hosts for their hospitality and left. We stopped and picked up the equipment I had left in the field the night before. After off-loading the equipment I went to Doc's office and reported what I had found out. He called Ed Douglas and discussed the results with him. They decided that Ed should stop work on the bombs he was building and redesign the arming mechanism. When he had finished that he would resume his building. They estimated that would take at least a month!

After he hung up the phone, Doc turned to me and said that was too long and that we would have to make some quicker. The ones we had made previously we decided also would take too long. We decided we would have to make another design that would not take so long to build. Doc then said that we should start with the firing mechanism, and that we should aim to make it about the diameter of a pencil. I thought he was crazy, but said nothing. We talked about the demolition blocks that i had been firing in the explosive reflection tests. These were 2 inches by 2 inches and about 4 inches tall, and were i/2 pound of TNT.. They had a well inside with a booster charge. With these we had been able to fire larger charges. One could put either an electrically fired, or a fused detonator in this well to make the demolition block to fire. Fused detonators had a burning fuse that caused the detonator to fire. Detonators were a little less than 1/4 inch in diameter and about 2 inches long. While we were talking, one of us recalled our collapsed tube which had caused a second order explosion in our pressure vessel at Lehigh. If we could make a tube collapse, have the collapse ignite a flame, we could use a fuse type detonator and have the flame set it off. I was set to work to find tubing about 4 inches long that would collapse at 2400 psi, the pressure at the sound channel axis.

I had a book that gave the formulas for the collapse of bodies under pressure. Taking the strength figures of brass I calculated how thick the wall would have to be to collapse at the desired pressure. We didn't have this size in the shop, so I had Chuck Bodman, the machine shop foreman to make an emergency order for some. We had to take 100 feet of tubing to have the supplier fill our order. When it arrived the next day, WE cut off a piece about 4 inches long. Chuck had ends machined to be .002 inches bigger than the tube diameter with the leading edge chamfered.. The back end was made equal to the outside diameter of the tubing and about 1/8 inch long. These ends were pressed into each end of our 4 inch piece of tube, and Harry Robinson, my assistant, and I put it in the pressure vessel and tested the collapse pressure. The calculations proved to be correct, and the tube collapsed very near to the desired 2400 psi.

Now we had our collapse tube, we put an engineers special detonator in it. This was the most powerful non-electric detonator that you could get. Then we had to find something that would initiate fire when the tube collapsed. The first thing that came into my head was matches. Since we couldn't put a whole match in I decided to cut off the head of a match and use it. I further decided that one match head might not be in the proper place, so we should use five match heads. To keep them from all falling into one place, where they might not be hit by the collapsing tube, I placed them on a piece of cellophane tape. We would cut off the excess tape and insert them into the tube. For safety reasons we built a device that surrounded all but the very tip of the tube with about an inch of brass, with a lever arm to press the head into the tube to seal it . This arm was made almost all of metal, but to the end we attached a short piece of wood. This made a hand grip that would break easily if somehow we set off the detonator while sealing the end.

Harry and I made twenty separate tests of this device and they all worked fine in the pressure vessel. It looked as if we had our desired firing device. However would they fire a demolition block? WE made up a number of detonators and sent them to Eleuthera with instructions of how to insert them into a demolition block. The tests were made by attaching a weight to the block to make it sink fast from the buoy boat. After twenty tests the answer came back that only half fired. Since the detonators had worked 100% in our pressure vessel, we decided that the extra thickness of our collapse tube reduced the power of the detonator explosion so that the firing of the demolition block was uncertain. The obvious answer was to increase the power. Since we were using the most powerful detonator made, we obtained a pellet of PETN, another explosive and put it in our tube beneath the detonator. Testing proved that we had a winner.

We got our further work with the sound channel under way. Because it was somewhat dangerous installing the match heads on their piece of tape, I called a match company and asked to buy the material that match heads were made up of, so we could paint it on the inside of our tube instead of inserting our tape unit. The match makers refused to sell us any, because they said it was too dangerous. Doc knew some of the people in DuPont explosives plant from our use of explosives in our prewar seismic work. He called them and asked if they could load our tubes directly, inserting something to make fire in the top for us. They said they probably could, but we had to visit them and show them exactly what we wanted.

Doc arranged for me to go down and talk with them. I went home and changed my clothes, and walked down to the train station to catch the evening train. On the way a young kid aimed his cap pistol at me and shot it saying "mister you're dead". I grabbed him, and I guess scared him badly, but when I could calm him down, I asked him where he bought his cap pistol ammunition. He named the store in Falmouth. At the station I called Harry at the office and told him to buy some and test them while I continued on my visit to DuPont. At DuPont's I explained what we wanted and they said it didn't sound too hard.

On my return to WHOI I found that Harry had successfully tested the cap pistol ammunition, and that he thought it was safer than our match heads. There was only one thing, it was hard to get a strip of cap pistol ammo in our tube. We solved that by bending a strip in half lengthwise and trimming the excess off each side. Now we had our firing device and we could make it easily and comparatively safely. Our work progressed.

About two months later, DuPont called us and said they had some detonators to try out. They had packed the explosive charge directly into our collapse tubes and had installed a coil of wire which contained a material that would make fire when struck hard enough. They sent them to us and we tested them in our pressure vessel. Some would fire and some would not. We called them and told them the results and told them of our success with cap pistol ammunition. After some discussion, they agreed to make some following our pattern, since this was war time, but they refused to prepare the cap pistol ammo. I don't know, but I think it hurt their pride that caps were better that the material they had come up with. Of course we prepared caps for them and sent them to them for all of our orders, usually one hundred at a time.

WE named these "Woods Hole Detonators".

We used these detonators on a trip from Eleuthera to Dakar.When a bomb was dropped near Dakar on one occasion, it was 2300 miles away. The bomb was dropped, and the explosion signal was received at our recording station by radio. We then shut down our gear and went to have our suppers. When we returned an hour later, after supper, we turned on the gear in time to receive the Sofar signal! We had the services of a naval ship for this experiment, with our Naval Liaison Lt. Newhouse firing the charges. We planned to have other cruises, but the Navy protested having their personnel to fire our bombs. Probably because they were eight demolition block lashed together with a sash weight to sink them rapidly.

Newhouse went to Bu Ord to try to get our bombs approved. He was stymied until we could tell them what chemicals were included in our firing system-- cap pistol ammunition. He called me and asked me to find out. I called the fireworks manufacturer and asked them. They stated that the chemicals they used were the most dangerous they had to handle. I asked then how come they could make cap pistol ammunition out of it and sell it to kids. Their answer was that in the quantities they used in cap pistol ammo it had negligible danger. I reported all this to Newhouse.

Newhouse went back to Bu Ord and gave them this information. While he sat there, the Bu Ord guy called the Bureau of Mine Safety and asked about the safety of this material, since the Dep't of Defense had no experience. The Bureau of Mine Safety had responsibility for the civilian explosives safety. Newhouse could overhear the man at the Bureau of Mine Safety laugh and say, "that stuff is so safe, explosive manufacturers make cap pistol ammunition out of it." Actually in large quantities it is awful stuff. It is made of fulminate of mercury, potassium permangonate, phosphorous, and carborundum amongst other things. To make cap pistol ammo they dip a device made of a line of 1/4 inch rods into a slurry of the material, then touch the paper backing, leaving a drop where each rod touches. After it dries a thin paper covering is added, sealing the explosive dot in.

The end result was that we received permission to use it on an experimental basis. To use them we had to insert the detonator into the demo block. This constitutes an armed charge. We were given a year before we had to use charges that would arm the explosive after it was well away from the ship or plane. Newhouse got an order for Bu Ord to provide us with such a device.

Although it is out of time context, I'll finish the explosive story here.

One year later I was sent to Bu Ord to arrange for a shipment of bombs so our work could continue. When I arrived I found out that they hadn't even started to design them yet. I asked how long a design would take. They said probably six months, with another four months to manufacture some, and several months for safety testing. I protested that this was outrageous, that we could do all but the safety testing in less than a month. They responded that why didn't we do it then, send ten specimens to them for safety testing which they would do immediately. We were given a three month extension to use our present system. In total disgust I took the train from Washington to Woods Hole.

On the train, I had the demolition manual. This listed all of the explosive devices, explosives, and auxiliary equipment available for Navy demolition use. I leafed through it and found a small bomb used to make smoke signals. The body was about two inches long, contained about a pound of smoke materials, and had a metal case including tail fins so they could be dropped from planes. On the back of an envelope, the only paper I had with me, I designed a bomb arming mechanism which could be fitted in the tail fin region. The bomb would have to be cut in the middle of the body and have the body extended to hold the four pound charge, but this was no problem. At woods Hole, I had Chuck Bodman start the construction of one arming mechanism. I called Newhouse and he had 100 empty bomb case sent to me, arriving within the week. We fitted the arming mechanism to the bomb and tested it in our pressure vessel. It worked well. I had Chuck build ten more immediately and we sent them to BuOrd for testing. We of course provided ten detonators, in which we did not include any charge. They tested them promptly and gave us our O.K. to use them with Navy personnel firing them from Navy ships and planes. This was achieved one month after my trip to Bu Ord. Our work progressed without further explosive hitches.

After WWII, Woods Hole had a number of Woods Hole detonators made up to fire at at least four other depths than the sound channel axis. They were used on other underwater sound experiments

As soon as we had our new model bombs made and filled with explosives, the Underwater Sound Lab. at New London (USNUSL) agreed to send a ship out to fire some shots to be received by Eleuthera. They sent a truck from New London to pick up the bombs. It arrived on a Saturday afternoon. I brought out the bombs, expecting to load the truck. The truck driver refused to take them until they were solidly crated. This did not make much sense, since only a bad fire, or external pressure of 2400 psi could set them off. Neither could be encountered by the truck. In war time there was no traffic on the road because of gas rationing. Nevertheless the driver was adamant and a whole lot of people deployed for the experiment would be held up if the shipment wasn't made. I called Stan Eldredge, the wood worker to ask for help. He must have been out somewhere since his phone didn't answer. I decided I'd have to make a crate. There was only one problem. All the tools were locked in a cupboard in Stan's wood shop. I took a fire axe and chopped the lock out of the door, so I could get the tools I needed. I made the crate and sent the bombs on their way.

The following Monday, I made sure I was there when Stan arrived. I told him what had happened, apologized as some of the tools were his own tools. Nevertheless he was angry. I was surprised since Stan and I had been very good friends. I had been careful in my chopping to make it as easy as possible to repair. As soon as Columbus, the director arrived, Stan went to him and complained. Columbus asked him why he didn't take an axe and chop my door down? Stan averred that he would do that, and left. Later he realized he would have to repair my door too, if he chopped it down. Stan wasn't my friend for the next week, but he got over it. We had many good laughs over the incident subsequently.

On another occasion we agreed to truck bombs to USNUSL for another experiment. When they were ready there was no one to drive the truck to New London. I had some other commitments for the experiment so I couldn't do it. In desperation I asked Beverly Steele, my secretary if she would do it. She replied that she would be glad to, but she didn't know how to drive the truck. It had a five speed gear shift. I told her I would show her how. I explained the system to her, then drove around the Eel Pond showing her the proper way to use the clutch and gear shift. She then got in the driver's seat, and drove around the Eel Pond. She did a fine job, so I loaded the bombs and sent her on her way. How do you women's lubbers like that? Shortly afterward Columbus called me into his office and asked me if I had done that. I told him I had and the reasons. He was aghast, and he made me promise not to do it again. Beverly got a big kick out of it when I told her. She was about 5' 10" tall and very thin. She was about twenty five, married to an army officer who was serving in the Pacific. She thought it was a great lark.

At about that time, Commander Knight, who had been put in overall charge of the project for the Navy asked for a report about the bombs. I wrote the report stating that most of the bombs used so far had cost in excess of $100 while the Woods Hole bomb cost $00.75. Commander Knight was incensed about the use of the double zero. It was in fact due to a misstrike on the typewriter. I did not hear of his attitude till much later. He would probably not have believed it was due to a misstrike anyway. He approved the use of Woods Hole detonators to make bombs, until the self arming bomb could be approved.

With the receiving station working, and bombs available the station was set to record bombs fired in a wide selection of locations in the Atlantic which were not behind continents or islands. This was accomplished by Newhouse flying on various routes of the Naval Air

Transport Services (NATS). Each hour Newhouse would enter the head and drop a bomb through the commode. At that time the commode dumped overboard. I don't think Newhouse always obtained proper authorization for this activity, but it worked. Since NATS routes did not cover all areas of interest, several cruises were arranged to cover them.

One cruise was sent out to delineate the shadow of Bermuda to our Eleuthera installation. Bombs were fired at one hour intervals as the ship passed at right angles to the line through Bermuda from Eleuthera, first 300 miles beyond Bermuda, and then 1000 miles beyond. At three hundred miles, the three bombs in the shadow of Bermuda were not heard. At 1000 miles, all the bombs were heard, but the ones in the shadow of Bermuda were reduced in intensity. This showed that diffraction occurred around Bermuda and that these sounds in sea water acted similarly to light in air.

While that experiment was under way, I was set to work to write a classified report on the work we had accomplished. We had made ray diagrams of the paths of sound originating at the axis of the sound channel for, Doc's original proposal, for a distance of about 45 miles. To do this we had taken representative velocity depth curves available from prewar cruises of temperature and salinity versus depth curves from different parts of the Atlantic. We took a mean curve and approximated it by seven linear gradient velocity depth segments. These showed that rays started at the axis with an angle of up to 12.20 degrees above the horizontal recurved downwards because of the increasing velocity toward the surface. Those rays started downward with angles up to 15.19 degrees, recurved upwards because of the increasing velocity beneath the sound channel axis. At higher upward angles, the rays impinged on the surface. With larger downward angles rays impinged on the bottom. Upward traveling rays after recurving reached the axis again and became downward traveling rays, and vice versa. Because of the stronger velocity gradients above the axis the upper recurved ray traveled a shorter horizontal distance than downward traveling rays. The upward traveling rays I called tops, and the bottom traveling rays I called bottoms. Rays contain a top and bottom I called cycles. Rays restricted to travel beneath the surface I called sound channel rays (SC), those which traveled between 12.20 and 15.19 degrees I called refracted and surface reflected (RSR) rays, Those with steeper inclinations, which were reflected both at the surface and the bottom I called reflected (R) rays. If sounds were made at depths slightly different than the SC axis, the rays which started downwards traveled slightly different paths than those started upwards at the same angle and would have slightly different times of arrival at a distant point. For distant shots, sounds received would have traveled a number of cycles, We had to develop a way to calculate the time of travel along rays since rays of different inclination traveled much different distances for any particular separation of receiver and shot. We derived such formulas and found the easiest to use involved a function called Gudermanions, named after Guderman who had first investigated these functions. Although obscure, we found a set of tables of these functions. They were tabulated in grads which I took to be the French word for degree. After I had spent a week calculating the times of travel along different rays, I found that in this case grads represented angles where a quadrant (90 degrees) was divided into 100 parts. The table had been computed when the metric system had been initiated. As part of the metric system, they had tried to divide a quadrant in 100 parts, but it had not been accepted with the rest of the metric system by the international community. I had to go back and convert all my angles into grads to calculate the desired travel times. It became apparent that three paths near one angle would have nearly the same travel times between two points on the axis. One with a whole number of cycles, one with the same number of cycles, but one less bottom, and one with the same number of cycles and one less top. If either the receiver or the shot were slightly off the axis, the path of even cycles would separate into two different paths of slightly different travel times. At significant separations of shot and receiver, there were a number of whole cycle paths between these points, and a number of different travel times.Those sound traveling closest to the axis, traveling the shortest distance, traveled the slowest and were the loudest since they suffered the least absorption. Since it was nigh impossible to locate the source or the receiver precisely at the axis, the sound arrived at a distant point in groups of four, with the weakest arriving first, becoming louder as each successive group arrived and having shorter intervals of time between each successive arrival, until the groups started to overlap each other. Thus the loudest sound arrived last and terminated abruptly. To facilitate calculations it was decided to graph the horizontal distance for tops, bottoms and cycles against axial angles. Then the times of lead time from the axial ray, the slowest, versus horizontal distance were computed for SC, RSR and R rays,

We had recorded, these sounds on channels of different frequency response which made the well known difference in absorption of sound apparent. The lower frequencies were absorbed less than the higher frequencies. Because of all these calculations and graphs I had made separate binders for etch investigation which I labeled Lorasto for long range sound transmission in the ocean. Later the Navy decided to name this study Sofar for sonic fixing and ranging.

Finally I had all the calculations and graphs made so I wrote a first draft of our report. When this was finished, Doc and I got together and went over the draft to correct it. We worked on it all day at the office, and then at night, I would go to Doc's house and we would work on it till midnight. This revision seemed to take longer than all the rest of the work put together. Doc seemed to want to agonize over every dot of every I and every cross of every t. Finally it was done, even though I thought it would never end. The manuscript was finally typed, the figures drawn and an inch thick Confidential report was completed . It was distributed to the list of recipients the BuShips had sent us.

While work progressed on the manuscript, we had been arranging the assets to undertake a large triangulation experiment. We would use Eleuthera as one station, set up a listening station near New York but on the far side of the Gulf Stream, and set up a listening station on Atlantis in the Sargasso Sea. Bombs would be fired along NATS routes crossing the area within the three stations and extending westward beyond. Because the war was over with Germany, it was decided that Atlantis could be safely sent to sea in the Atlantic again. She had been too good a target to risk in the Atlantic while the German submarines were active.

I was chosen to man the Atlantis in the Sargasso sea. While we were on our way to sea, the Japanese surrender occurred and we were uncertain whether that would cancel our experiment or whether we should proceed. The captain asked for an answer from WHOI, meanwhile he anchored the ship on the continental shelf. After waiting all night we received a message to proceed with our task. We got underway again and proceeded to our position near 25 North and 60 west. Atlantis was one of the few ships, at that time, capable of anchoring in deep water (about 2700 fathoms).

We started the anchoring procedure and I attached a spar at right angles to the anchor wire rope at the position we expected to remain at the sound channel axis. We attached a hydrophone to the end of the spar, about twenty feet from the wire rope, and fed the hydrophore cable over as we proceeded to finish anchoring. When we had settled down, we turned on our listening gear and found it terribly noisy. After allowing plenty of time for the noise to abate, we decided it never would. The anchor cable was just too noisy. We started to recover the hydrophone cable only to find it had wound itself around and around the wire rope anchor rode. This was my first experience, that because of the great weight of the wire rope, as it was paid out it would rotate to unlay some of the turns that we're made in its manufacture. In order to try to save the cable to the hydrophore we tried to push the turns of cable around the wire rope down as the wire rope was recovered. After recovering about 100 feet of the 4800 feet of cable, this became impossible from on deck. I then had them rig a bosun's chair under the bow, and I got on it and tried to push the coils down as the wire rope was recovered. It soon became evident this would not work, so I took a pair of diagonal cutters (dikes in our vernacular) and started cutting the cable turns off the wire rope. After clearing the mess within my reach, they would recover some wire rope until I could no longer push loops down the wire rope, Hoisting was stopped and I would spend another fifteen minutes cutting cable off the wire rope. This was repeated until we had recovered the hydrophone and cut the spar off the wire rope. Then we let out the wire again until we were anchored. This process had taken about eight hours to complete,

Anchoring in deep water is quite different from the technique in shallow water. In shallow water one lets out about three times the water depth on the anchor rode. This is to make sure the ship is pulling nearly horizontally on the anchor which continues to set the anchor firmly in the bottom. In deep water the anchor is attached to about 300 feet of heavy chain. The other end of the chain is attached to a heavy weight, about 1000 pounds of lead which in turn is attached to the 20,000 feet or so of wire rope on the ship's trawl winch. Several hundred feet of wire rope, more than required to lay the heavy weight on bottom is paid out. The heavy weight and heavy chain assures that the anchor is pulled along the bottom so that its flukes get a good grip on the bottom. The ship bobbing up and down on the waves and under the influence of the waves, may lift the weight a little ways off bottom occasionally, but it quickly settles on bottom again.

Sitting on the bosun's chair under the bow, near the wire rope is quite a hazardous undertaking. If the wire rope should part, it is most likely to part at the sheave over which it pays over the bow. Since it is under great tension, from the weight of the 18000 or so feet of wire rope between the bottom and the ship, if it should part the wire rope wound unlay and whip around the area of the brake. This would do great damage to anything nearby.

Having destroyed the cable to the hydrophore, we had to fashion a new one of 5000 feet out of 1000 foot lengths of demolition cable. When it was complete we lowered the hydrophore over the stern. It was satisfactorily quiet. We radioed to shore that we were in position and ready to proceed.

At an appropriate time after we had left, the Valour had left and taken up her position near New York and lowered a hydrophore to the SC axis. She was left to drift as she did not have a deep trawl winch and wire rope, Being near to shore, she could get Loran fixes near to the time that she received a Sofar signal, and we could work out our Sofar ranges from these Loran fixes. Atlantis was too far away to receive Loran signals. When all was ready, Newhouse started dropping bombs along NATS routes, and from planes especially assigned to this work.

We soon found out that we had a noisy hydrophore for about two hours, then a quiet one for about four hours and this pattern repeated itself each six hours. We could also observe about a one to two knot water current passing our anchored ship each period that our hydrophore was noisy. For one six hour period it would be in one direction, and in the succeeding one in the opposite direction. Nevertheless we were able to record enough of the Sofar signals for our job. The Valour drifting, was able to record nearly all the signals. Eleuthera recorded all of them. After about half our month, our anchor wire rope parted at the sheave at the bow, and we lost our ability to anchor. We notified WHOI and kept on recording Sofar Signals until the experiment was concluded. The captain and the mates did their best to take sights and fix our positions.

On our return to Woods Hole, about the middle of the fall, Ewing and I went to work calculating the fixes from those bombs where we had data from all stations.The navigator on the plane Newhouse had used provided positions of the plane for each bomb drop. When he found out that we were going to check them, he asked to rework his data and moved some of them as much as much as ten miles. He claimed his positions were good to within three miles. We sometimes found his original positions better than his revised ones. Other times, his revised ones were better.

We found our closure on positions were not as good as we expected. We decided this was because we had done all of our calculations on rhumb lines and that we should have used great circle paths. We found formulas for great circle paths, and recalculated all of our data. We found our circle of uncertainty after this correction was only about one mile. Of course, after the anchor on Atlantis parted this uncertainty, decreased the accuracy to be expected because of the degraded position data for Atlantis

In February 1946 Doc moved to Columbia University to take up his position as Associate Professor. He left me to complete the report on the triangulation. In April I went down to Columbia with all the data and Doc and I outlined the report to be written, I went back to Woods Hole and drafted the first draft of the report. In mid May I returned to Columbia and Doc and I revised the report. I went back to Woods Hole and reproduced the report and sent it to the approved distribution list.

One of the serendipitous results of this experiment resulted from the drift of the Valour. In the month she had drifted she had made a track of nearly three quarters of a circle of nearly 60 miles in diameter. Columbus decided this was because of an eddy that broke off the Gulf Stream. Several years later, he organized an experiment involving several ships that mapped out several eddies and wrote an important paper about the Gulf Stream from this work.

In wartime people get too wound up in their work just as they do in peacetime. They need to unwind. We did too, at Woods Hole.

Unwinding in Woods Hole in war time was more complicated than in most places. There was only one store with few choices where one could buy food. Falmouth was the nearest place to shop for food. Thus, food purchasing used up most of a household's gasoline ration. Relaxing had to be found without much use of a car. One way we did this was to have a cocktail party at our homes. This moved from one home to another. None of us had much money, so this occurred probably only about monthly.

Vine and I decided that we wanted to learn to sail. He sold his car, and our two families used my car. This was not too hard as we all got along well together, and the ladies coordinated their grocery shopping. We used the proceeds to buy a cat boat called the Rip. This boat was fifty years old. It had benches in the cabin, which comprised the front half of the boat. In the after cockpit, both sides were also equipped with benches. Woods Hole was a great place to sail from. The Elizabeth Islands extended, mostly southerly, separating Vineyard sound from Buzzard Bay. There were many friendly coves along both sides of the islands to explore, and beaches on which to picnic or from which to swim. Cutty Hunk had an interesting harbor to visit. There were several passages between the islands to transit. Some of them had an interesting current through them.

To the westward along the south shore of Cape Cod, there were a number of villages with harbors to visit. Vineyard sound turned eastward near Woods Hole and Martha's Vineyard formed most of the southern margin. Farther east, Nantucket continued the southern margin. Nantucket had an interesting port, and Martha's Vineyard had three. We soon found that to explore all this territory we wanted to stay overnight, So Vine and I built wood framed, canvas mattresses double bunks on each side. Since Rip was twenty feet long, and about ten feet wide, there was ample room in the cabin for this. We also built a small galley, really a shelf with cupboards underneath to store foods. We got a pressure gasoline single burner stove which we could use to heat things. A five gallon jug was our water supply. A water pail was our head. The person using the head segregated themselves in the cabin for privacy.

We had a lot of fun gunkholing (visiting the many coves and inlets) but soon found the wind unreliable in such places. We found a small two horsepower gasoline engine, similar to those used on lawn mowers, connected it to a model A Ford transmission to a propeller shaft through the skeg. We had to cut out some room in the rudder for the propeller. We made this modification on the marine railway belonging to the MBL (Marine Biological Laboratory) which had mostly gone into hibernation for the duration. We also used this marine railway for the annual bottom cleaning and painting.They allowed us to use it without charge. There was no clutch in our system, so we shifted from neutral to first gear, or reverse gear quickly to avoid grinding the gears. This improved our gunkholing a great deal. After we had a kid apiece, we took them along in their carrying baskets. These were placed on the floor between the bunks.

Later on, we wanted to improve the upwind sailing, which was poor in a cat boat with only one gaff-rigged sail, and with the mast almost as far forward as it could be located. we attached a two by six, about four feet long to the deck at the bow, ran a stay to the mast head, and another to the bow at the water line. Our ladies fashioned a small jib out of canvas, using their sewing machines, and we had a jib. This improved our upwind sailing, but we could not still hold our own against the sloops that some of our friends had. We would get to sail about once a month for a Saturday afternoon and Sunday., between about April and November. Most of the time the Vines and Worzels sailed together, Sometimes one or the other went alone. In the summer, we sometimes could take a picnic supper, and take a short sail from about five to eight . Sometimes we would do our grocery shopping at Vineyard Haven since it was only about an hours sail from Woods Hole and the grocery store was close to the dock. This used little gasoline.

In 1944 we sold Rip, which we decided was held together by water pressure, since the nails, in excess of fifty years old, must have rusted out. Rip we found got he name from an actor who had owned her when she was new. He often played the role of Rip Van Winkle in a play he frequently appeared in. Stan Poole told us of a thirty foot sloop in Chilmark Harbor, where he lived, that he thought we could buy cheaply. We went down to look at it and bought it on a Sunday. The lines were all rotted out and the sails were not much better. Nevertheless, Vine and I undertook to sail her back to 'Woods Hole. We obtained some line from a lobsterman that he was discarding and rigged Evangeline, which we soon shortened to Angie, with the minimum necessary to sail. It was a fall day, quite chilly, and the tide was against us going up Vineyard Sound. It took all afternoon and it was almost dark when we got to Woods Hole. We got quite cold as the afternoon waned, so we started a fire in the cabin in an old pail, using some of the cabin paneling for fuel. With this we could take turns getting warm in the cabin, and getting cold sailing on deck.

We outfitted Angie with all new rigging. Her gaff main sail and jib were usable and with some painting and caulking we soon had her ready. She was a Crosby sloop which had been built in the period when Crosby was changing from building catboats to building sloops. So she was about half catboat and half sloop. She had a semi-keel and a centerboard. In the intervening years the centerboard had been abandoned, but the centerboard trunk remained as an obstacle in the center of the cabin. Nevertheless, it was a big improvement in our fleet. She sailed much better into the wind, she had much more room in the cabin, we could stand up in the cabin, and she had a model A ford engine in the cockpit. We built four bunks in the cabin, and much more of a galley. Well up in the bow we fashioned some children's bunks. We each now had two children to take sailing with us. She was steered with a tiller instead of a wheel that we had used on Rip.

We soon became dissatisfied with the engine, it was nearly rusted out, so we removed it and bought an old Swedish two cylinder engine which we installed in its place. This made the cockpit roomier and used much less gas. We could now move at about four knots under power. We still only used the engine near the docks, and for some gunkholing. About a year later, we heard of a sloop that had burned in Falmouth and that her mast had burned off at the deck and had fallen into the water, thus being saved. The remainder of the mast was about twenty five feet. We bought this mast, fitted it to Angie and changed her from gaff rigged to jib-headed rigged. The main sail was easier to hoist, and she sailed much better. We had to reposition the mast and although we had made the calculations about the center of effort, we put the most about a foot too far aft, bemuse we wanted to make sure she had a weather helm i.e. if the rudder were untended she would turn upwind and stall. We didn't have enough certainty of our calculations to give her a moderate weather helm. We had the ladies, now experts, since they had made a jib for Rip, to make a mainsail for Angie. This was quite a chore and required somewhat heavier canvas. Their sewing machines were just able to handle the heavier canvas, but the ladies struggled to make such a large sail. It worked well,but we were informed that they had now retired from sail making. One time in the approaches to Chatham harbor, a gust of wind caught us and heeled the boat over about 70 degrees. Everything went flying including the ladies and children below in the cabin. Noone was hurt and we kept a closer eye on the wind afterwards. This showed, though, what a good job the ladies did on our sails.

Another time we started sailing early in the year and put into Vineyard Haven and tied up at the public dock behind a magnificent 45 foot yacht. Vine and I were working on the boat as we hadn't yet completed getting her ready for the new season. Adelaide came on deck from the cabin, followed by her daughter. Soon afterwards, Dottie and Sandy came on deck, followed by Nubie, our dog. The man on the yacht hollered over and asked how many more we had on board? We answered only two babies. He then invited us on board for a drink and regaled us with stories of his early days of sailing, and told us he envied us because he thought we were having more fun than he was.

Another time we set sail for Nantucket and the fog settled in when we were abreast of Vineyard Haven. In war time, you were supposed to come to anchor in a fog, so that traffic monitors could detect any enemy activity on the coastal waters. This was shortly after a German U-boat had landed three men on Long Island to set up a spy operation. They were quickly caught, but the fog rules resulted. We ignored them and sailed all the way to the entrance to Nantucket harbor. We were unsure how close we were, but the fog lifted when we were one mile before hitting the breakwater for the harbor. We took evasive action, and entered the harbor and anchored. Then the fog descended again. The Coast Guard did not bother us, although they must have known that we had been moving in the fog. The next day, when we left, the weather was fine and a steady wind was blowing. We set course for the sea buoy off of Vineyard Haven and lashed the sail and rudder in place. It was about fifteen miles and about three hours sailing to the buoy, and while we were only about 100 yards from the buoy, Vine and I were still uncertain which side of it we would pass. We were never able to do as well again.

Sometimes we would go to some beach as Gay Head and want to anchor the boat and have a picnic on shore. We devised a method of doing that without having to have someone swim ashore. We would drop the anchor at a safe distance offshore, feeding the anchor rode through the anchor ring, but not tying it. We would then pay out the anchor rode through the anchor ring until we could beach the boat. We'd then unload the people and the food on the beach, push the boat off and let out anchor line until the boat was riding well off the beach in deep water. we'd then drive in a stake in the beach and tie the anchor line to it. When we were done, we'd pull in the anchor line pulling the boat up to the anchor. By continuing to pull on the line the anchor would be pulled off the bottom until it was just dragging on bottom and we could pull the boat to the beach and load up again. Stan Poole, who had taught me almost all I knew about seamanship, chided me for anchoring our boat offshore and everybody going ashore. When I told him how we were doing it, he allowed he had never thought of doing it that way but he must have taught us well.

The winter seemed too long in 1942 (I think it was) so we decided to put on the "Oceanographic Follies" in February. This was a series of vaudeville acts. Some of the girls sang quite well, some people danced and so on. I read "The Shooting of Dan McGrew" and a bunch of my friends with suitable costumes and makeup acted out the parts in pantomime behind me on the stage. We all had a lot of fun doing it and we invited all of the WHOI people and all the townspeople to attend. There was no fee. I suspect the townies thought we were all idiots, but it helped break up their winter too.

One August we decided to put on a regular clam-bake. George Woollard organized it. He got a group of us to go over in the morning and dig a hole in the beach near Vine's house. We then lined it with stone, and built a fire in it. The fire was tended until 2 p.m. when it was allowed to burn down. Then we put in corn, clams and lobsters, covered it with large leaves and sand. At six we went to the beach en mass and had drinks. At seven we decided to eat and opened our pit. The corn was the only thing cooked. We were all hungry, so we got big pots and boiled the clams and lobsters. Although too late, all the food was good. Some of the people by that time could not tell whether it was or not. Then we sat around and talked and sang until midnight, when it broke up. The next morning three of us had to go over and clear up all the garbage, and refill the hole. As a clam bake it was a farce, but as a break in our routine, it was a great success.

In 1945, we again felt the winter was getting too long and so we decided we needed another "Oceanographic Follies". Again it was a series of vaudeville acts, mostly singing and dancing. There were about ten acts and George Woollard, in tuxedo, acted as Master of Ceremonies introducing the acts with a humorous introduction. While George was holding forth, I came on the stage and propped a door against the stage curtain and stood behind him swinging an axe back and forth. Just before the last act I took a crayon and drew a hook on the glass panel of the door and hung my coat on the hook and walked off the stage. Of course I had a suction cup on the inside of the collar to help me. This was also to remind everyone of the incident of my chopping down Stan Eldridge's tool cabinet door. I was also in a skit that Chuck Bodman put on. He had a dummy on a rolling table and operated on the dummy with shop tools all the time calling the dummy Joe Worzel. I was on the bottom shelf of the table hidden from the audience by a sheet. Towards the end of the skit, I opened the sheet and took picture of the audience from my perch.

I was also in the closing chorus. It consisted of about eight fellows singing sea shanties. Most of the fellows were dressed as fishermen. George Woollard was dressed in a sheet, like, a man in a book he had recently found which made a farce of scientific expeditions to a South Sea Island complete with dusky maidens dressed in grass skirts. Dot Ryder had received a grass Skirt and bra for her oldest daughter, about 12 years old , from her husband who was serving in the Navy in the Pacific. George prevailed on me to wear these, even though they were too small. We spliced some additional material in back so I could fasten them in place. I inserted two grapefruit in an appropriate place to make it authentic. During our chorus, one of the grapefruit started to escape, I reached up to replace it, and George reached over to help me. This brought down the house, Honest, we hadn't rehearsed that!

I had received the nickname of Joe at Lehigh. There was a song about 1937 that went as follows "What do you know Joe. Don' know nothin" and so on. Somehow it took the campus by storm and everybody called everybody else Joe. Even a lot of professors were Joe. It eventually died out and everyone returned back to their proper name but me. I guess Lamar was too hard for most people to remember. Anyway, I continued to be called Joe, and I was Joe Worzel for the rest of my professional life. Only after I retired to Boiling Spring Lakes and my sister refused to call me Joe and called me Lamar, did I return to my own name. Friends from my professional years still call me Joe. In several books published by colleagues I was called Joseph Worzel, although the J. in my name stood for John.

In the spring of 1946, Ewing, Vine, Woolard and I wrote a proposal to the Geological Society of America requesting a $47,750 grant to do shallow water seismic lines off Barnegat Bay and, and Cape May New Jersey, to develop the deep sea seismic method which had been interrupted by the war, and to make submarine gravity measurement along them and the Cape Henry, and Woods Hole lines where the first shallow water seismic work had been done before the war. We were granted $10,050 for shallow seismic, $16,450 to develop deep sea seismic refraction, and $6,000 for gravity measurements. This was the largest grant that GSA had made up to that date. They had the money, because they had not made any grants during the war years. We believed that we were returning to a prewar status for doing scientific research.

Doc came to Woods Hole in June 1946, bringing Frank Press, and Nelson Steenland as graduate students to do summer research. They had signed on at Columbia as graduate students working with Doc. Frank Press had just married Billie. Dorothy and I owned a hose we had bought on Millfield Street. We lived in the front house and rented a room to the Presses. Our back apartment was rented to Paul Smith and his wife.

Doc had arranged to borrow the P446 and crew from the air force. It was an air-sea rescue boat which could make about 30 knots. We arranged to use the Balanus from WHOII, which made 8 knots. Balanus became our shot boat for Shallow Seismic Refraction Work, and P 446 became our recording boat. Frank and Doc were on P 446, and Nelson and I were on Balanuus. Doc was tied up with Columbus when we were finally equipped to go to sea, so we went out to the mouth of Vineyard Sound to test out our gear, recently installed. We fired about a dozen shots at ranges of 1 to 10 miles. Frank radioed that we received good water waves, but no ground waves. We rendezvoused and Nelson and I went over to the P 446 to look at the records. I had had the most experience with sound transmission at that time. When I saw the records, I convinced the others that our geophone just had no sensitivity for low frequency waves, because the springs were too stiff. We returned to Woods Hole, We opened the geophone, removed the stiff springs, and replaced them with softer ones. The next day we went out again and were able to shoot a refraction profile. The softer springs recorded the ground waves well.

We returned to Woods Hole, and the next day Doc joined us and we started for Cape May. We left one day before the others, because of their superior speed.Throughout our campaign, we would start for our next station, the P 446 would wait several hours and start for the station, passing us on the way. They were always there before we were. During our first station, the Balanus began to leak quite badly, The Captain radioed Woods Hole saying our explosives had damaged the vessel. We were instructed to go to a shipyard in the New York area to determine the trouble and fix it. When we were hauled out, they found a knot that had dried out so that it no longer was fast in one of the bottom boards. When it was in its proper orientation, the water pressure held it in place enough to reduce leakage to a nominal amount. The explosion work had caused the knot to lodge cross wise in its hole. In that position it caused a large leak. This hole was properly sealed off and we had no more trouble with it. The captain and I had a discussion about the damage done by explosions near a ship for the rest of the time that we shot the Barngat bay and Cape May lines. He insisted that I make sure that we were further away from shots thereafter. It was unnecessary, but we did it anyway.

For part of the time Dick Edwards joined us on Balanus. We had built racks across the width of Balanus to handle depth charges since they weighed 450 lbs. That was 300 pounds of explosive, and 150 pounds of case, arming mechanism, and firing mechanism. We could roll the depth charges off the racks when we needed them. We, however carried about six more than the racks could carry, and they were lashed end up to the bulwarks. When we had exhausted our racks, we had to find a way to put the spares on the racks. Dick, who was quite strong, said, "let's just pick them up and set them on the racks". I said they were too heavy, but he convinced me we should give it a try. Dick and I rolled the spares near the racks, and lifted them into the racks. I just wasn't going to let Dick best me. About a year later, Dick joined us as a graduate student. He never finished his graduate work. I suspect it was for financial reasons.

During the summer of 1946 we completed, the Cape May, New York. and Woods Hole seismic profiles. These essentially extended the Cape May, Barnegat Bay land profiles across the continental shelf and about 100 miles beyond the edge of the shelf. In addition we shot the Woods Hole profile from the mouth of Vineyard Sound across the continental shelf and 100 miles beyond the shelf break. The explosives for this work was supplied by the Navy from excess war explosive supplies. With the Cape Henry profile that Ewing and Crary had shot in 1935 this made four profiles from the Fall Line of the coastal plain to 100 miles beyond the continental shelf break

The Columbia years

In mid September, we packed up all of our equipment and moved to New York where I had registered in graduate school. My salary at Woods Hole was terminated. That was all the help that I received. I considered that Columbus had reneged on the promises he had made to keep my salary low, throughout the war.

My father had bought a house called the Gingerbread House at Lake Mohawk and I moved my family, Dorothy, Sandy, and Howard in it for the winter. My father claimed that he got as much rent for it in summertime, as he would if he rented it out all year long, so he made it available rent free. I rented a room in the dormitory at Columbia where I stayed during the week, returning to Lake Mohawk for the weekends. My savings from the war years were now called upon. Nelson Steenland had not found any place for his family, so Dorothy and I made room for them in the Gingerbread house. We left one of our cars with the families, and we took the other one to New York each week. Nelson, too acquired a dormitory room at Caolumbia. This was a little crowded, but we got along all right. There were many G.I.'s like Steenland having trouble finding living arrangements while going to school in the New York area, so they arranged the barracks at Camp Shanks, the port of New York embarkation center for troops moving overseas during WWII, as minimal living quarters for GI's attending Colleges and Universities in the New York area. Steenlands acquired one of these from the fall of 1947 until he finished his graduate work in 1949. Somehow Doc arranged to obtain one of these apartments for Angelo Ludas and family also. Angelo was not a GI, but he had worked on the Manhattan Project at Columbia during the war.

Doc had been given two rooms in Schermerhorn Extension for his work until modifications could be made in Schermerhorn Hall for a suite of rooms, five of them with a dark room and two storage closets across the hall, for his permanent quarters. We got quite crowded since Rene Brilliant, Bill Donn, Gordon Hamilton, and Ivan Tolstoi had joined Steenland, Press, and I as his graduate students.

There soon were lists of excess government equipment circulating in the Universities. From one of these I requested a group of old Loran and Radar sets. These were not in working order,but we had the idea that research would return to the prewar routine of little money available for research. I knew that electronic gear, which had become common working equipment, would also be a major part of our research. Since we had had considerable difficulty getting parts before, I thought these discarded electronics items would furnish us with many resistors, condensers, and many other parts which we would need. These were piled into a corner of one of our rooms. We had three Russian visitors come to visit Ewing. He had a meeting to attend, so I was assigned to show them some of our work and explain what we planned. While in the room with the Radars and Lorans, one of them asked what we planned to do with them. I said we had got them so we would have a stock of spare parts. They said "We understand" in a tone that made it clear they didn't believe me. Later they again pointed to these units and asked what we were going to do with them. I gave them the same answer as before. Again they said "We understand" with doubt in their voice. I suppose they believed that we were going to attempt to use Loran in geophysical exploration.

Our new quarters were finally ready in he early spring and we gladly and quickly moved into them. One room was filled with desks, from government surplus, and became the graduate student study area. One Room was divided by a partition and became Doc's office with the outside for his secretary. The next room, a very large one became the laboratory where all of our equipment was kept and where experiments could be carried out. There was a small room that had been hollowed out of the Manhattan Schist, that underlay this ground floor, that could be reached by a trap door in the floor. We planned to make earthquake seismic observations in it. Two of the rooms were set aside for a machine shop. We had become so accustomed to build instruments during the war, that we could not conceive of doing research without a shop.

Soon Angelo Ludas was hired as our shop foreman (and total work force) to equip the shop. He made good use of the lists of government surplus and equipped our shop with a big Monarch Lathe, a small West Bend Lathe, a large boring machine, a drill press, and a milling machine. He also stocked up with many hand tools and accessories for the main machines. He was even able to acquire a good supply of metal stocks. We talked Woods Hole out of one of the 16 inch naval shells we had acquired while we were still there. Angelo and I installed this in the floor of one of the shop rooms, and Ange built a vertical cylinder to become an oil separator, next to it. We bought a hydraulic pump and a sears pump jack which we installed to pump oil into the top of the separator, pushing water out of the bottom of the separator into the shell. Thus, we had a pressure vessel for testing deep sea equipment. This was a big improvement over our earlier ones which pumped oil directly into the shell. That made testing a very messy business as the oil emulsified in the shell and covered everything in it with a slick coating of oil and water. Our new one only had water in the shell, and our testing was a lot easier and cleaner.

Our seismic vault was not a success as the traffic on Amsterdam Avenue, and the swaying of Schermerhorn Hall in the wind, about an eight story building, created too much noise on our instruments. We could do some things in it, but not most of the sensitive things we wanted to do.

We had now acquired a couple of more grad students, and the lab became cluttered with various equipment and experiments, both ongoing and finished. Finally all the grad students got together and agreed to spend Friday afternoons cleaning up the lab and dark room. One Friday we decided to remove one of the shop doors that was getting in the way. We removed the pins, and I put my hand under the door to hold it off the floor. It was an all metal door and too heavy to hold. I tried to remove my hand but was not quick enough, and the meaty end of my finger was sliced off. I went to the dispensary for repairs and I was admonished to go back to my dorm and rest. Instead I went back to the lab to work. About a half hour later we were moving a rack of shelves which just touched one of the fluorescent lights overhead. It had frosted glass light dispersers on them. One of these broke and fell slicing a gash in my forehead just above my eye. Again I went back to the dispensary for repairs. I also got a lecture. The lecture didn't do any good, as I went back to work. A few days later all the frosted glass light dispensers were replaced with plastic ones. I guess the accident did at least some good.

We moved all of our gear to Woods Hole as soon as classes were over in the summer of 1947. Wuenschel had joined our group by that time. Doc and I had arranged a cruise on the submarine USS Tusk. Doc trained Wuenschel, Hamilton and I in the use of the pendulum apparatus on that cruise which took place in June and early July. That summer we got a contract with ONR (Office of Naval Research) to make gravity observations on submarines . Doc asked me if I wanted to run it as it could give me a salary while going to grad school. I accepted forthwith. I arranged for a cruise on USS Conger in the early fall and Wuenschel made a number of crossings of the Peru- Chile trench. I arranged another cruise from Panama crossing the continental margin along the northern margin of South

America. When we got to Panama we found the squadron commander had changed the boat from the one that had been assigned to the USS Conger. The skipper was not too happy with this, as he felt they had already missed out on training exercises the other boats had participated in, which gave them an edge on advancement. Nevertheless Hamilton went on Conger and made the observations.

Doc arranged a one month cruise on Atlantis with the rest of the students while we were engaged in the submarine operations. In the fall we loaded up all of our gear and returned to Columbia. I persuaded Chuck Drake to go on a submarine cruise with me in early 1948 assuring him support for graduate school afterwards. After the training cruise on USS Argonaut he went on a long cruise in the Pacific on USS Capitaine, completing the trip in December.

During the summer of 1948 Doc took most of the rest of the grad students to the Mid-Atlantic Ridge on a cruise funded by the National Geographic. He planned to take extensive soundings on that cruise. The Submarine Signal Sounder of that time had a rotary arm with a stylus that could record either on a scale of 200 fathoms, or 2000 fathoms. Since there would be considerable depths in excess of 2000 fathoms along the way, he asked the electronics shop to modify the sounder to record at least to 3000 fms. After about a month, they came back and said there was no way to do it. Doc called me in and asked me to give it a try. I found that the outgoing signal was triggered by a micro-switch as the arm passed the 0 depth on the scale. I found a way to mount another micro-switch at a position of the arm that was just 2000 fms earlier than the normal one. With a toggle switch then you could record either on the 0 to 2000 fm. scale, or the 2000 to 4000 fm. scale. This modification was quickly copied within the oceanographic community. The Bureau of Ships even printed the directions on one of their general charts for general Navy use, even though equipment modifications were forbidden by the Bureau of Ships.

Doc planned to take ocean bottom cores adapting the Stetson Corer to use the technique the Swedes had just developed. This used a piston inside the corer that was maintained at the bottom surface while the corer pressed into the bottom. Thus, if the sediment tended to stick to the corer wall, a vacuum was created between the sediment and the piston, and the sea pressure overcame the wall friction and moved the sediment to the piston leaving room for more core. This made it possible to take cores longer than the ten feet that had previously been the maximum length. With our version of this gear we took cores, over the years, of 20 to 60 ft. in length, depending on the hardness and stickiness of the sediments. To use this technique, the core was released by a trigger about 30 feet above the bottom, and the lowering of the winch had to be stopped immediately. Doc was worried that it would be difficult to tell when they were nearing the bottom, so he asked me to develop a sounding device that would give him ample warning. He developed this concern just two weeks before sailing. I got the shop at WHOI to build a pressure case, while I put together an oscillator that used a strobotron tube to send out sound signals with a choice of 1 or five second intervals. The idea was to listen to my pinger as the corer approached the bottom. On the ships sounder the signal that started upwards would be recorded followed by the signal which had echoed from the bottom. These signals would get closer as the bottom got closer. By this method the approach to bottom could be controlled. I could not get everything completed before sailing, so I explained to one of the students how to finish it. Later I found out that it had never been used. The student was kept too busy to complete the sounder by the time they were ready to take a core. They found they could detect the bottom contact with the tension meter on the winch, so my device was never completed.

On that cruise they found a trough in the crest of the ridge on several crossings, and discovered a fault transverse to the ridge. They took many underwater pictures.

The National Geographic[Society was pleased with the results of the cruise, except for one thing. They had asked Doc to take pictures of the people operating the various equipment. Since most of the gear was going over the side of the ship, most of the pictures prominently showed mostly fannies. This was not up to the NGS standards.

Doc was pleased with the soundings, but found several features that needed to improve. One was the distortion because the soundings were made on the circular path of the rotating arm. These would have to be made on a vertical rectilinear path to make a really good record. On the records just taken, all of the soundings had to be read off the record and replotted, since the soundings near 2000 fms would vary back and forth, the switching of the fathometer from one scale to the other was tedious and sometimes even difficult. He wanted this to be automatic and in some way not to have to be repeatedly shifting when the soundings were near the transfer depth. They also found that the echo timing was controlled by the frequency fed to the sounder. Since this frequency was from a generator in the engine room which wandered depending on the electrical load. The engineers would correct the frequency from time to time. When they were busy, this was neglected for various periods of time. He got King Couper at the Bureau of Ships to have a sounder made with a rectilinear recording system designed. This qas adopted by the Navy. This system also could record over the full range of depths of the ocean, 6000 fathoms. By having successive depth ranges that had overlaps, the rapid switching back and forth at the depth where the scales changed was avoided. Somehow they decided to change to 12 kilohertz from the 20 kilohertz formerly used.

When Doc had decided to take ocean bottom cores, Columbus cautioned him not to take too many as it was so expensive to analyze them. Doc responded that he would quit when two cores were just alike. This never happened and Doc started a regime of taking frequent cores in all our sea work. We avoided the overwhelming expenses by using the cores as a resource for particular studies, that someone was personally interested in, rather than to try to study everything possible from each core. This was the start of the core library we established later at Lamont, and continues to this day. The cores have been studied repeatedly as each new idea or technique evolved. At least one core has been completely used up by this repeated sampling from it. Scientists from all over the world have come or sent for samples from individual cores.

The Stetson corer was hard to use, The wire rope from the ship ended at the core release. A second wire rope was attached to the core release which went to the piston inside the corer. This wire had to be changed each time you wanted to change the free fall of the corer, or the length of the core pipe. Doc asked Angelo to design a way that the wire rope from the ship could pass through the core release, attaching the core release to it, and allowing the appropriate length of the wire rope for the core length, and the free fall distance so that it could be attached directly to the piston. Attaching the release to the wire rope with a wire rope clamp would cause a kink in the wire rope because of the 1000 pound weight of the corer. Ange came to me and asked for help to design a method of attaching the core release to the wire without causing a crimp. I suggested using a device similar to parallel rules used in navigation. If the wire rope were constrained within the two parallel bars, and the corer were attached to one of the bars the weight of the corer could be distributed over whatever length you wanted to make the parallel bars. The more weight applied to one bar the harder the parallel bars would clamp on the wire rope. By distributing the weight to the length of the bars would prevent crimping the wire rope. Ange took this idea and constructed the release to have a parallel bar arrangement through which the wire rope could pass.

When in use, the wire rope was passed through the gripper- release directly to the piston inside the core tube. Whatever amount of wire rope you needed to reach bottom could be left between the release and the piston. When it hit bottom, the release would let the corer drop, the winch was stopped, the corer would free-fall to the bottom. If you calculated correctly, the piston would be arrested at the bottom and the corer would continue into the bottom. The corer would pass by the piston as it took a core, until the outside friction on the core tube stopped penetration. If the core on the inside of the core tube stuck to the wall it would create a vacuum, above the sediment and the sediment would be maintained at the piston by the outside pressure, thus allowing room for additional care at the bottom off the tube. When the wire rope was recovered at the ship,a stop was made when the release reached the ship, the release was lifted releasing its grip on the wire rope. The release was supported from the ship, and the wire rope was pulled up through it until the core reached the ship. This made the coring easy. The only difficult part was to watch the tension gauge to detect when the release freed the weight of the corer from the wire rope. After some experience we became quite expert at that task.

In the summer of 1948, the Atlantis could not be made available to us so we chartered the Kevin Moran and Doc took must of the grad students back to the ridge. They had the new core rig to use and they were able to take some explosives to attempt to take seismic reflection records. Once an hour they took an explosive reflection record. At the end of the cruise they could no find any way to interpret the reflection records as all the sub-bottom reflectors were at different places. It was decided that the bottom reflectors varied too much in the hour between records. The National Geographic Society sent out a photographer to photograph the operations. They were much more satisfied with the results than from the previous year.

They made a thorough examination of the fracture zone across the ridge, that had been discovered the previous year. There could be no doubt that it was a fault crossing the ridge. Photographs of the valley in the peak of the ridge showed an abundance of pillow lava. The abundance of animal life on the bottom was evident in the photos of the ridge as well as in the basin crossings on the trip to the ridge and back.

Sand layers were found in some cores. Some sand had been found in a dredge on Challenger expedition, but since it was thought that sand could not be transported to the ocean basins, it was thought that it must have been left from a previous shallow water use of the dredge, i.e. the dredge hadn't been cleaned well enough. This excuse could not be used in this case, and there was a problem. How did sand get to the bottom of an ocean basin?

While they were gone to the ridge, I continued arranging and staffing gravity cruises on submarines. I was also busy setting up the computation technique for the reduction of the gravity data. This required 84 computations and thirty readings from the records. Doc had signed on two graduate students, Heezen and Northrop, too late to make the ridge cruise. They were left to me to arrange cruises for them to photograph the bottom along the continental shelf. I arranged to use the Asterias, a thirty foot motor launch belonging to WHOI for this purpose. They did a good job and extensive coverage of the continental shelf was acquired.

There is little about the school year that sticks out in my memory except the annual Christmas party. All of the graduate department students got together to set the format of the party. It was decided to make it like a scientific society meeting with each group of students presenting a paper, to be humorous, in their area of specialization. These were also supposed to make gentle fun of each principal professor. At the end, there was to be a presentation of a funny present from each group to their principal professor. At that time, there had appeared the results of a study showing that pigeons navigated with help from the magnetic field. Our group of students got together and wrote a paper that purported that Dr. Ewing obtained pigeons and trained them to fly around particular magnetic anomalies. He was supposed to have them trained to fly round and round each anomaly. In that way, the anomaly would be marked out by a circle of white on the ground. A well drilling rig would be brought in and set up to drill on the target.

For the present, we caught a pigeon, enclosed it in a shoe box, and wrapped it up just before the party. When Doc opened the box, the pigeon flew out and perched on a protuberance near one top corner of the room. Everyone in the room was startled by the pigeons flight and the wing noise! We had anticipated this result and had tied a long piece of ribbon to one leg, so that we could recover the pigeon and release him. Press, Steenland, and I had caught the pigeon on the campus where they were numerous. Nevertheless, the pigeons were wary and it took us a number of tries to capture one. People passing on the campus gave us our share (for at least a year) of dirty looks.

We had applied for and received a grant from ONR to make seismic refraction measurements on the continental shelf, to extend our previous work. Doc was away, I don't remember where, but he had arranged for Press and I to take the other graduate students out on Atlantis for our 1949 cruise. We made seismic refraction measurements off Nova Scotia, and the Grand Banks. When we attempted to take a deep sea core, we were thwarted by the winch wire rope. It was too short. WHOI knew we wanted to take more cores, but had not told us about the short wire rope. Unhappily we had to content ourselves with ocean bottom photographs.

In 1948 Doc and I were asked to set up a Sofar research station. We could not use the station at Eleuthera, that we had abandoned with the termination of war research. It had been returned to its owners who wanted to set up a tourist operation on the grounds. Anyway, in peacetime, getting to Eleuthera would be difficult and expensive. We decided that Bermuda would be easy to reach.

In February we got an opportunity to use Atlantis and Caryn on a two ship operation. We decided to attempt a deep sea refraction measurement from the sea surface, using some of the techniques we had learned during the war. Doc couldn't go, so I was made chief scientist on Atlantis with Hamilton to assist. Hersey was made chief scientist on Caryn with Press to assist. When we reached deep water, Caryn deployed a couple of hydrophones and Atlantis started away firing charges. Things were going quite well, when the weather changed and a storm hit us. Caryn was blown downwind under bare poles. Her engines were not strong enough to keep her headed into the stormy seas. We followed keeping track of Caryn with our radar. It was tough, but we were all worried about the safety of Caryn and extraordinary efforts to keep track of her on the radar succeeded. The radar we had in those days was pretty rudimentary. Eventually the storm passed, and we set up again to attempt the refraction measurements. Caryn again set up to receive the sounds, and we on Atlantis moved away firing various size charges as the range opened to 35 miles. The largest charges were depth charges, 300 pounds of TNT. Then Caryn moved toward our final shot position and set up again to receive the sounds. We, on Atlantis moved out, firing, on the reciprocal course to the one we had followed previously.

Again, the charge size varied as the distance increased until we felt the charges were not big enough for recording ground waves. This turned out to be the first reversed deep sea refraction station completed in the deep ocean. At its completion, Caryn returned to Woods Hole, and Atlantis went into Bermuda to receive a new complement of scientists to continue its research cruise. This was the prototype reversed refraction seismic station which became our primary objective at sea for many years.

Ham and I stayed in Bermuda a few days looking over the various locations that were available, near to deep water, where we might set up a Sofar station. Dressed in our work clothes, on our last day in Bermuda, we returned to the Bermuda Biological Station where we had been staying. We found J.W. Smith, the ONR representative who was pushing the Sofar project and a couple of U.S. Navy Captains waiting for us. They wanted to know what we had found. We went out on the porch, unrolled our chart of the area and placed it on the table. The wind started blowing the chart, so I placed an ashtray on one corner, and pulled out my sheath knife that I always carried while working, and threw it on the table to keep a second corner down. We showed them where all the locations we had looked at were on the chart, and discussed the pros and cons of each. We indicated the one we thought was best for our purpose.

When we returned to New York we found out that the story was being passed around Washington that when an important ONR committee had visited us in Bermuda, the chart that we were using started to billow, this man, who looked like a pirate, pulled out a knife and pinned the chart to the table.

Nevertheless, our choice was taken and Ham went to Bermuda to set up the Sofar station. The former St. David's Lighthouse became the primary building of the Sofar station. By the end of 1949, Ham had a group in Bermuda who had set up a laboratory, established a geophone at 800 fathoms connected to the lab at St. David's. Before setting the geophone, Ham heard of a superior hydrophone which had been developed at the Naval Research Laboratory (NRL). When he went to inquire about it he found it required about twenty vacuum tubes right near to the hydrophone to obtain excellent low frequency response. He told the NRL representative, that Dr. Ewing would never allow such a device to be placed on the ocean bottom because of the high probability of its early failure. The NRL man said, "Oh well, all those old-fashioned guys would soon die off". The equipment we installed was still working when the station was finally abandoned about 1990.

Ham immediately started monitoring all the ocean sounds and started setting up various experiments at the station. Outside of whale calls and earthquakes, the interference from natural sounds was minimal.

By the summer of 1948 we were getting too big for our quarters. We still were adding graduate students and our lab space was inadequate. We requested more space without success. We, in desperation, designed a new building we thought could be built on the lower campus, between Schermerhorn Extension and the power house. About then, Dr. Ewing received an inquiry from MIT (Massachusetts Institute of Technology) about whether he would be willing to come there and establish a geophysics operation, bringing all of his students with him. Dr. Ewing agreed to make a trip to MIT to discuss it with them . He went to MIT and took with him four senior graduate students. The ones he took took were Frank Press, Nelson Steenland, Gordon Hamilton , and myself. They showed us around the Geology Department, discussed with us how we would fit in as an appendage of the Geology Department, and then sent us down to see the Hetty Green Estate. A few years previously they had acquired it as a gift. It was located near New Bedford on Buzzards Bay. They were then only using it for the Van der Graf Generator experiments. There was plenty of room on the estate and we were assured that necessary buildings and a pier on Buzzards Bay could be built where we could dock vessels as required. The only problem was that it was about 60 miles from the Boston Campus. It would involve lots of travel between Boston and New Bedford as the educational part of the work would have to take place in Boston and the research part would have to take place on the Hetty Green estate.

When we returned to Columbia, President Eisenhower, who was President of the University and Professor Kerr, who was the Chairman of the Department of the Geology, had a talk with Dr. Ewing. They countered MIT's proposal by offering the Lamont Estate in Palisades, N.Y. Columbia would accept the gift of the Lamont Estate, which would provide plenty of space for our group, if the Geology Department would raise enough money to operate it for at least three years. It was to be understood that it could not become a financial burden to the University. It was estimated that it would require $100,000 for those first three years. Paul Kerr persuaded five mining companies to provide $20,000 apiece to establish the Lamont Geological Observatory.

When we went to the Lamont Estate to look at it, we found that it consisted of nearly 120 acres of land on the top of the Palisades overlooking the Hudson River. Most of it was in New York, but about ten acres extended into New Jersey. It was about 20 miles from the Columbia Campus. At that time it was still furnished as the Lamont's had lived in it. It was lavishly furnished with furniture. Most of the rooms had fireplaces with marvelous marble facades, and the dining room had a magnificent chandelier of crystal. The living room later became our seminar room, the dining room our core laboratory, the kitchen complex, our geochemistry function, and the bedrooms on the second and third floors became offices for the scientific staff, our business office in the front hall.

Dr. Ewing convened the four of us and put the question "should we accept the offer of MIT or remain at Columbia with the Lamont Estate for our laboratory operations?" We debated the difficulties of operating our research from the base 60 miles from our educational operations in Boston at MIT, with the difficulties of using the Lamont Estate only 20 miles from the Columbia Campus. In addition the MIT offer would require to uproot all of our activities and equipment and move to the Boston area. It appeared that MIT would offer more support from the University than we could expect at Columbia. Finally Doc put it to a vote and it was unanimously agreed that we should stay at Columbia and use the Lamont Estate.

The Lamont Geological Observatory years,

In December 1949 President Eisenhower received the deed of the Lamont property from Mrs. Lamont. Shortly afterwards, Frank Press and I set up a seismometer on the floor of the empty swimming pool which had been carved out of the Palisades diabase. There was a crack in the pool so that it would no longer hold water. On our return, a few days later we found a notice pinned to the door stating that the place was not zoned for work and that all work on the place must cease. This led to several town meetings. The problem was that they expected us to bring in a number of school age children and the local school felt they would have financial difficulty in accommodating them, especially since the Lamont Estate had been the major taxpayer in the town of Palisades, N.Y.

Columbia University being tax exempt would place a big burden on this small school unit. A compromise was made for us to pay $20,000 a year in lieu of taxes and we got our work permit. We knew we could have prevailed in court without any payment, but it seemed reasonable to pay as we expected to have 8 or 10 students to add to the local school. It is interesting that in about 1975, when Lamont-Doherty had only a couple of students in the school the town decided this payment was no longer adequate and it should be increased significantly. This time Columbia went to court and the town not only did not get an increase, it received no payment!

As soon as possible Doc moved on to the grounds in the former gardener's cottage. I left the dorm and started sleeping on the third floor of Lamont Hall. The Lamont's had removed almost all of the furniture, and the fancy fireplace facades replacing them with simple slate facades. After my kids completed their school terms in Sparta, our family moved into the two guest rooms adjoining the swimming pool.

Dave Ericsson moved into one of the bedrooms and Mrs. Smith, who stayed on, gave him his breakfast. Dave turned the dining room into the core laboratory. Larry Kulp set up the geochemistry lab, which at that time was mostly carbon 14 dating, in the kitchen complex. Doc soon moved his office into the former second floor sitting room with his secretary adjacent in what had been Mrs. Lamont's bedroom. Doc set up a workroom for his charts and overflow from his office in Mr. Lamont's bedroom. Most of the grad students moved into the bedrooms on the second and third floor.

The grounds superintendent had stayed on, but he was drunk most of the time, so we had to let him go. He stayed in the area awhile because he knew we would have to shut off the water for the grounds for the winter. He thought we would have to rehire him as he was the only one who knew where the shutoff valves were. However we fooled him. I had found plans of the estate including all of the water system, which had the shutoff valve locations on them. We were able to take care of the water system without help. I had moved my family into the barn helpers house on the hill east of the present machine shop before cold weather set in. This gave us more room, but we were still cramped. The Presses moved into the cottage that had been the barn keepers. When we got rid of the grounds man, I was allowed to move my family into the house he had lived in, across the courtyard from Dr. Ewing and family, and Angelo Ludas moved into the house on the hill.

In the 1940's SubDevGru 2 (Submarine Development Group 2) was formed by the U.S. Navy to establish the means for our submarines to attack enemy submarines. A similar group was also set up in the Pacific.

In February 1950, Capt. Roy Benson, ComSubGru 2, called a meeting inviting people who had an active part in underwater sound research during WWII to attend. About 50 people from government laboratories, civilian laboratories, such as Woods Hole Oceanographic Institution, and people who had left these laboratories for the academic world, attended.

At that time, I had just received my Ph. D degree at Columbia University. Dr. Ewing and I were invited. Dr. Ewing was unable to attend, but I did.

The meeting was held for two days at the New London Submarine Base. Capt. Roy Benson started the meeting by explaining that CNO (Chief of Naval Operations) had set up the development groups as Submarine Hunter Killer groups. SubDevGru 2 had been operating for about a year and had concentrated on the means to destroy the enemy submarines. Satisfied that if they could find an enemy submarine they could destroy it, they were now concerned about locating enemy submarines operating at sea, mostly in deep water. The techniques used in WWII, not by then much advanced, could only locate a submarine at ranges of the order of 2 miles or less. This would be unsatisfactory for a hunter killer group, and they were searching for any way to increase these ranges significantly. He asked the participants for help.

For the rest of the two days various people spoke on their ideas on this subject. There was one interruption of about an hour to take the group on board a fleet type submarine, attached to SubGru 2, since many in attendance had never been aboard a submarine. This happened to be a very chilly day in late winter. I was familiar with fleet type submarines, since my graduate students and I had been using them for making gravity measurements at sea for about 3 years. I realized what a flail it would be to remove coats and store them, and later recover them. I decided to make the 10 minute walk outside without my overcoat. This was noted by the staff of SubDevGru 2 and other participants, and I later learned that there had been a number of comments about that crazy guy from Columbia University.

As the meeting was drawing to a close, Captain Benson got up and said that he had heard from everybody in the room but me and he wondered what I had to say. I responded that all I had heard at the meeting seemed to me as if all the suggestions together might increase the detection ranges by 10 to 20%, but would not be enough for the desired purpose. I then continued that Dr. Ewing and I had written a paper during WWII pointing out that low frequency sound traveled much greater distances in the ocean than high frequency sounds. This had been confirmed by several experiments we had conducted and reported, but no one had made use of this fact. I further said that passive listening ships during the war had reported hearing the low frequency sounds from convoys at ranges of more than 100 miles. These reports had been dismissed as improbable by the authorities and nothing had been done with the information. I then took a gamble and stated that I thought that the use of low frequencies could expand detection ranges several 10's of miles.

I then pointed out that the background noise level in the ocean was approximately proportional to the band width and that restricting the sound to a very narrow band where a submarine made a loud noise would give a much better signal to noise level and consequently extend the detection range. I was asked if such low frequency equipment existed. I replied that we had just established a station at Bermuda with a geophone planted on bottom in 800 fathoms of water that was capable of operating down to 20 hertz. These remarks seemed to stun the audience and the meeting was adjourned. I returned to New York.

In January 1950 I had taken a part-time job at ONR as assistant to the geophysicist (Beau Perkins) in the geophysics branch. It came about this way. Beau Perkins called me in and asked me to come in and help. I wanted to stay at Lamont. He told me that unless I helped it was unlikely there would be any more ONR contracts at Lamont. I talked to Doc and told him the situation. He suggested that I try to accept a part-time situation at ONR. It was finally agreed that I would become a consultant to ONR spending alternate weeks at ONR and Lamont. On my second week at ONR, I found Beau Perkins had left ONR and I had the whole job to do on alternate weeks, while they sought a replacement for Beau.

While I was in Washington I stayed at the Gordon Hotel. This was a very inexpensive hotel just two blocks from the White House. My rent was $3 per night. Nearby hotels in Washington went for about $15 per night at that time. The Gordon had been Dolly Madison's home that had been turned into about half residence and half transients hotel. Dr. Ewing and George Woolard had first found the Gordon. They introduced me to it. Pretty soon all of the students in geophysics were staying there when we went to the AGU meetings. It was convenient, in a nice neighborhood and had large high ceilinged rooms. The large rooms often made it possible for six or more of us to stay in one room with even good division of the rent the rent. Later on the Wisconsin contingent also stayed thee too. It was almost like a separate AGU meeting.

After the meeting in New London, I had spent the following week in Washington. On my return to Lamont in the succeeding week, I received a call from Captain Benson saying he would like to take me up on my suggestion of using low frequencies; would I write him a proposal of how to proceed. After some thought, I wrote a proposal about setting a short tethered buoy above our hydrophone in Bermuda. Then having a snorkel submarine to circle the buoy at a radius of one mile while we would observe the sound level in narrow bands at frequencies between 20 and 300 hertz. After discovering a frequency where the submarine made the greatest noise, we would send the submarine on range runs to see how far away we could still detect them. Another two weeks passed, and I received another call from Captain Benson stating that they would like to undertake this experiment. He stated that he would send a fleet type submarine, and a snorkel boat to Bermuda in mid May if I could get ready by then. I agreed to do it, although I pointed out the difficulties I would have with my schedule of alternate weeks in Washington and Palisades.

I dropped everything at Lamont and collected together the material to set a short tethered buoy. I already had a narrow band low frequency sound level meter. However, I worried that it could not do the job adequately so I bought a frequency analyzer that could be used on many frequency ranges, that analyzed the frequency versus amplitude over a range of a couple of hundred cycles presenting the results on a cathode ray tube. I fitted this device with a camera so that a record could be made of each frequency analysis. I sent the equipment to Ham in Bermuda.

I went to Bermuda during the week before the subs were to arrive. With some of Ham's group and the buoy boat and a great deal of finagling we set the buoy with such a tether that it could not wander on the surface more than a radius of 10 feet. The submarines arrived in Bermuda on schedule with Capt. Benson, his executive officer, Commander Ozzie Lynch, and his research officer, Lieutenant Commander Harmon Sherry. We set up a schedule using the boats each on an alternate day. That way, the boat on standby could give their crew liberty while we were working with the other. The boat for each day was to sail from St. Georges harbor at sunrise arriving in the vicinity of our buoy within an hour. Operations would continue until about one hour, before sunset, when the boat would return to St. Georges harbor for the night. I had made arrangements for Capt. Benson, his staff and myself for accommodations and meals at the Bermuda Biological Station. After a couple of days, when it had taken us two hours to travel back to the BBS, have lunch and return to the Sofar station, Capt. Benson had the subs provide us with bread and baloney and we ate our lunch while continuing our work. Ham had adequate facilities to provide us with coffee. Ham and his group continued with their own work except they were denied the use of the deep geophone, which we were using.

We soon set up a routine that as the boat would circle our buoy at a range of 1 mile, I would make observations of the sound intensity in steps of about 5 hertz in the frequency range of 20 to 300 hertz. The band was 1% of the frequency to which we were set. Cmdr. Sherry would record the readings on a data sheet, and as well as he could would plot them on a graph paper. The readings came too fast to keep up, but he plotted as many as he could. After supper at the Bio Station, we would catch up on the plotting, discuss the results, and make plans for any deviations of plan for the next day. We normally got to bed about midnight. We got up about 5:30 the next day to start a new days operations. Photographs of the cathode ray frequency analyzer were made for each test.

We made frequency plots of the fleet type boat on the surface, running on diesel power, and submerged at periscope depth, and later at 100 foot depth. These were observed at full speed, half speed, and minimum speed. Similar tests were made of the snorkel boat, but adding a series of runs while snorkeling. We organized the change from one set of conditions to the next by radio communication between the station and the boat. Cmdr. Lynch would pass my request for a change of test to the boat. After a bit, Capt. Benson asked me to knock off the formal request to have the change of tests, and merely tell them what I wanted next.

After a couple of days of operations, the buoy broke loose during the night and disappeared. It looked like we would have to suspend operations to set another buoy. The submarine commander when he learned of our dilemma convinced us that he had sufficient data that he could make the circles without the buoy. We tried it and it worked. The other boat was similarly able to continue our operations without the buoy.

We never did use the results of the frequency analyzer, considering it a backup, because of the delay in developing the film and making the required prints so they could be used. It was too cumbersome and slow, especially as the results from the General Radio sound analyzer was producing satisfactory results, and it used a narrow frequency band.

Towards the end of the week, Dr. Ewing arrived ostensibly to read earthquake seismograms made at the St. Georges station. In reality, I had asked Dr. Ewing to come to bail me out if I was having trouble. Things were going smoothly and he did not have to. He did get me aside and told me that I shouldn't be telling the Navy brass what to do, but should be making very polite requests. I told him that Capt. Benson had asked me to knock off the politeness and to just tell them what to do so the work could be done expeditiously. He returned to New York after a couple of days, since things were going quite well.

After the boats had made a large number of circles, and we had made a lot of measurements, we could all see that the surfaced boats and the snorkeling boat had a significant noise peak at about 100 hertz. Since, at that time, it was considered that a boat on patrol or en route to a patrol area would be proceeding on the surface, or snorkeling most of the time, we decided to,arrange our range runs in these modes.

The last two days were employed making range runs, first with the snorkeling boat at full speed, and the next day with the fleet type surfaced at full speed. Capt. Benson arranged for them to stop their engines on a schedule known to him but kept from us. This was to make certain that we were not deluding ourselves. We were able to follow each boat to ranges of the order of 100 miles. We all considered this a breakthrough since we had increased detection ranges by a factor of about 50.

The snorkeling boat was detached and sent to New London after we had concluded their range run. The fleet boat returned to Bermuda at the end of their ranger run. Cmdr. Sherry and I returned to New London on it, with all of the data. We worked on the report on the trip home.

Since I had missed one week in Washington because of these tests. After I spent the weekend at home, I went to Washington the following week. I reported our results to the Chief of Naval Research and to Manny Piore, his chief scientist. I worked that week on only those matters that could not wait, spending the rest of my time organizing the report of our results, and writing the narrative explaining the operations and interpreting the results. I completed this work in the following weekend at Lamont, and returned to New London Sunday Night as I had agreed with Capt. Benson.

Capt. Benson turned out the whole staff to work up the report. We had a yeoman typing up the stencils of the narrative, while I proofread them. Other yeoman were drafting fair copies of the graphs. Two other yeoman were mimeographing everything as quickly as they were finished, and another was collating the copies. The report was classified as secret. Capt. Benson designed the cover page which was stenciled on heavy yellow paper. When the copies were completed, they were stapled together and copies sent to the appropriate addressees. I remember especially that copies were sent to the Chief of Naval Operations, Com Sub Lant, Com Sub Gru 1, various Navy Laboratories, and certain civilian labs. By the end of that week the report was completed and distributed. This was only four weeks after we started to work with the boats at Bermuda. I don't know of any other report that was researched and reported that quickly.

About two weeks later Com Sub Dev Gru 2 called a meeting at Columbia University. This was to communicate these results to all of those people active in the underwater sound community. I probably made the greatest mistake of my career in insisting that Commander Sherry make the presentation instead of doing it myself. This prevented most of the people knowing that it was principally my ideas and execution.

Shortly after this meeting, Bell Labs was chosen to develop suitable equipment for fleet use of this program. The Office of Naval Research funded Columbia University to pursue the development of equipment and to further the development of low frequency sound for many purposes. James W. Smith of ONR's Special Projects Branch approached me to become director of Hudson Labs as it was to be known. I declined stating that there were many people capable of pursuing the idea as well as I could. I thought people like me should be kept working on new ideas. Ultimately Gene Booth, a physicist, was chosen as the Lab Director, and Jack Nafe was chosen as Director of Research. I served as a consultant to them for a year.

Developments were quick to follow; and the Navy soon set up the Sosus system of ocean bottom detectors to monitor the oceans ship traffic. Hudson Labs grew to employ about 300 people and became useful to the Navy on other projects too.

The peak 100 hertz submarine signature was quickly identified as the firing rate of the cylinders of the diesel engines used in the diesel-electric drive. Other noise peaks were tracked down to pumps and other machinery on the sub. Soon urgent steps were taken to eliminate or minimize the sources of these sounds of submarines by equipment improvements and vibration isolation systems. Our submarines are now much quieter. There are probably only one or two people left who know that I had anything to do with this work.

While I am on the subject of SubDevGru 2 I will continue with my association. I am not sure of the dates that the following happened.

I guess that because of the previous success, ComSubGru 2 asked if I had any ideas about how a submerged submarine could navigate, as they anticipated that Radar would deny them a chance to surface if they wanted to keep themselves undetected. I replied that I knew of two ways. The first was to use pendulums to measure gravity on submerged submarines. Since gravity is in large part a function of latitude, that with a latitude and dead reckoning they could get a reasonable location. This would be especially true if they were keeping frequent gravity measurements. Furthermore I pointed out that specific features prominent in gravity data, such as sea mounts and many other features could be used as "lighthouses". They could upgrade their navigation by visiting such features every few days. I also pointed out that we had been charting an abundance of such features widely spread about the oceans of the world.

The second method I proposed was to reverse the Sofar system. In the Sofar system, an impulsive event was located by hydrophones fixed at known positions in the ocean. Triangulation then determined the position of the event. We had demonstrated this system while we were still at WHOI in 1945. With this system we were able to locate a series of explosions fired by a ship in transit more accurately than the ship could determine its position by normal celestial navigation.

We later called the reversed Sofar system Rafos, Sofar spelled backwards. The plan would be to set up three stations well separated to make impulsive signals at a precisely determined time. A submarine could receive these signals and with triangulation determine their position.

Com Sub Dev Gru 2 decided to set up experiments to test these two systems, and I was chosen to plan them. We first set up the gravity measurement system.

We decided to make three transects in the Gulf Of Maine measuring gravity at 6 hour intervals. We would use the Latitude system as a trial navigation. Later we would contour the gravity results, find some features that could be used as "lighthouses". A second cruise in the Gulf of Maine would use these l "lighthouses" as navigation fixed points. The experiment went well and the results were fairly satisfactory, but the lengthy reduction of the gravity data was considered a "difficulty".

We then set up a sequence of Rafos experiments. First we hired a small vessel to position itself in deep water SW of New York. It would fire small explosive charges at fixed times at the axis of the Sound Channel. I equipped the submarine with hydrophones to be positioned at 1000, 2000, 3000, and 4000 feet. We also used a hull mounted hydrophone. After the hydrophones were deployed after dawn, the submarine made a stationary dive to periscope depth. The explosive sounds from the shooting ship, which moved around in the ocean, were recorded throughout the day. Just before dark, we would make a stationary surface, and recover the hydrophones.

We did this for a week, while the shooting ship moved to numerous positions. The handling of the hydrophones presented a problem. The over side hydrophones were attached to demolition cable. A wooden box was built at the sub base which had numerous holes drilled in it so that it was free flooding. This box was mounted behind the bridge on U.S.S.Toro. The hydrophone cables were fed into the box, with the shortest at the bottom, the next longest above it and so on. As long as the demolition cables were not moved in the box, the cables could be payed out without getting any tangles. When they were at their appropriate depths beneath the submarine, each one was tied off at the rail along the cigarette deck behind the conning tower. They were tied about 10 feet apart. We had no problems of getting anything tangled despite the fact four long cables close to each other were dangling beneath the sub. Since recovering the cables would be a chore, I had the Lamont shop make an electric motor , with a long extension cord, connected to a gear reduction box which in turn was connected to a gypsy head. The motor could be removed and taken below when we submerged. This winch could be used to recover the cables after we surfaced. The motor could be brought on deck, and attached to the winch with a chain to keep it in its proper position...

We determined that all the hydrophones , including the hull mounted one, could detect the Rafos signals from one half pound charges fired at ranges of at least 1000 miles, at the sound channel axis .

Having determined that the signals could be monitored without the necessity of deeper hydrophones, We designed another experiment. I chartered three vessels, equipped them to fire one half pound charges at the sound channel axis at 3 hour intervals at the precise second of the hour. One vessel was located south of Cape Cod. A second one was located just beyond the Gulf Stream off New York. and the third just beyond the Gulf Stream off Cape Hatteras.These positions were chosen since they were within range of the Loran navigation system so the vessels could maintain a nearly fixed position. The submarine carried out operations between Bermuda and the mainland in deep water, recording fixes from the Rafos beacons. The submarine would surface as required to obtain Loran fixes, to check the accuracy of the submerged Rafos fixes, as required.

The conclusion of the experiment was that the Rafos fixes were at least as accurate as the Loran fixes. The Loran system in operation at the time was called Loran A as Loran C had not yet become operational.

Com Sub Dev Gru 2, Captain Hydeman, who had replaced Captain Benson by this time, decided the Rafos system was more satisfactory for their purposes and carried out an experiment with the submarine crew determining the fixes. He also had Navy ships providing the explosive sounds. Unfortunately, the Navy ships had many misfires so it didn't work out too well. He next requested that we furnish firing crews to the Navy ships to carry out the next try. This worked out successfully.

At about that time, a secret Navy conference was called in Washington about the Hunter -Killer concept. Various papers were given about different aspects of the program. Captain Hydeman made a presentation that said that Com Sub Gru 2 would use narrow band low frequency sound to detect an enemy submarine and that he would navigate his submarines using Rafos.

The Navy thereafter further developed these systems for their use, and I returned to my regular program of research.

Subsequently the Navy deployed the Sosus system using fixed stations off islands and continental coasts for an ocean wide surveillance system. Sometimes arrays of hydrophones were floated well above the ocean floor into the axis of the sound channel. All stations were provided with narrow band low frequency monitoring equipment. Each ship had a different frequency signature. This was true of ships that were ostensibly identical, because of slight differences of installed equipment and modifications made during their working life. Naturally these modifications were not actually identical.

From here on the time sequence of various narrative elements are only approximate since so many things were going on and I did not keep a diary.

About 1949, since Atlantis was not available for us, we obtained two forty foot launches from the Navy as government furnished property. We believed we could do refraction profiles near shore with them when we could not get a deep sea vessel.While we were getting them ready for sea, by scraping and painting the bottoms, and painting the bulwarks, one of the shipyard workers stood and watched us for a while, then asked if that is what we had gone to school so many years to learn to do. We replied that when you had too little financing you had to replace financing with labor.

When they were nearly ready for sea, I was approached by the builders of the Thruway bridge to see if we could help them find out about possible rock layers beneath the Hudson River on which they could anchor the bridge structure. They had made drill holes of 50 to 100 feet deep along the proposed path of the bridge and had only found solid rock in the hole nearest the eastern shore. They were concerned that the rock might only be a little deeper than their drill holes, but the drilling was so expensive they wanted a cheaper way to find out. I responded that seismic refraction could give a cross section. We could do it if ONR would permit us to use the two motor launches for the purpose and could furnish the explosives from the excess quantities the Navy had left over from the war.

I contacted our contracting officer and convinced him that a cross section across the Hudson River near Nyack would be an important geological investigation and that the Thruway contractor would then pay for the expense of installing the seismic gear in the vessels and getting all the radios and other gear for seismic gear operational. They agreed this would be a good shakedown cruise for us and to supply the explosives from Navy excesses.

Chuck Drake, Jack Oliver, and Walter Beckman, all graduate students agreed to join in on the project. We set up the Picket Boat as the receiving boat with Walt Beckman as the ship operator and Jack Oliver as the seismic recorder, and the Torpedo Retriever, with Chuck Drake and I sharing as operators and shot firing crew. We planted the geophones in the bottom in the shallow part of the river, and used hydrophones in the deeper channel (about 50 feet deep). Hereafter we will call these two vessels Picket and Retriever, as we did in our operations.

We made continuous end to end, and reversed profiles along the whole planned track of the bridge, and made two section at right angles to the proposed bridge track extending about 5 miles north and south of the track. These latter were to make sure that the bottom structure did not change radically nearby. Our shooting was noticed, even though we only had to use small charges, and the Herald Tribune contacted us to find out what we were doing. They then asked to come along one day to obtain a story. We agreed and took them with us on the retriever. After watching the plume of spray rise above the water from our shots, they asked if we ever killed any fish. I replied no, that sometimes we would stun some, but they soon recovered and swam off. The very next shot must have gone off in a school of fish, because after the shot about 100 fish were floating belly-up in the vicinity. They of course said "no dead fish hunh". Instead of answering, I drove the boat to where the fish were floating and suggested that they pick up one of the fish. As soon as their hand touched the fish, it turned right side up and quickly swam away before they could close their hand. They were convinced that the fish were only stunned, and not killed. I had learned about this when we stunned a nice big cod in

Boston Harbor during the war, and we had decided to pick it up and have a cod dinner, only to lose it. They wrote a nice feature article about our work and assured the world we only stunned the fish and did not kill them.

We finished the field work in about a month and immediately analyzed the data. We were able to show them that the granitic rocks exposed on the east bank of the river, dipped about 5 degrees westward covered by Triassic sandstones dipping the same way, which were in turn covered bye sediments. The topmost layer of less consolidated sediments covered the sediments to a depth of about 100 feet. the deepest sedimentary section, almost 800 feet, was near the west shore, shallowing to about 300 feet above the sandstones about half way across the 15,000 feet width of the river. From that point the sediments were about 300 feet thick to where the sandstones pinched out against the granite. When the glaciers melted there must have been Triassic sandstone covering most of the valley floor. The glacier must have carved out the Triassic sandstones. The sediments from the glacial melting must have supplied all of the sediments beneath the present river, so the main channel for the ice, about 800 feet deep must have been close to the west shore with shallower sections toward the east shore. The present river channel lies above the sediments, with the main channel close to the east shore.

The contractor decided that the sandstone was too deep and too brittle to carry the main span of the bridge so they designed coffer dams, concrete boxes, which were several hundred feet square and 100 feet deep , one for each end of the main span. They were floated into position sunk in the sediment and sediment pumped into them to sink them into the sediment. The bridge was built across the river channel anchored to these coffer dams. Sediment was removed from the coffer dams as the weight of the bridge increased to maintain them at the correct depth. Several hundred 50 feet long piles were driven into the bottom from inside the coffer dams to create anchor enough to prevent the bridge from being pushed downstream by ice in the winter. The rest of the bridge was supported on bents driven only into the sediment on both side of the main span. Their elevation is monitored and sediment is removed if the bridge settles, or added if the bridge rises. I understand that since the first year adjustments seldom have to be made. This type of construction was first attempted in the Normandy landings in WW II for the landing structure.

In 1938 seismic refraction lines were made on Lid a and Dick and Reliance in the Gulf of Maine, when Atlantis could not be obtained. The line was from Portland Maine to Wilkinson Bank. After the Throughway bridge work was done we outfitted the Retriever with recording gear as well as Picket. The gear, at that time, had six 45 volt b batteries connected together to supply the b voltage for the vacuum tube amplifiers. We soldered theses connections to avoid circuit noise in the amps. On the Retriever these were mounted under the forward hatch.Before leaving New York harbor, we crossed the harbor to Earle New Jersey to load up with explosives to take with us to the Gulf of Maine.Crossing the bay, we ran into head winds which developed a quite severe chop in the bay. The boats pitched considerably and threw a lot of water onto the fore decks. The hatch on Retriever leaked and sea water was getting onto the batteries causing them to short and arc. They had to be disconnected to save them, I took out my sheath knife, that I usually carried while at sea, and started cutting the interconnections. With sea water on the floor and on the batteries each time i cut a line I would get a severe shock. I shouted my displeasure at this as i cut the six interconnections. Chuck Drake who was watching almost died laughing. After they were cut apart. we moved them to a place where they could be kept dry, and dried off the sea water and washed the outside of the batteries with fresh water. We continued on our way.

When we got to Woods Hole I arranged for the Caryn to join us in the Gulf of Maine on the line from Rockland Maine to Georges Bank, and the line from Gloucester, Mass. to Yarmouth, Nova Scotia. . The Picket and Retriever could not carry enough fuel , water or victuals for these trips across the Gulf of Maine, so Caryn was to carry the bulk of explosives and act as tender replenishing us as necessary. We left before Caryn received the explosives from Hingham, Mass. and made the inshore profiles on the states ends of the lines while awaiting Caryn. When Caryn arrived we made the offshore profiles. We would start operation at daylight and continue until dark. Then Picket and Retriever would rendezvous and spend the night jogging around the area of our next profile since they rolled so much when hove too that you could not sleep. We had two high school kids with us on each vessel to assist.I would stay awake half the night with one of the kids to assist, and Chuck would stay awake the other half of the night with another kid to assist. We would drive the boat at slow speed for a couple of hours on a course that would minimize the rolling, and then reverse the course for a couple of hours. The watch was always glad when it was time to call the other watch.

When we had to revictual, we would come up to the lea side if Caryn keeping some truck tires between the vessels and tie up. Needless to say we did not waste time while we were tied together as the different motions of the vessels caused them to collide and separate. On one occasion, Chuck was approaching Caryn to tie up, made a miscalculation and ran into Caryn splintering one of the boards on her bulwarks. After that, he made me take the conn anytime we came alongside Caryn.

One night, in the middle of the Gulf, a storm blew up and we could not find any course which would not cause water to come over the bows of Retriever. the result was that we were taking quite a lot of water through the leaky hatch. About midnight, the pump run by the main engine quit and it looked like our situation was becoming serious. We called Picket on the radio and explained what was happening . They told us that they had a manual pump and would try to come near enough to us to pass it to us. This didn't work as it was too dangerous, so I threw them a line, they attached the pump and we pulled it over. This was a manual pump about four feet high. The outflow could not reach the deck so we had to pump a bucket full, carry it on deck, dump it and return for another. All four of us had to work that night, tow of us operated rating the vessel, minimizing the water over the bow, and two of us pumping and carrying water. about three in the morning, we again got the pump on the engine working. We all got a short nap during the rest of the night.

On our last profile, which was on Georges Bank, we set up station on the north end of the line and Picket set up on the south end. Caryn fired charges starting at Retriever and ending at Picket. When we had finished, I radioed them to head for the entrance buoy at the channel between Nantucket and Chatham, and that we would set our own course and join them at the buoy. This was a run of about 70 miles. After about five hours of run, we saw the masts of Caryn and without anyone slacking speed, we fell into line and made the last two hours to the entrance buoy together. When we got to the dock at WHOI. the captain of the Caryn complimented me on our fine navigation. I never told him that we thought we were heading for the entrance buoy and not to join the others. If we had not seen them, we would have missed the entrance buoy by about ten miles. We only had tank compasses that we had obtained on the government surplus lists and had never been able to swing them to determine their corrections.

In the 50 to 52 period there were three noteworthy things whose specific timing eludes me. One was building the first machine shop. The shop in Schermerhorn Hall had become inadequate. There was a need for more machinists, for additional machines, and a larger storage area for shop materials. Angelo had become an important part of our research team, but he could not do everything that we needed. His attitude, his bonhomie and his made up words, like "faysay-lugan' helped cement the esprit de corps of the group. We saw the move to Lamont as an opportunity to get a proper shop. We persuaded Columbia to build the machine shop as an addition on the greenhouse that the Lamont's had. Since it was built as a machine shop, it had sufficient height to have an overhead traveling chain fall capable of handling 2000 pounds. Angelo surveyed the lists of machine tools that were surplus for the Navy and we were able to get nearly all that he felt we needed. These were installed in the new shop. We could now add to our shop staff as needed. ONR allowed our shop expenses to be carried as a separate item from our scientific budgets. This was important, because the scientists (often students) came up with new instrumental needs of their programs, they could go to the shop and request it, without having to first fight to have additional funds added to their program. The new shop had a stock room in which seldom used but often necessary tools were kept as well as supplies of taps, dies and other items that often suffered from breakage or wearing out in the course of our shop work. Supplies of rods, bars and sheets of brass, aluminum, and steel were accumulated so that new ideas could get underway quickly. Most of the graduate students did some work in the shop at this time. Much later on, they had little participation in the shop and their ingenuity suffered. These latter students would all say why should we learn this when the professionals can do it quicker and better.

The second item was started by a telephone call that Doc received from the Carnegie Institution of Washington. They had been storing the winch Piggot had used in his early explosive coring of the ocean bottom for about 20 years without it being used. The upkeep seemed to them to be excessive. Knowing that we operated an extensive coring program, they asked if we wanted the winch, Doc sent me down to Washington to look at the winch and decide whether we should take it. The winch could hold about 30,000 feet of i/2 inch wire rope. It had a planetary drive so that either the drum could be used, or a lathe gypsy head on one end of the drum. It had a 100 hp GM diesel for power, a truck transmission for gear reduction and a chain drive to the planetary drive. It was built on a six inch bed of I beams. It stood about eight feet high and was about eight by twelve feet on the base. They had kept it well cleaned and painted in all the years since Pigot had used it till then. On Pighot's last coring attempt in the Puerto Rico Trench, the strain on the wire had caused the drum flanges to spread so that they dragged against the frame. They had been able to recover their equipment despite the drag from the frame, but had never again tried to use the winch. Pigot's mode of operation was to move the winch with its load of wire rope, about 12 tons, to each ship he had been able to get for his work. He had a large boom, that could be attached to the ship , that was capable to use to hoist the winch on board in case the ship's own gear could not. There were also two large wooden cases which housed enough rope 2 in in diameter and triple blocks so that a block an fall could be attached to the boom to lift the winch.

Although I didn't know how, when, or where we could use this equipment, I urged Doc to accept the offer. The only catch was we would have to pay to move it all to Lamont. I went to ONR and persuaded them to fund us for the move on the basis it would give us adaptability to use more different ships.The equipment arrived at Lamont on a big flat bed truck and we had them off load the winch in the yard behind the shop and the boxes of the rest of the equipment into the cow barn for storage.

The third event of interest started with our receiving a list of surplus property that the Navy had which could be made available to ONR contractors. In looking this list over I found two 7 ton fork lifts, a large reel of armored underwater twenty conductor cable and several other small items which I requested. I had no special use in mind, I just thought they would be useful. The Navy shipped them to us without charging us for the shipping.When the flat bed trailer with the fork lifts and the armored cable arrived, I had them off load these items in the yard by the machine shop. While the off loading was going on, Doc came by and saw what was going on. He asked me why I had in mind to use the fork lifts for. I told him nothing, I just thought they would be useful. he then said "I understand , you ordered the forklifts so that we could handle the big reel of cable, and the big reel of cable so that you would have something to use the fork lifts for".

Not long afterwards, we connected the seismographs in the root cellar -now the seismograph vault-to Lamont Hall where the seismic recorders were mounted in the front hall. We could not have afforded this arrangement at that time if ewe hand"t had the armored cable.

Soon after we received the fork lifts, ONR contacted us and said that they urgently needed a fork lift for Hudson Labs and could we spare one. We did. We of course kept the better of the two for our own use, and I still believe it is in use at the Lamont-Doherrty Earth Observatory, Lamont's present name. Without a fork lift there were many jobs over the years that would have been much harder for us to accomplish.

Later the picket and retriever were used in Long Island Sound by Bernie Luskin and John Ewing to develop the sparker. The sparker set off a spark in the water making a medium frequency sound. This reflected from the bottom and from layers within the top about 100 feet of bottom. WHOI pioneered this device, and we felt we had to have one for our deep sea work. It was used for a number of years, but was later scrapped as it was quite dangerous. It had large oil filled capacitors which could give a lethal shock if inadvertently contacted by an operator. Around salt water it was especially dangerous.

In 1951 we got involved with turbidity currents. Doc and Heezen investigated the breakage of the cross Atlantic telegraph cables after the 1929 earthquake near the Grand Banks. These cables, about five in number, were broken in sequence with the closest to the Banks breaking first, and the farthest breaking last. These cables passed south of the banks at a number of miles distant. Unable to think of any other mechanism, they decided that the earthquake must have started a landslide which turned into a turbidity current which being denser than sea water would rush down the continental slope. This idea had been proposed earlier to account for the cutting if the submarine canyons/. This was one of several ideas, but no corroborating evidence of their existence had been discovered. Taking the times at which the cables had broken they determined that a turbidity current would have had a speed of up to 100 miles per hour. This seemed so inconceivable, that most geologists scoffed at the idea. Kuenen, a geologist of the Netherlands (I believe) who had been modeling turbidity currents in a flume, made some calculations from these data and forecast the settling of the sediment on the basin floor in a delta-like structure giving estimated thickness of the sediments at several distances.

On a cruise, in 1951 (?) Doc and Heezen took soundings around the proposed delta-like area which demonstrated the existence of such a feature. They also took several cores in the Delta region and showed the thickness of the overlying mass was substantially what Kuenen had estimated. The sediments we're also graded with the coarsest on the bottom grading to the finest at the top. This proved to be overwhelming evidence of turbidity currents. Nevertheless, many geologists would not accept the idea, especially they thought the speeds of 100 mph excessive. Some geologist took this concept and looked at rock section on land that had been hard to interpret. They found graded beds and all the other features that had been described. It still took many years for the profession as a whole to accept the idea. Now it is accepted as common knowledge.

When the cable laying ships went out to repair the cables, they picked up one end of the broken cable, spliced on a section of cable, then dredged up the other cable and spliced it together again. The cable ships anchored in the area while they made the splices. On one occasion after splicing the cable together and throwing it over the side, while recovering their anchor, they broke the cable they had just spliced. They had to repair it again. When discussing the event, the Captain of the of the ship said "Wouldn't you know it, the one chance in a thousand happens nine tomes out of ten".

In the summer of 1951 we convinced ONR that we needed a vessel to do deep sea work with our students during the summer months. At that time, they had concluded that Hudson Labs needed a deep water ship part of the time. They decided to make a deep sea tug available to the two Columbia Labs for alternate cruises. The tug was chosen because of the size, and the towing winch, which ONR thought could be modified to a deep sea winch. Since we had had the most experience with deep sea ships, we were chosen to work with the Brooklyn Navy Yard in the conversion of the Allegheny. Doc chose Chuck Drake and I for this chore. Chuck and I made a trip down to the Navy yard once a week during the planning and the conversion. The work was completed in early 1992 and we agreed to let Hudson Laboratories take the first cruise. We planned our cruise turn to start in early June and last until September. In the fall of 91 we had started to accumulate all of the equipment that we would need for the cruise. In February we were informed that Hudson Labs would need the ship for the summer for some of the classified work they were doing, and they had priority over us. This was devastating to our plans.

Doc and I had to leave for a scientific meeting in Cambridge, England just as we received this information. On the trip over and back, we discussed at length what we could do. We had spent most of our contract funds accumulating the equipment and made arrangements with a number of students to participate in the summer cruise , and there was no way we could arrange a cruise without a deep sea vessel. We finally decided that Doc would have to go to ONR on our return and convince them that since they had caused this disruption in our plans they should pay the cost for chartering a vessel for us. He must have been persuasive because they agreed. On his return to Lamont he appointed me to find a suitable vessel.

I purchased a Yachting magazine and perused all of the ads for ship sales or charters. The one that looked best was for a vessel known as Vema, advertised by a man named Kennedy in Lunenburg, Nova Scotia. I called him on the phone and everything he told me sounded good, so I made arrangements to go to Nova Scotia to see the ship. She was 200 fit, a three masted schooner with a Burmeister Wayne auxiliary engine which could drive her at ten knots. She had started out as the yacht Hussar built for Mr. E. F Hutton in about 1910. It later was sold to Vetlesen a Swedish shipping tycoon, who had changed her name to VEMA after his wife's name Maud Vetlesen. In World War 2 the U.S.Government had acquired her and converted her to a merchant marine training ship. The whole midships area had been removed and triple decker folding bunks had been installed along one side.The rest of the midships area was a large space used for training purposes when the weather would not permit them to take place on deck. The after area, formerly the owners lavish quarters, had been converted into storage spaces and a spartan living quarters for the training supervisors. There were two large deck cabins aft which had hosed more of the trainees. A cabin amidships housed the Radio equipment, the radio operator and served as the chart room. An elevated enclosed are just in front of this cabin served as a bridge. It was nearly as wide as the ship at that point. As a yacht it was steered from on deck aft, since they expected to sail most of the time. The merchant marine had installed a steering wheel forward on the bridge with an engine room telegraph and a very small chart table. The sails were functional, but the engine was not operating. Kennedy had found her on the mud flats of Staten Island starting to be scrapped. He decide she was too god to be scrapped, bought her and sailed her to Lunenberg where he had two men working to get her engines working. I was inspecting her in February and there was no heat operating. I have seldom felt as cold as in the engine room.

Vema was larger than Atlantis, had considerably more deck space and cabin space that could be used for scientific purpose and scientific crew. I told Kennedy that I thought we could use her and requested his bid for a charter starting at New York on the 1st of June and ending the 1st of September in New York. He said that we could charter her with a crew under his command to operate the ship, and feed a crew of 10 scientist, for the three months for $20,000. I told him I thought the ship would do, but that I would have to clear it with ONR first.

I returned to New York and told Doc that I thought I had found a suitable ship if the charter price could be met. Doc called ONR and they agreed that they would add that amount of money to our contract. I called Kennedy and asked him to come to New York to sign the necessary charter papers with the Columbia contracting group. He came down and we met at

Lamont.While we were discussing the charter I asked him what he would sell us the ship for. He answered $100,000 and that if we bought it before the charter was up, he would allow the charter fee to be deducted from the price. We concluded the charter and he returned to his home.

I went out on the first cruise of Vema on a charter from Kennedy. We took a lot of things with us since we didn't know whether all of the equipment we had installed would work. We thought that with the things we had we could make our gear work or devise other gear that would. One of these items was a reel of multiconducter cable which weighed about 1000 pounds. Shortly after leaving New York we ran into a storm and this reel of cable came loose and was sliding around the deck with the water coming on deck pushing it around. In the rough weather it was dangerous to get in the way of the reel of cable. Captain Kennedy and i went on deck to try to get it secured before it did some real damage. We could no get it secured to the rail before the water would move it away. Finally we got a long rope, fastened one end to the rail looped it around the cable and got a turn on the rail. Each time it moved towards the rail we would take in slack until we had it captured at the rail. We then secured it properly and had no more trouble.

The only other event that sticks in my mind from that trip was that the head which had been used by the merchant marine trainees was in the deck cabin aft and had multiple sinks etc. This became the scientists head. We started to leave cakes of soap on the sinks for later use. Shortly we found no soap at the sinks each time we wanted to wash up. After some detective work we found the captain's dog, a large Husky,was sneaking in and eating our soap. A.apparently he felt he was not getting enough fat in his diet. The captain would not tie up his dog, and could not get him to leave our soap alone, so we had to take our soap into our cabins , where we would have to go get it each time we wanted to wash up

Before we left on our cruise, they had had trouble with cracks in two of the cylinder heads of the diesel. Kennedy found a man who could make a repair and it was working when we went to sea. shortly, however, two more cylinder head developed cracks and we could no longer use the engine and we had to complete our cruise under sail. Kennedy was a good sailor, and even though his hands didn't know too much we got along fine. We had to go into Guantanamo Bay to pick up explosives while the engines were out of use. We entered the bay under full sail in the middle of Sunday Afternoon. All of the fleet stationed in the area was at anchor in the bay This made the bay crowded with Navy ships. We sailed into the bay, made a right angle turn, then Kennedy dropped the Mizzen, Main, and Foresail, followed by the three jibs all by himself. With no sail on we ran about a mile upwind before we lost way. He then loosed the anchor and we fell back downwind letting the anchor chain out until the proper amount was overboard. We came to a stop in the front line of Navy ships between two of the largest ones. We noticed all the watches on the Navy ships were on deck watching our maneuver. The base was closed because it was Sunday , so on Monday Captain Kennedy and I made a visit to the base Commander to arrange for the delivery of the explosives. The Commander remarked on the excellent execution of our entrance and anchoring in the bay. He said he had watched the whole thing from his porch. We received the explosives that had been requisitioned for us and left port the following day, again under sail only.

At the completion of this cruise we were pleased with the roominess of the vessel and how well she could do our work so we decided that the ship was excellent for our need if the engines could be made to work and that we should try to purchase it.It was put on dry dock to do some bottom repairs and to attach our sounder head better. There were two weeks until the original charter time was up. We arranged a meeting on Friday afternoon, the last day of the charter, to finalize the purchase, or to arrange an additional charter for three months. On that Friday, I was working on the controller for the hydrographic winch shortly after lunch when Doc called on the phone and said that he had struck out, he could not raise the money for the purchase. I told the captain and went back to work. Shortly, I decided this was a mistake and I got in my car and drove to Lamont. I went in to see Doc, who was working with Frank Press, and said "Doc, this is a mistake. If we are ever to have a ship this is the one. If we don't get this one, we'll have to just make up our minds that we will never have a ship". Doc turned to Frank and said "What do you think?" Frank answered "Joe is right".

Doc then decided that we would try to get Columbia University to put up the money for the ship with the understanding that we would repay it. He called the treasurer's office and asked to speak to My Campbell. His secretary said he was not in. Doc then said I must talk to him now, so tell me how I can reach him. She told him there was no way. By persisting, he finally found out that Mr. Campbell was out on the golf course. Doc then called Mrs. Campbell, who he had met on several occasions, and asked her how to get My Campbell on the telephone. She called the golf club and then called back telling us that there was noone at the golf course that could go out on the course and get Mr. Campbell so that she would drive over there and get him to call back. About three quarters of an hour later Mr. Campbell called. Doc told him the situation and that it was imperative that we get a ship now. He also told Mr. Campbell that we would make him prouder of the Vema than he was of any part of Columbia University if the would do it. Finally Mr. Campbell agreed and said he would call his office and make the necessary arrangements.

By then it was 3 p.m.. Shortly Mr. Campbell called back and said it was too late to make the necessary bank arrangements from the treasurers office, but that he would write a check on his personal account for the $80,000 to secure the vessel, providing that we would not let any of the trustees know of the transaction until he had made them aware. Doc agreed. Mr. Campbell, Doc and I met with Kennedy at 5 p.m. in the treasurers office and notified him that we were purchasing the vessel instead of extending the charter and that we wanted him to continue as the Captain at least through the next cruise. He was very disappointed as he had decided that Vema was such a good vessel that he could probably sell her for more. Nevertheless, the sale went through. The first thing Monday morning, Doc received a call from one of the trustees asking him if he had bought o vessel for Columbia University. Doc had to admit it and asked how he found out. The trustee said that the treasurers office had made arrangement for insurance for the vessel Friday afternoon with his company. Doc then told him that we would make him as proud of that vessel as he was of any other part of Columbia University. We never heard of any other repercussions.

At that time all of our government work was under contract with ONR. I went to ONR and convinced them to amortize the ship at the rate of $10,000 per year. This was continued for ten years. They authorized numerous changes to the vessel over the years. After about five years, the National Scince Foundation was formed and was supporting about half of our work. They shared equally with ONR in the amortization and the various changes we made to the ship.

In anticipation of the second trip of Vema we decided to install the Piggot winch. This meant that we had to correct the spread of the flanges. In order to work on the flanges. we had to remove the wire rope, all 30,000 feet of it. We set up the winch in the yard behind the machine shop and by running the winch we started to unreel the wire rope. Since it had been wound on under tension, as soon as we made any slack the cable made tightly twisted loops (assholes in the vernacular). We did not want to wind these onto our spare reel so we had to find a way to untwist them.

The wire rope is manufactured by twisting small wires in a counterclockwise direction to make strands, and six strands are wound counterclockwise around a core of hemp, about the size of a strand. When the wire rope is put under tension, it unwinds clockwise. When it is wound under these conditions it retains the unwinding of the tension under which it was wound. When then removed without tension as we were doing, the wire rope wanted to return to its original condition so the twisted loops resulted.

We wanted to save the wire rope since it was in good condition and a replacement would cost about $5.000 which would have been too hard for us to find on short notice. The twisted loops would only get more numerous and tighter twisted as we progressed, so we had to find a way to untwist it. At first we tried to turn the reel on which we were winding the rope around and around on the ground to take out the twists. This took too much effort and time so we had to find another way. Doc came along and suggested that we lift the reel, on which we were winding it on one of the fork lifts and dive the fork lift in circles to take the turns out. We tried this and it soon became obvious that it would take at least a week and we would all get thoroughly dizzy by this method, so another method had to be found.

We then got a strong back, a strong bar, supported on the tines of the fork lift, supporting the take-up reel on an axle supported on each end by a chain beneath another bar which was supported beneath the strong back be a chain and a swivel. We then would slack a length of wire rope from the winch, wind it on the take-up reel until we came to a kink. We would then take the twist out by rotating the reel on the swivel and proceed to the next kink. It was hard work as the take-up reel became heavy quite soon and kept getting heavier. We made up two teams of four, one to run the winch, one to manage the forklift, and two to rotate the take-up reel. It still took us about 10 hours to unwind the wire rope onto the take-up reel.

As we wound the rope off, the flanges of the winch reel returned close to their original position. To try to prevent the spreading of the flanges, i/2 inch thick gussets, about 3 inches on the base and about 12 inches long had been welded between the core, which extended beyond the flange by about 4 inches, and the flanges. Sixteen of these had been welded on at equal spacing around the circumference. We decided to add as many more gussets made of 3/4 inch material half way between the existing ones. It took four of us four days to complete the job. This was the first welding experience that three of us had, which probably didn't speed up the job any. Nevertheless Angelo was patient and after a few hours of welding, we learned the proper technique. This skill stood me in good stead in the following years. Originally, in my welding learning, I was too impatient and advanced my weld too fast, thus making too poor a bond with the metal, with the result that about the time I was finishing one pass, the weld, where I had started, started to peel off. This served as a lesson for the others, and they got good penetration and they had no trouble. Naturally I took quite a ribbing about this for several days.

When we finished the repairs, we had to wind the wire rope back on the winch drum. This time it took only about six hours. Unfortunately we could not wind it on under tension causing problems with the wire rope pushing down a couple of layers when each deeper lowering was made at sea. This meant lowering slowly to prevent the wire rope from snagging and breaking. The winch was installed on Vema at the shipyard where the ship was having some bottom work done on it between cruises. We had an A frame which had been removed from a fisheries vessel and left at Woods Hole Oceanographic Institute. WHOI was glad to get rid of it, and we were glad to have it, so we brought it to New York and had it installed about amidships on the starboard side of Vema. The winch was mounted just forward of the aft cabin. This gave us the maximum distance from the base of the A frame. This was necessary so that the level wind could work. It never worked perfectly, but it was adequate until years later, when we added Lebus Spooling, which nearly worked perfectly. We used this rig for a number of years and took many bottom cores with it. It was slow and took us about eight hours to take a core in 3000 fathoms ocean depth.

While the ship was on dry dock, we had a sound head for the new 12 kilohertz sounder, the Navy had developed, installed on the keel. This was a great improvement on the previous sounders as it recorded rectilinearly on continuously moving teledeltos paper about 10 inches wide. It was still not satisfactory as the topography was still not enlarged in the vertical enough to study most of the bottom features.Doc set Bernie Luskin to work to expand this scale. He obtained Times Facsimile machines which recorded on continuous paper of 20 inches length and 12 inches in width. By installing an ingenious switching system he could prevent recording on each pass across the paper until the segment of the appropriate depth range of the bottom occurred. He made the scale across the paper equivalent to 400 fathoms The paper had to be changed each hour by the watch, i.e. about each 10 miles of travel. The watch also had to keep track of which 400 fathom sector was being recorded. We made the sounder record in the chart room, in the cabin on the deck just aft of the bridge.

On an early cruise, we had Bernie's contraption in the chart room with the 270 volts of 45 volt batteries mounted on the deck. A storm arose, and sea water started to come into the chart room around the doors, even though there were kick boards which were supposed to keep it out. The drain in the cabin was partially clogged, so the sea water was coming in faster that it could be drained. The rolling of the vessel made the sea water, ever deepening, slosh back and forth and pretty soon it was threatening the 45 volt batteries. Bernie and I solved that by picking up the batteries and holding them, then we would bail the water out of the cabin with a dust pan. After awhile the water would get low enough we could return the batteries to the floor and get a rest. About then more waves would send water into the cabin and we'd have to repeat the process. As the storm worsened, we spent more and more time holding up batteries and bailing. This went on all night, but finally in the morning the storm abated and we could get some rest. We were thoroughly exhausted, but the sounder worked throughout most of the storm.The next day the drain was cleared and we built a rack to elevate the batteries about 18 inches above the deck.

Another feature of the Facsimile recorder was that its mechanism was controlled by a tuning fork that was accurate to a part in one million. This corrected the troubles we had had with the earlier equipment whose recorder was controlled by the ships generator which fluctuated with the electric load it was carrying, and the attention the engineer could give it.

This equipment evolved under Bernie's guidance so that the switching was controlled electronically instead of by relays. We put the recorder made by the Navy in the chart room for the use of the ship's officers, and took the signal aft for the scientific party with the Facsimile recorder giving expanded scale recording. We soon found that we lost recording in rough weather because of the bubbles which passed between the sounder head and the ship's bottom.

Shortly after acquiring Vema we obtained a copy of the magnetometer which had been developed in WWII for submarine detection. We enclosed it in a water tight fish" and towed it far enough astern that the ship's field did not affect it. The recorder was established in the laboratory which was developing into the after lab. In the lower lab, the core were appraised, wrapped in water tight paper and sealed, and stored below decks for more thorough study after return to Lamont.

About this time a nuclear resonance magnetometer was developed which was more compact and easier to use. Doc set one of our electronics people to develop one for towing behind the ship. Soon this replaced the older one and was easier to use. Magnetic recording all the time the ship was underway soon joined sounder recording.

Bruce Heezen, a graduate student, was chief scientist on Vema 2. He studied the topography in the deposition area of the turbidity current started by the earthquake of 1928. He also took cores in the deposit as outlined for him by Dr. Ewing. During the cruise, Captain Kennedy made a port stop in Lunenburg and off-loaded a large amount of equipment that was on Vema. On her return to New York we demanded the equipment returned. He refused and since we had no list of what was specifically included in the purchase of Vema there was little we could do. So, we removed Captain Kennedy from his command. He took his crew with him. We later heard that all the equipment was lost shortly due to a fire.

In searching for another Captain we found a man who convinced us he could take over command of the ship and obtain a crew that would be less expensive than Kenmnedy and his crew had been.. We decided to give him a try. He indeed obtained a crew and they were less expensive. However, they were an unruly lot and after two of them started a fight in the forecastle which included some knife play we got rid of that Captain and his crew. We then were able to persuade the former Captain of the Kevin Moran, the tug we had hired earlier, to take the Vema. He insisted that he did not have any experience in sail, so Doc persuaded Captain MacMurray, former Captain of Atlantis, at that time retired and living in retirement in Sailor's Snug Harbor on Staten Island, to go along as sailing master to train our Captain in sailing technique, He would provide the expertise in sailing until the captain felt comfortable under sail. At about this time, we added Hank Skjerding as chief engineer. He was recommended to us by Burmeister Wayne who we had to put the engines in first class shape, after we had nursed them along for a couple of years

After the completion of the Heezen Cruise, we arranged to have Vema and Atlantis meet at Puerto Rico to carry out seismic refraction work across the Puerto Rico Trench. Ewing was to be aboard Atlantis as Chief Scientist, and I was Chief Scientist on Vema. We left new York with much of the equipment jury rigged, as we had not had enough time to properly install it. Nevertheless, we made numerous observations on the way to Puerto Rico. Engine problems described above continued on this cruise, and most of the cruise was completed with sail propulsion for Vema. The seismic refraction work had now evolved to operate as follows. Atlantis would heave too and become the listening ship. Vema would proceed on a straight course firing explosive charges at intervals as requested by Atlantis. These charges would vary in size from 1/2 pound in the first mile or two to 300 pounds (a navy depth charge) at the extreme range of fifty or sixty miles.

Then Vema would lay too, and Atlantis would approach firing charges at intervals as requested by Vema. Again large charges were used at great distances, with lesser sizes used as the distance closed.The shot instant was transmitted by radio from the shooting ship to the receiving ship. The receiving ship would slack the hydrophone cables so that the ship was not tugging on them, making noise, when the sounds arrived at the receiver. The travel times of the sound wanes that had traveled by different paths through the various sub-bottom layers were plotted against distance. Analysis of these reversed stations produced the true velocity in the sub-bottom as well as the layer thicknesses beneath each receiving vessel. The underway vessel would record soundings continuously.

On the following cruise Dr. Ewing went as Chief Scientist. Shortly after leaving New York about half way to Bermuda they ran into a severe storm.They had taken several 55 gallon drums of fuel on deck, as the planned leg of the trip would extend the fuel capacity of the tanks. In the morning, just after daylight John Ewing had just come on deck when an unusually large wave came over the rail and broke the lashing of the fuel drums. The first mate was also coming on deck from a forward cabin. The mate got the second mate, and John got DR. Ewing to help secure the fuel drums. They had just got them secured in the waist, and Doc told me later, he had just said they will never come loose again, than an even bigger wave came on board and broke the new lashings and took the drums and the four men over the side. The bridge watch saw it all and quickly spread the word "man overboard". The Captain came on deck with Captain MacMurray and took charge. He set Captain MacMurray in charge of the deck and he climbed the mast to try to keep track of the fuel drums. All hands were called on deck to act as lookouts. The seas were so huge, Captain MacMurray was unsure that he could safely turn the ship around. They managed to make it and returned to the area of the fuel drums where they picked up the Second Mate. On the next pass they did not fond anyone else. After passing the drums a second time they turned and and lay too to look for others. They had just given up and were starting to get under way again toward the drums, when one of the lookouts happened to look down and saw John Ewing about fifty feet off the side of the vessel. They threw him a line, and soon had him on board.

John said that he had caught the log line trailing after the vessel, when he went overboard, and was dragged some ways from the drums. The strain on his hands became too strong, and he had to let go. It was just a lucky accident that the ship turned and stopped near him. He had been yelling at the top of his lungs, but none of the lookouts heard him in the noise of the storm.

On the return to the drums, DR. Ewing was sighted and the ship was maneuvered near him. He had been hit in the back of the neck when he was washed overboard and his left side was paralyzed. He could not get hold of the rope they threw him, but the ship drifted down on him. On a severe roll, while he was directly alongside, one of the lookouts was able to get his arms under Doc's armpits while the rail was even with the water surface, and they soon had him on board. They searched the area until they could no longer find the drums, which had been separated by the seas. They did not ever find the First Mate.. They then made their way towards Bermuda.

Back in New York,. I received a call from the Coast Guard that the ship was in a severe storm, the ship was distasted, the mast was floating nearby, still attached to the ship by the stays, and was threatening to sink the ship, and that several men had been lost. This was such an improbable story I didn't believe it. A story was reported on the news programs saying the ship was having troubles, and that some crew were lost. I soon began to receive calls from wives of the scientists on board demanding to know if their loved ones were among the lost. I could only tell them that I did not know, that I was in touch with the Coast Guard and that I would let them know as soon as I could. The Coast Guard called me several times that morning, each time with a different story of the condition of the ship and the crew. Finally the ship made it into Bermuda and I found out that only the First Mate had been lost, and that three others including DR. Ewing and his brother had been washed overboard and recovered. The ship had never lost her mast. I immediately left for Bermuda as Dr. Ewing was still relegated to his bunk and was not too coherent.

On my arrival in Bermuda I learned details of the story recounted above. Gordon Hamilton, director of our Sofar Station in Bermuda, and I surveyed the ship damage, and with Ham's knowledge of the Bermuda work force, to arrange to make repairs. The after cabin, near where the wave had come aboard had its paneling severely cracked, and the two inch in diameter turn buckles in the rigging had been bent about 20 degrees. The stairs from the main deck to the poop deck, nearby had been ripped out bodily and only a few splinters still adhered to the securing fixtures. There was much other damage, but I can no longer remember what. The repairs were made in about three days.

A memorial service was held for the lost Mate in the graveyard in St Georges, Bermuda. All of the ship's company attended but Dr. Ewing, who was still relegated to his bunk. When first recovered, his speech was slurred and hardly understandable. After the three days, he could speak without slurring, and he was regaining his feeling on his left side. He told me that one time, when the shiv was turning while he was overboard, he could see all of one side of the ship's bottom! That must have been one whopping roll!. He also said that when he first came to the surface he was near to one of the drums and he swam over to it and tried to grab hold of it. There was nothing to grab onto and he was not having any success, when the Second Mate appeared on the other end of the barrel and told him to grab one end while he got the other. That was when he realized that he was partially paralyzed. He held on for awhile, but soon could not hold any longer. He then concentrated on keeping afloat. The next thing he remembered was being pulled onto Vema. I believe that all of them would have been lost if Captain MacMurray had not been on board as sailing master, and if the drums had not gone overboard too.

Dr Ewing insisted that the cruise must continue as there was a planned operation with one of the oil companies in the Gulf of Mexico. He obviously could not go to sea in this condition, so I arranged for him to return to New York, and I continued the cruise. Gordon Hamilton came along too. It was a hard time on board. When anything became difficult, the staff would state vehemently, that had I been on board during the storm, I wouldn't be asking them to work so hard. Gordon and I pitched in and we were able to get them to help us with the scientific program, so that little was lost. On arriving at Galveston, Texas, about a month later, Dr. Ewing was on the dock to meet us. He took over again, sending Gordon and me home. He recovered the full use of his limbs in that month, but he always had a limp in his left leg for the rest of his life.

In 1952 ONR organized a scientific meeting at Ranch Santa Fe in California to summarize the work that was being carried out at the oceanographic institutions. My job was to summarize the work being pursued at Lamont. I reported that Lamont was actively pursuing continuous soundings, seismic refraction at sea, ocean bottom coring, gravity measurements on board submarines, magnetic measurements from a body towed a sufficient distance from Vema to avoid the magnetic field of Vema, a tank to recover 300 gallon samples of sea water, which could be analyzed for the Carbon 14 content, which in turn could give the time the water sample had been out of contact with the sea surface, use of the Sofar channel to obtain topographic echoes. These could be used to locate sea mounts, some of which had never before been discovered.

The 300 gallon tank for recovering water samples was attached to the coring wire rope at such a point to recover a water sample at the desired depth. Originally the tank was a monster of Steel which made handling difficult. In time the tank was modified to be made of fabric that could be collapsed when descending, and released to expand to obtain the desired size of sample at the appropriate depth. Later the tank was modified to be made of fibreglass, and smaller samples were needed. To make sure it was properly vented, it scooped water into the tank while it was being lowered so that the water sample was continually changed to represent the depth at which the tank was located. At the proper depth, the tank was closed and a sample was recovered. The Geochemists took these samples and removed the dissolved carbon dioxide, concentrated the carbon and measured the Carbon 14/Carbon 13 ratio. This was interpreted as an age since the water at that depth had been in contact with the surface of the sea. They soon demonstrated that the circulation through the deep sea was much slower than had been believed. This water tank addition to the wire, when we took ocean bottom cores,was the beginning of a number of additions to our work by doubling or tippling the use of various techniques which greatly increased our efficiency. It also allowed us to add additional investigations to our repertoire without requiring vast amounts of additional time.

The trawl winch had some problems. On one of the early cruises, the sleeve bearings for the main drum caused trouble. We attempted to fix it by jacking up the drum at sea, after each lowering to scrape the bearings by hand. This operation took about four hours to complete, and had a certain amount of danger since the about 5 ton reel of wire rope was not too well secured when lifted in this way in a seaway. This never succeeded, even though some improvement resulted.

At the next return of the ship to New York, Angelo replaced the sleeve bearing on the drive side of the drum, with a self aligning ball bearing that we had obtained from government surplus. This was one of those kind of things that I requested, not knowing what we would use, it for. This bearing was about 15 inches in outside diameter, with an internal diameter of about 8 inches. It had a double row of bearings, about 1 inch in diameter, in the ball race. The inside of the outer race was shaped as a part of a sphere, so that if the shaft was not quite straight, the inner race would adjust itself as it was turning. By the time the ship was approaching Bermuda at the completions of the first leg of the cruise, we knew that this did not satisfactorily solve the whole problem. I decided that we would have to have a spherical ball bearing on the other side as well. This was not so easy as this side had the planetary gears which had to turn inside the inner race of the bearing. I carefully drew up a bearing system with an outer spherical ball bearing, with another ball bearing inside the inner race to accommodate the planetary drive. This all required a redesign of the support strantion, which was very limited by the space available around it. I purchased all of the parts that would be needed and sent Hank Skjerding to Bermuda to have the stanchion fabricated locally in Bermuda. This solved the bearing problems of the winch drum and we could take more frequent bottom cores.

Soon, we realized that the time to take bottom cores was too great, and Doc set Chuck Drake, Walt Beckmann, and I to speed up the winch operation. As it stood. we had to lower the core in gear with the diesel engine acting as a brake. This is called lugging and is very bad for the life of diesel engines. We decided that we should find a better way to lower the core, which meant really a better way to control the speed of the drum while lowering, with the engine disconnected. We had heard of a device called a Parkersburg Brake which was used on large trucks descending the long slopes of the Rocky Mountains. It amounted to a centrifugal water pump, whose output could be throttled back be restricting the outflow of the water. This caused the water to heat up, so a means to cool the water off, had to be provided. We decided that we would arrange to allow the core to fall under free fall with a Parkersburg Brake to control the rate of descent.

The truck transmission had not proved satisfactory as it could not be shifted into a higher speed gear as the cluching time was enough that, when hoisting, the drum would come to a stop while trying to shift gears.

The load could not be brought up to sped in a higher gear. The result was we had to hoist all the way in a low gear. It was taking us about two hours to lower the core to bottom, under control, about 15 minutes to manipulate the contact with bottom and take a core, and about four hours to hoist the core back to the surface. We decided that with a torque converter, a device that allows speed to be increased when load is reduced without any required gear shift, we could recover the cores quicker. We acquired an Allison torque converter of an appropriate size from GM, a dog clutch that could decouple the torque converter from the drive as desired, and a Parkersburg Brake. When the ship was next in overhaul, we undertook to make the changes to the winch. The truck transmission was removed and the torque converter installed in it's place, and the dog clutch inserted between the converter and the chain drive to the reel.. The Parkersburg Brake was installed on the opposite side of the chain drive, and the plumbing and a 30 gallon tank of water were established above the chain drive so the brake could be used.

The three of us went to sea when the overhaul was completed to see how our modifications worked. We found that we could lower the core to 2700 fathoms, under free fall, controlled by the Parkersburg Brake in forty minutes. Emptying the brake of water and closing the dog clutch we could recover the core in 80 minutes. This meant an operation that had formerly taken about six hours could now be accomplished in just two hours. We started taking cores at least daily instead of every two or three days formerly!

Another feature of the winch which had caused us trouble was the level wind. The gear box that drove it could not be adjusted exactly right, so every once in a while the wire under strain would reverse itself and wind in the wrong direction for six or eight revolution before the level wind could again force it back in the right direction. When it finally returned to the right direction, the level wind had advanced a few turns so the wire rope was piled up at one place followed by a depression immediately following. Early on we had disconnected the gear drive and made it manually operated. With experience, a man could achieve an acceptable level wind, but it was far from perfect. About 1960 we heard of a level wind scheme known as Lebus Spooling that was operated by the strain on the wire used in oil well drilling. We obtained that and we achieved nearly perfect level winding..

In the early 1960's the Navy outfitted several ships to do some bottom work and needed winches. They came and saw ours and decided to have copies made for their work.Four winches were made. Later, in Galveston, this was a great help to me as I was able to acquire one of these winches, no longer in use, for R.V. Ida Green.

On an early cruise, the wire rope parted and the corer was lost with a lengthy section of wire rope. The wire rope left was too short to reach bottom.This occurred in the middle of a year long cruise in the vicinity of Australia. Coring and associated work had to be discontinued although we had a spare corer on board. We immediately ordered a new 30,000 foot 1/2 inch wire rope to be delivered in Australia. This took about two months to accomplish and meanwhile the work of the ship had to be greatly abridged, making that part of the cruise less than satisfactory. I set out to find a way to carry a spare core wire on board for such emergencies.

The next time the ship was in New York for her annual overhaul, we built a storage reel below decks in what had been the paint locker. This was a space near the bow of the ship just aft of the crew gangway. It was two decks down, and had no large access entry.

We had two angle irons bent in a circle of eight feet in diameter. In order to get them to the storage area, before the weld to complete the circle was made, we spread it apart sufficiently to accommodate the thickness of the main deck. Then, starting one end through the hatch by turning the piece around its center, acting like a screw we turned it until it had passed through the hatch to the next deck. We repeated this process through the hatch of the second deck and we had our circular pieces eight feet in diameter in the storage area. We then welded the ends together and we had a circular piece of angle iron. We added spokes of the same size of angle iron (2 x 2 inch) to brace the circle, and also to form sides to keep the wire rope contained. These two circular sections were then attached to an axle at the center. Frames were welded together Between the ships frames on which we placed self adjusting bearings. These could accommodate any lack of linearity of the axle. Thus we had a reel eight feet in diameter and eight feet long, with an axle supported on both ends by the ships frames. We mounted a spoked wheel on one extended end of the axle which accommodated a chain drive from a gear box driven by a D. C. motor. The D.C. motor had a controller which allowed four speeds to be used . Because of the shape of the space. the reel had to be placed so that its axis was parallel to the ships length. To provide a sort of level wind, we installed a block attached to a cum-along in front of the reel.

The wire rope to be wound on the reel was supported on jacks on the dock with the wire lead to the ship"s A-frame. From the base of the A-frame a block lead the wire forward to the hatch on deck. A block in the hatch led the wire down to the lower deck where a couple of blacks led it to the hatch to the second deck. Below this hatch, a block led the wire to the block on the cum-along, where the wire made a right angle to the storage reel. The tension on the wire rope created a force forward from the cum-along so that by jacking it in by a ratchet drive 1/2 inch per ratchet, the lead for the wire could be translated in the direction of the reel axis. When the far side of the reel was reached, the cum-along could be reversed to allow the wire tension to pull the lead back forward. Inn this way wire could be wound on our fabricated reel. To take wire rope off the reel, the wire had to be pulled in the opposite direction. When installing the wire rope on our trawl winch the lead from the block at the base of the A-frame was changed to lead to the deck winch.

After this installation, we always carried a spare wire rope with us. It was needed at least a a few times, to provide an extension to the wire rope on the trawl winch, or to replace the wire rope on the winch. Sometimes replacement was needed because of wear, or corrosion, not just for loss. We never again had to abridge our observations because we did not have wire rope enough to reach bottom

Soon after we had our own ship, we added a hydrographic winch. This handled 5/32 inch wire rope, 30,000 feet long. This was used to lower lighter equipment to bottom. The 1/2 inch wire rope had a breaking strain of 20,000 pounds, while the 5/32 had a breaking strain of 2,000 pounds. We lowered ocean bottom cameras, some light weight biological dredges, and small water samplers on this lighter wire rope. We soon found that we needed ocean floor pictures to go with the cores we were taking. This additional information was helpful in interpreting the coring results. Making separate lowerings took twice as much time as just taking a core. To increase our efficiency we started to try using both wires at once. Since these were both led over the same side, and were separated by only about 40 feet, we soon got the wires tangled. This was very bad, because the smaller wire would become localized on the bigger wire and no matter which we pulled in the smaller would be moved over the larger which would soon cut the larger one off. We tried to pull them in at the same rate, but we were still sawing the larger wire. We tried every maneuver we could think of by the ship in an effort to untangle them, without success. Finally we had to cut the smaller wire to save the big wire and the more expensive gear attached to it.

After some experimentation, we established a technique of using both wires at the same time. When a load is placed on a wire, and a length is played out, it takes a wire angle from the ship which depends, in part, on the currents affecting the wire, and the wind drift of the ship. Usually the more wire out, the larger the wire angle becomes. With the trawl wire, the wire angle rarely exceeds about 15 degrees, and most times it is less than 10 degrees. With the underwater camera on the hydro wire, however, the wire angle is often as much as 30 degrees, and most times about 15 degrees. We learned that if we started the underwater camera down fifteen minutes before the trawl wire, this bigger wire angle would keep them from tangling, provided we also recovered the trawl wire about 15 minutes earlier than the hydrographic wire. Thus with an extra 30 minutes, we could obtain about six photographs of the ocean floor in the close vicinity of where we took the bottom core.

Several times the core was hard to withdraw from the bottom. There we would be with several thousands of dollars of equipment stuck in he bottom and $6,000 of wire rope at risk, with the probability that if it parted it would be at the A-frame. By maneuvering the vessel around and repeatedly raising and letting out wire, we usually were able to get loose from the bottom. One time, in the Bartlett Deep, south of Cuba, we got stuck in the bottom. After about an hour of trying to get loose, we were getting desperate. I finally decided that we must have penetrated a very strong layer in the bottom and the only way we could get loose was by a vertical pull. We maneuvered the ship until the wire rope was vertical and took an extra strain of a couple thousand pounds more than the weight of our gear. It did not come loose. While maintaining the vertical wire angle by the ship's maneuvers we quickly dropped off the excess strain and then reapplied it. The fourth time we did this, it came loose and we recovered our gear. We carefully examined the core and found that though we had penetrated the bottom by thirty feet, that at ten feet we had cored a hard layer about 5 inches thick. Our core catcher, which was about 1/4 inch in diameter bigger than the core pipe, must have caught on the hard layer when we tried to recover it. We felt very lucky that we had recovered our gear intact.

By the third or fourth cruise we had added bottom picture taking in the vicinity of the bottom coring to the group of observations outlined above in the Rancho Santa Fe conference. The sparker work which was successful in shallow water did not have enough power to work in deep water. It worked detailing the upper part of the sub-bottom because it was lower frequency than the sounder. It did not have enough power to work in water depths greater than about 100 fathoms. Besides, danger of the big capacitors charged to a high voltage made it unlikely to be developed for sufficient power to work in deep water. The original sounders of 20 kilohertz had negligible bottom penetrating power. The newer 12 kilohertz sounders occasionally showed a slight penetration in some places. It occurred to me that if we could devise a device of lower frequency that operated like the sounder, we might be able to get some bottom penetration on a continuous basis. I persuaded the Bureau of Ships to let me try. The size of the sounder head that was used would limit the low frequency that we could achieve. I decided that a sounder head about 4 feet in diameter was the largest practical size to mount on the keel. This turned out to make it possible to use a sound frequency of 3.5 kilohertz. I found a place that would manufacture a sound head using magnetostriction for 3.5 kilohertz and an electronics package to drive it to send out a signal at 10 second intervals.

In view of the quenching of the 12 kilohertz sounder by bubbles entrained in the water by the motions of the ships bow. I decided we should mount the new sound head about three feet below the keel to avoid this effect. To do that we had a structure built , which we called a bathtub, about three feet deep and about four feet in breadth and about six feet long , with the front end curved and tapered in about a 45 degree angle. Since we were adding this below the keel, we decided to mount the 12 kilohertz head on the bathtub too. The back end of the bathtub was left open so that it was completely flooded with water. This was mounted on the keel in the shipyard.The shape was designed to shed any bubbles upwards and to the sides. The captain was nervous about having a light structure extending below the keel. He soon adapted to it and made sure to keep the ship in water deeper than 20 feet, The ship's draft was 17 feet, and the bathtub added 3 feet. He soon became comfortable and kept in mind he had a three foot extension below the keel to accommodate. This device worked quite well and in most ares gave us record of any layering in the top 50 to 70 feet of the bottom . It also avoided most of the quenching of water bubbles. It is interesting that this addition is still maintained on most oceanographic ships nearly forty years later. The choice of 3.5 kilohertz was quite arbitrary and decided only by what I thought, at that time, was a reasonable size to add to a ship's keel.

On an early cruise we had installed a new main drive shaft in the trawl winch because we noted there was a lot of wear on the sun gear machined in the drive shaft end which operated the planetary gear drive. I went as chief scientist on the first leg of the cruise which was to join a ship we had chartered, the Wissama, at Puerto Rico to carry out seismic refraction measurements between Puerto Rico and the Panama Canal. Bob Menzies was a biologist working with us who wanted to make biological dredges in the bottom of the Puerto Rico trench about fifty miles north of Puerto Rico.This was a depth of 4500 fathoms. Since we only had 3000 fathoms of wire on the winch we added a 2000 fathom 5/16 inch wire rope on the end. After we had finished the biological trawl in the trench, we started to recover the gear. After recovering about 500 fathoms, we heard a loud noise and the winch stopped abruptly. The only conclusion that could be made was that the new sun gear had broken! We were in a dilemma. We were stalled with 4000 fms. of wire out, and an inoperable winch. Because we were uncertain of the new shaft, Angelo had sent the old one on board, with the idea that it could be replaced in port if necessary.. We had never considered the breakdown with most of our wire rope out. It was a fairly calm spell, so I decided to attempt to replace the drive shaft at sea. This could be hazardous, since we would have to jack up the about 5 ton cable reel, as part of the shaft replacement operation. Since I was the only one on board that had assisted in the original shaft replacement, I would have to be the main stay of the operation. I sent a cable to the lab explaining our dilemma and that I was going to attempt the replacement at sea.

We transferred the load of cable, over the side, to a cumalong supported from the A. frame, and created enough slack in the wire rope that we could jack up the winch drum. As usual, a kink developed in the slacked wire rope. We jacked up the winch drum, removed the starboard stanchion, and removed the outside case of the planetary gears. We examined the sun gear and found one of the 16 teeth about 4 inches long had snapped off.We had to remove the cover of the chain drive on the other side and remove the chain drive gear from the shaft. This allowed us to remove the damaged shaft, about 6 feet long, and replace it with the old one. We then reassembled the chain drive gear and the planetary system. I decided to rotate the planetary system through a complete revolution before replacing the stanchion and chain drive. It was well that I did, because after a small amount of rotation it could no longer be moved. This meant reopening[the planetary drive to determine the problem. We found that the broken tooth was lodged in the circumferential gear and jammed against one of the planet gears. I should have thought to locate the broken tooth before we tried to assemble the planetary drive! The tooth was wedged in so tightly that we had to drive it out of the gear with a hammer and punch. When we reassembled the planetary gear system, this time, it worked properly, and we replaced the stanchion, and the chain drive and tested the dive with the engine. All was well.

The winch had broken down at 4 p.m, and it was 6 a.m. the next day that we could again try to recover our wire rope. First we had to put a block of wood in the eye of the kink, so that as we took back the strain of the wire rope on the winch it would not make a sever kink. Thank goodness we had the torque convertor, because it allowed us to take the strain back on the winch in a creep so that we did not make a severe crimp in the wire rope. As we took up the strain very gradually, the kink untwisted until the last twist opened and the block of wood was ejected. When we had the strain on the winch, we removed the cumalong from the wire rope and the A-frame and we were ready to hoist again. We recovered the wire rope at very slow speed since we were worried about the strain on everything. At noon we finally got the biological dredge on board. It was in shambles, all bent with several broken members. The contents of the dredge was lost. Of course, it had been dangling at a depth of 4000 fms. in a seaway for about 14 hours, and spent about 6 hours on the return to the surface. We were lucky we had calm weather throughout our repair efforts, as we had to lift the heavy members of the winch with a chain fall attached to the boom of the main sail. In even a moderate sea, we wouldn't have been able to attempt the repair!

We sent a message to the lab that we had succeeded in repairing the winch, and that the bio trawl was destroyed. We never did get our biological trawl in the Puerto Rico Trench, as we had used up our time on the repair and we had to go into port to meet Wissama. We continued using the replaced shaft throughout the remaining 8 or 10 months of the remainder of the cruise.

Shortly after starting the seismic work, the Wissama radioed us that John Hennion, Chief Scientist, was sick and running a fever. They didn't know how much as they did not have a thermometer. We ran down to them and transferred a thermometer to them. With this information and a description of the symptoms we radioed the coast guard for a health estimate. They advised we get him to port as quickly as possible, so we sent Wissama back to Puerto Rico and transferred John to a hospital. John had been the Chief Scientist on Wissama, so I had to make Charlie Bentley Chief

Scientist. At that t time Charlie was in his earliest years as a grad student and it was a big responsibility for him.

While they were gone we carried out work in the Caribbean. While using the hydrographic winch it became apparent that the flanges of the drum were dragging on the frame of the winch.This meant, either the drum had stretched, or broken allowing the flanges to contact the frame. We let all the wire out with only a small weight on the end, while the ship continued to travel at cruising speed. This was the only way there was any possibility to save the wire rope, which cost a couple of thousand dollars, a large sum to us at that time. We tied the wire rope off, with a cumalong, at that A-frame. We removed the drum from the winch after determining that the core of the winch had stretched and thinned. Oceanographic ships of that day, often had problems with the drum flanges, as designers didn't realize how much the wedging action of the multiple layers of wire rope had on the flanges.We took the drum down to the shop and cut a section out of the core by jury rigging the cut-off saw in the shop. After removing an inch of core, I welded the two halves back together. We strengthened the core by laying strips of metal along the core and welding them in place. These were placed close enough together around the circumference that the wire rope was not damaged by bridging the gaps between them. We then replaced the drum on the winch, reattached the wire rope and recovered all 3000 fms. We used that repaired winch drum for a number of years before we built a much improved hydrographic winch.

When Wissama returned, we continued the seismic refraction work arriving at Coca Solo only about two days later than planned. I was glad to be relieved as Chief Scientist on Vema and to return home for a rest. Before the next cruise, we had the shipyard help us with the winch repairs. They did some damage to the gypsy head in reassembly. They did all the final winch assembly. I was sent on the first leg of the next cruise to test out the winch operation. On our first coring station all was going well on recovering the core, so while recovery continued, I went to the cabin top where the hydrographic winch was located to check on the bottom photography. I had been there only a short while, when the trawl winch started to run away. Everybody ran away from it as it was quite dangerous. Rust and other debris was being thrown in the air, sparks were flying, and the wire rope was accelerating in the outward direction. I slid down the ladder from the cabin top, ran to the trawl winch, assessed the situation, which by now was getting quite desperate because of the speed of the wire rope. I saw the clutch brake was slipping and grabbed the big wheel which tightened it and started tightening it. Gradually the wire rope again came under control and finally I was able to stop the pay out.

After a short break for everyone to recover our calm, I tightened the clutch brake as much as I could and we continued recovering the wire rope and core, slowly at first, and gradually returning to our usual speed of recovery. We all breathed s sigh of relief, because a runaway winch was no joke. With a load of core and wire rope of about 10,000 pounds accelerating the payout, the winch would;d have reached speeds where it might soon have disintegrated, throwing parts around at great speed. Eventually the wire rope would have parted, and we knew when this happened the ends would have whipped around at high sped and damaged anything it might have hit, and certainly killed anyone that it might have touched. After this experience, we never let anyone work on the trawl winch but our own staff.

About this time, we borrowed the temperature probe that Sir Edward Bullard of Cambridge, England had developed to make measurements of heat flow from the ocean bottom. We were unsatisfied with it because it took as long to deploy it as it did our core rig. Thus, it added an extra two hours of time to a bottom station for only a small gain in data. We soon had to return it to Sir Edward, anyhow. Doc set Mark Langseth to developing equipment to attach to our coring rig to make the same measurements.

While all of these things had been going on, my students and I were continuing with the gravity measurements at sea on submarines. This often resolved down to me making arrangements with ComSubLant, or ComSubPac for a cruise on which we could make measurements. Often these were cruises to other countries for establishing our capability to operate worldwide. I would go with a student to New London, Norfolk, Key west, San Diego, or Pearl Harbor and help set up the equipment in whatever space the CO made available to us. Often it was the ammunition locker. They would off load the ammunition and we would install our equipment during their outfitting stage for the cruise. We had to bring the equipment down one piece at a time through the hatch in the crew's mess. Some, we could lower the 20 feet from the deck on a rope, but the most sensitive parts we would have to hold in front of us, with one man on deck assisting by paying out rope, and carefully descend the ladder. It usually took us about three days to load the equipment, assemble it and get it working. We would take one day to make the necessary base observations before the cruise. This was difficult because of the vessel bumping against the dock. Sometimes we had to make the observations at night when things quieted down on the sub and harbor traffic was at a minimum. Similar observations were made at any port stop that might be made during the cruise, and at the completion of the cruise, all of the food stores had to be loaded on board through the same hatch, and we often had to dodge those operations.

Removing the equipment at the end of the cruise was even harder, as we had to lift it up through the hatch. On a number of cruises, I could not go on the cruise because of other duties, and the observer, usually a student, would have to make all the sea observations. The records were made photographically and had to be developed at the completion of the observation. We had made a development box with three stainless steel pans to hold the developer, the wash bath, and hypo. After the record was stabilized in the hypo (about 5 minutes) it was moved to a bucket of water to wash for at least 15 minutes. Then it had to be hung up to dry for at least an hour. The records were marked and rolled up for later reading and computation.

Observations were usually made at six hour intervals while in transit. Some Captains would let their executive officers make the midwatch dive during the night so they could get their proper night's sleep. Others would not, and we would have to skip that observation so we did not cause the captain to get too tired. Our observer (s) would get their sleep between dives and work on the records between observations.. Thus on a transit we would get an observation about every 100 miles. The submarines of those days mostly traveled on the surface, only diving to hide, or in our case to make observations. They made many more dives for us than they would have just doing their own operations. Most captains used this as a training opportunity. Every man on board had to be trained to do every job. Usually this was hard to accomplish, but on our cruises they had ample opportunities and their men all became proficient in each operation. I tried to get some of the captains to write a letter explaining how our cruises helped in their men's training, but I was never successful. They were missing out on their regular training and they were afraid that their commanders would use the opportunity to point this out. On each cruise I would have to take our observer and with the captain make at least a courtesy visit to the squadron commander, and sometimes the division commander. I of course took this opportunity to thank them for their assistance. Most were very cordial. Occasionally one would comment on how much our operations interfered with training and that only orders from higher up were obtaining their cooperation. At the conclusion of each cruise, I would write a letter to the squadron commander expressing our appreciation for the hospitality and assistance that we had had on the recent cruise. Copies were sent to their division commander, ComSubLant or ComSubPac and CNO.

The submariners were the best group of people that I ever had the pleasure to work with. They were always courteous, kind, and considerate. They were friendly and welcomed us into the officers wardroom, even though our work on our records slightly restricted their use of the wardroom. The meals as a rule were consistently excellent, especially Sunday breakfast (steak and eggs). It was truly a highlight in the life of each observer. Meanwhile we were not neglecting the interpretation of the gravity data. First in the Puerto Rico Trench we had determined by seismic means that there was a great thickness of sediments. With this information and the gravity data we were able to compute the location of the base of the crust. This killed the theory of the Tectogene which had been accepted for about 30 years. There was no large downbuckle of the crust beneath the Puerto Rico Trench. In 1953 I arranged for the U.S .S.Diablo to make a much more detailed gravity profile across the Puerto Rico Trench . The profile was extended into the Caribbean sea for a couple of hundred miles. With this profile we were able to improve our profile across the Puerto Rico Trench and to show that there was a similar trench filled to the normal depth of the Caribbean Sea with sediment. This trench was not quite as deep as the Puerto Rico trench, but it also did not have any Tectogene. Because of our admiration of the work of Dr. Vening Meinesz, we did not make a big ballyhoo of the demise of the Tectogene, his invention. This passed with few people taking much note of it.

The second important paper at this time was the first paper on the structure of the continental margins. Using the seismic results along the Atlantic coast of the U.S. and combining the gravity data with it we were able to delineate the transition of the continental margin into the structure of the deep sea. This transition occurred in a distance of 100 km and was centered on about the 1000 fm. curve. Seismology had long determined the crust-mantle interface at a depth of about 30 km. Our sea seismic data had for several years indicated oceanic crust- mantle interface to be at a depth of 10 km. Our work along the Atlantic coast had delineated the sedimentary interfaces above the basement. We used the gravity data along these same profiles to delineate the crust-mantle interface, connecting the two.

The third important paper of this period was to establish a standard oceanic crust and continental crust. We surveyed all of the seismic results in a normal sea depth of about 5 km, six in number and averaged them to make an average sea crustal section. This turned out to be 5.0 km of water, 1.0 km of sediment, and 4.5 km of oceanic crust underlain by a mantle with a seismic velocity of about 8.0 km/sec. We took a density of 1.03 for sea water, well known, an average density of 2.30 for the sediments ate sea, and a density of about 2.84 for the crustal density and 3.27 for mantle density. These densities were chosen on the basis of their seismic velocities and according to the Nafe - Drake velocity - density curve. We then applied the same analysis to seismic results for sea level continental results. This led to a density of 2.84 for continental crust and 3.27 for mantle. Seven continental sections were available. These two columns were in isostatic equilibrium. It had been known for years that this equilibrium existed. With these standard columns we could now work out crustal sections in intermediate places using the gravity data to establish their sections. We then applied these concepts widely to the gravity data we had been accumulating. We established the depth to the crust-mantle interface in a number of locations around the world. When the seismic measurements were made in these locations, it was found that our results were quite accurate.

Thus we established the interpretation of gravity anomalies in terms of geologic structure. Up to this time, gravity data had almost entirely been interpreted in terms of the isostatic balance of large regions. Soon others were interpreting gravity data in terms of geologic structure also.

I find I omitted an important item that occurred in the period between 1946 and 1950, when I was a graduate student. I am including it here.

Doc came back from an expedition and complained that he had great trouble with the bottom cameras. They were operated by touching bottom. The problem was that the switch which was supposed to operate the cameras didn't always work. It was a spring loaded toggle switch which was encased in rubber which was filled with oil so that the rubber didn't collapse into the switch mechanism causing failure. A weight was hung on the switch which was relieved when it hit bottom allowing the spring toggle switch to turn the current on thus operating the camera.

The problems were oil leakage, water leakage into the switch shorting it out, and fouling of the cord between the trigger weight and the switch. These didn't all happen all at once, of course, but they happened frequently enough to frustrate the bottom photography too often. Doc explained these difficulties and stated that we had to find a way to make the bottom contact switch much more reliable, say 100 %..

I was presented with this problem about supper time. I had mysupper and went to my room to study. I found studying impossible because Ikept going over in my mind possible solutions to the problem. I had not got a satisfactory solution by about 11 P.M. so I went to bed. This too was frustrated, because I couldn't turn my mind off. As I lay there it occurred to me that we used six volts to activate the various cameras that we had built. Why couldn't we put four flashlight A cells into a tube and add a switch In series with them and we would have the battery pack and the switch all in one parcel. This suggested that we needed a switch almost identical in diameter to an A cell thus making the buttery pack only slightly bigger. Finally it occurred to me that micro switches of that period were about 1 3/4 inches long. about 3/4 inches wide and about 1 inch tall, not. too far different to the size of an A battery. All we had to find was a way to activate the micro switch. This only required a push on a pin of about .005 inches. It seemed to me that it would not be too hard to have an exterior magnet trigger a piece of iron so that it would push the micro switch pin. This could be arranged be making a toggle with in iron weight on one end to be attracted by a magnet so the other end would push on the micro switch. I thought about that a while and realized that almost any movement of the assembly would cause the toggle to operate the micro switch because of its inertia. This led to the concept that the toggle should be made with an equal weight of non-magnetic material at at the same distance on the other side of the toggle. Both weights would have the same inertia and motion would be canceled. A light one leaf spring would keep the toggle from pressing on the micro switch trigger until the magnet would overcome the spring tension. With further thought, I realized that this whole mechanism could be made exactly the size of one of the flashlight batteries, and the battery case would have to be lengthened enough for one additional battery. This device served us for the rest of my years at sea.

After receiving my doctorate in 1949, I had been made an instructor by the Geology Department as Doc's assistant. Frank press also received an identical appointment. In 1950 Frank and I were appointed as Assistant Professors in the Department of Geology. That was about the time that the events described on page 162 occurred. It must have been a hard time for

Doc because he had to get identical appointments for Frank and I so that neither of us was alienated.

In 1952 Frank and I were both elevated to Associate Professor. This was about the time that the events described on *page170* occurred. Again the double appointment was very difficult.

In January or February of 1954, Vema departed on a cruise with Dr. Ewing and John Ewing aboard. It was a stormy time and about a week after Vema's departure I received a call from the Coast guard stating that the Vema had made a distress call and that she was dismasted, with the mast still attached to the ship by the rigging and was in serious trouble because of the storm causing the ship and the mast to collide. They believed several men had been seriously injured and that possibly several lost. The ship was about a couple hundred miles north of Bermuda and they were trying to send aid. I didn't believe it because, other than a submarine, Vema was the strongest ship at sea. She had frames at 12 inch spacing, rather than the conventional 18 inches. The frames were made of angle iron 3/8 inch thick, while most ships were made with only 1/4 inch. Furthermore she was rigged in similar beefed up fashion.

Apparently the Coast Guard made a news release with this same story, because I started to get phone calls from relatives of scientists and technicians on board asking if their relative was all right. I had to tell them that we were trying to get in touch with Vema and get the correct story, and that I did not believe the Coast Guard report because Vema was so strongly built, promising to let them know as soon as we could get any reliable reports from the ship. Late in the day, I received a radio message from Vema stating that four men had been washed overboard and three had been recovered, and that some damage had been done to the ship but that she was on her way to Bermuda. Because of the weather they believed it would take them about two days. We did not know who had gone overboard. I called all of the people that had called me and reported the message from the ship and that we did not know who was lost. I left for Beremuda so I would be there when they arrived.

In retrospect, I should have called all of the families as soon as I had received the message from the Coast Guard, instead of waiting to get accurate information first, which was my intent.

When Vema arrived in Bermuda, I learned the following: Vema had run into a severe storm at sea and had been laying hove too most of the night. About 7 in the m,morning, just about daylight, Doc (Ewing) came on deck near the after laboratory, when a much larger than usual wave came on board causing four 50 gallon drums of fuel, Vema was carrying on deck to break their lashings and start to slide around on deck. At about the same time, the first mate observed the drum lashings to fail. The mate went back in the pilot house an got the second mate to come help to get the drums under control. Doc went back in the lab and got John Ewing, who was just coming topside, to help. The four of them, with heroic efforts got the drums back at the rail on the main deck, and relashed them to the rail. Doc later told me that he had just said "there they will never come loose again", When an even larger wave came on board, broke the new lashings and took the four drums and the four men overboard.

Capt. Donald Gould witnessed the men going overboard and called all hands on deck to search for them. Doc had prevailed on Capt. Mac Murray to sail on the cruise as sailing master. Capt. Mac Murray had retired, from the Atlantis of WHOI, to the sailer's Snug Harbor, a retirement community for sailers. He agreed to go on this cruise because Capt. Gpould had had little sailing experience. and needed advice and help to handle the ship under sail. Capt. Gould put the bridge in the hands of Capt. MacMurray and climbed the mast to overlook the thirty foot tall waves. By the time everyone was on deck. the ship had been blown quite far from the barrels. The ship had to be turned to return to the area where the men and the barrels were. Capt. Mac Murray later told me he had serious doubts that the ship could be turned in such a seaway, but he had to try. During the turn, when the ship was broadside to the waves the rolling was severe rolling at least 70 degrees from the vertical . Doc later told me that he was watching the ship when he was in the water and that he could see the whole side of the ship's bottom when she turned. After about twenty minutes they got back to the barrels and hove too, to look for the men. The second mate Fulton Brown was spotted and quickly brought aboard. By that time the ship had been blown quite far from the barrels again and they were about to get underway again, when one of the deck watchers saw John Ewing about 20 feet alongside. When he was brought on board he said he had been shouting at the top of his lungs, but could not be heard above the noise of the storm. After he was on board the ship was turned again and returned to the vicinity of the barrels. Doc was spotted after awhile and they threw him a line so he could get over to the vessel. He could not pull himself nearby so the ship had to be maneuvered closer to him. When he was alongside, a severe roll brought the rail down to the water surface, and one of the men on deck got his arms under Doc's armpits and he was hoisted out of the water by the roll of the ship. Several people assisted in pulling him on board. He couldn't speak, and was pyrolyzed on his left side. They quickly got him below and in a bunk and under some warm blankets. He had been in the water about 40 minutes. The ship searched for a couple of hours, but never found the first mate, Charles Wilkie. By that time the barrels had separated considerably and the ship could no longer find them. Reluctantly they gave up the search and lay too until the storm abated. Then they headed for Bermuda.

If the barrels had not gone overboard, it is probable that noone could have been saved. They served as a marker that Capt. Gould could keep the vessel searching in the right area. John Ewing told us that when he went overboard, he came up near the log line towed from the stern of Vema. He grabbed that line and was towed away, but the strain got too much for his hands and he had to let go. It was just luck that got the ship to lay too for a short while near to John. Doc said that he had talked to the first mate just after surfacing, and the mate asked him to grab the other end of the barrel. He couldn't and that was the last anyone knew of the mate. It was about two days after the accident that Doc recovered his voice and could tell of his experience. He said that he hit his head on something as he went overboard. He didn't realize that he was partially paralyzed until he tried to grab the other end of the barrel.

A memorial service was held for Charles Wilkie in the cometary at Bermuda. All of the people on board, most of the people at our Bermuda Sofar station and I attended. Doc could not attend as he was still kept in bed by the doctor's orders.

The ship suffered quite a lot of damage. The things I especially remember is the split paneling if the strong deckhouse of the lower lab on the main deck. the stairs (three steps) ripped from their fastenings on the main deck and the poop deck, and the bent turnbuckles of the after mast rigging. The stair fastenings were still attached to the main deck and the poop deck, with chunks of 1 1/2 inch teak literally ripped from the side panels of the stairs. The turnbuckles were bent right next to their barrels where the screws 2 inches in diameter were bent about 10 degrees. The force of the wave against the taut turnbuckles to bend them that m,much is hard to imagine. It's no wonder the tripled 5/8 inch in diameter ropes that were used to lash the drums to the rail parted.

It took about a week to get the necessary repairs to the ship made. It probably would have taken more than two weeks, but Gordon Hamilton, who was in charge of out Bermuda Sofar Station knew the local people and shops that could help us. Doc insisted that the ship get underway as quickly as possible, because we had made arrangements to work with on of the oil companies in the Gulf of Mexico a few weeks later. Doc was not able to go along, so we sent him back to New York to recuperate and I went on Vema as Chief Scientist. Gordon Hamilton came along with me. The next few weeks were very hard, because when anything difficult came up the group on board would tell me that had I been on board during the accident I wouldn't be asking them to do that. Gordon Hamilton was a great help since Gordon and I pitched in and pretty soon the others would join and the work got done.

Vema arrived in Galveston in time to make our appointment. Doc was standing on the dock when we arrived. He didn't look too well but he had recovered the use of his left side and had a slight limp in his left leg which lasted the rest of his life. Gordon had to return to Bermuda. I thought I should go along , but Doc insisted that he could manage all right and insisted that I return to Lamont to carry on my work interrupted by the accident.

Because of the accident, Doc had been interviewed extensively by the news media. In the interview with the New York Herald Tribune, he was quoted as saying that the greatest development in earth science would result from drilling the sediments on the ocean bottom.

The Geochemistry program had expanded at LGO to utilize the whole kitchen complex of the main house that I started to call Lamont Hall. Pretty soon everyone was calling it that. Larry Kulp, who was in charge of the Geochemistry program, wanted to expand further to carry out additional geochemistry programs, but there was no room. Larry went out and raised enough money to build the Geohemistry Laboratory just beyond the former swimming pool building. This was dedicated in the fall of 1954. We had erected a tent nearby to house the ceremonies as we had no inside room large enough. A hurricane was threatening at the time of the dedication, but the dedication went off and the tent was removed, as the hurricane moved out to sea instead of coming ashore. We only got a deluge of rain.

Part of Doc's arrangements when he moved to Columbia University was some money for some equipment, he ordered a Frost gravimeter. The gravimeter was nearing completion as the summer of 1947 approached. Doc sent me down, now a graduate student,to Tulsa, where the Frost Company was located to learn as much as possible about it so that we could keep it operational, and take delivery. At that time Frost was making gravimeters for the oil business that could measure gravity to a few hundredths of a milligal (1 milligal is 1 millionth of the value of g). Since we didn't need such accuracy Doc had asked that a stronger measurement spring be installed that would let us get 1 milligal accuracy. The regular gravimeters had what was called drift-that is measurements of a single station at different times would slowly change. To counter this effect they had to return to one base frequently to calibrate the drift, or to leap frog, that is measure station a then go to b then return to a, return to b then on to c, then return to b and so on. These return measurements would allow one to determine the drift. A by-product of the stronger spring was that our meter had no drift. To my knowledge, for many years, this was the only gravimeter with that attribute. George Woollard borrowed it a number of times to take advantage of that property.

On my way to Tulsa I picked up a hitch hiker about 200 miles before I got to Tulsa. After we had talked a while, he told me he was a pilot. He suggested that we stop and I should rent a plane and he would give me a ride. I of course had very little money and was in a hurry to get to Tulsa, so I declined. He became quit insistent, but I continued to decline. Finally he let it drop. As we came close to Tulsa he started to tell me all about Tulsa and what there was to see. Doc had arranged for me to stay with John Owens, who had participated in the small print cruise with us during the war. I had John's address, and I made the mistake of asking my hitch hiker how to get there. He told me. When we got to Tulsa I said that I would leave him at my turnoff. He insisted that in town he would have trouble getting a ride and that I should take him across town. I reluctantly took him to a bridge where the highway left town on the other side. He then insisted that I take him across the bridge. I said no, that I had come as far as I was going to go and that he should get out.

I returned to John Owen's house. They greeted me and made me welcome. I explained my mission in Tulsa, and they insisted that I should eat dinner and breakfast with them while I was there. The next morning, a Monday, I went out and found that my car had been broken into and two altimeters that I had left in the car, which were government furnished property, had been stolen. I reported the break in and theft to the police. They questioned me and it was decided my hitch hiker had either done it, or just another street theft since I didn't know anyone but the Owen's, and noone else knew I was there. A couple of days later the police returned the altimeters to me saying they had been found on the bridge at which I had left my passenger off. They were damaged and it appeared that they had been thrown out the window of a passing car. Needless to say, I never again picked up any hitch hiker!

At Frost's they turned me over to the machinist that built the gravimeters and he gave me thorough indoctrination in how the meters were put together and how to use them and maintain them. At that time the gravimeter was a cylinder about 14 inches in diameter and 18 inches tall. It was set on a leveling platform which was mounted on a tripod. There was a portable tripod, and another that was added to my car in place of the seat next to the driver. The idea was that the car could be stopped, the tripod lowered through the floor and set up on the ground being totally separated from the car. The meter could be put on the leveling platform, leveled, the instrument released from its clamps and after giving it time to settle, a dial attached to the reading spring could be adjusted so that an image from the gravimeter beam viewed through a telescope could be brought to a mark in the center of the field of view. The reading of the dial would tell how much the spring had be stretched or released to achieve the match of the gravity field . It was necessary to make observations at a number of stations where gravity was known, to calibrate the dial, so that the readings could be translated into milligals.
The meter was in a pressure proof case so that it would not be affected by changes of barometric pressure. The case with the meter was surrounded by a heater and insulation so that the temperature could be maintained constant. A lot of the bulk was due to the insulation. The temperature had to be elevated above any air temperature that you might experience so that the temperature could be maintained as there was no way to cool the instrument. Ours was set at 109.4 degrees Fahrenheit with a mercury thermostat. It could be maintained within 0.1 degree.

The Frost company had a calibration range that they used for their meters. I was sent out to calibrate our meter. While I was there, to the surprise of the company, our meter did not drift like their regular meters did. It was judged that the weaker spring that they used in their regular meters were over stressed which is what caused them to drift. Our meter with a stronger spring did not have to be over stressed and hence had little or no drift. The trip over their calibration range was not very successful because it used only such a small portion of the dial, with our stronger spring, that the calibration was inadequate.

We had anticipated my making gravity measurements on my return to Columbia, so I had a manual of gravity measurements made by the Coast and Geodetic Survey across the united States. They had included directions about the location of each one. I stopped at each location along my route back to New York, including a visit to the National Gravity Base at the

Commerce Building in Washington, D.C. While I couldn't reach the actual National Base Station, because it was a Saturday afternoon, I could reach the door at the street nearest the National Base. George Woollard had made sufficient connections with the Base and the doorway that I could make the connection with my group of measurements. These measurements gave us a good calibration of our meter. Our connection with the National Base Station gave us a good value of gravity at Columbia where we made our pendulum base observations for our submarine gravity cruises. We also took the meter to Woods Hole where we made connections to the base we had used for our submarine gravity cruises based at WHOI. When we moved to Lamont, I, established a base in the basement where I could make observations with the pendulums used on the submarines. Taking the cue from my problem at the national base, I also set up a base station just outside the basement door of Lamont Hall for the use of others who might want a gravity connection when they could not get into the building.

Since we could not get an elevation with sufficient accuracy from the topographic maps of the area near Lamont, I had to find a Coast and Geodetic Elevation marker near route 9W. With a surveyer's Transit, one of the students and I ran a level line from the elevation marker up the hill to Lamont Hall. I established the elevations at the pendulum base, at the base just outside the basement door of Lamont Hall, and an outcrop of the Palisade Diabase on the north lawn of Lamont Hall. We drilled a hole in the Diabase and had the shop build a brass marker which we cemented into the outcrop, marked with the elevation.

The Coast and Geodetic gravity bases were all marked with brass plaques, usually in a concrete monument that extended well below the ground surface. These were identified with a number and a warning not to move or deface the marker. Sometimes these took a lot of searching to find. Landmarks had changed in the 20 to 30 years since they had been established. In one town it was described as being at the post office. At the post office, it was apparent that it was a fairly new building and could not have been there when the base was established. Enquiry in the Post Office gave us the address of the previous Post Office. When I arrived there I could not match up the directions with any of the landmarks. I finally found an old man and quizzed him to see if I could find the proper spot. After a little bit he said, "You must mean the previous Post Office. He gave me that address and the landmarks checked and I found the marker.

On another occasion the marker was in a community park. I searched for about a half hour for the marker without success. I finally found the park caretaker and explained what I wanted. He told me that they had updated the park a few years ago, and they had found the marker near the baseball filed and seeing the warning not to deface it they had dug it up and replaced it across the baseball field where it was not in the way. Naturally I could not use that one.

In the gravity work at sea, I had been trying to compute the gravity attraction of various sections to interpret the geology existing where the observations were made. In the recent past, most gravity interpretation was in terms of whether there was isostatic balance, and if not, what degree of imbalance was present. To make geological interpretations it was necessary to use such knowledge that was known in the area and account for the gravity differences by the remanning undetermined layers present. This was greatly assisted by the paper published by Nafe and Drake that established the relationship between the seismic velocity and density . With that curve I could take the seismic refraction results and compute the gravity attraction , and subtract that from my observed values of gravity which made the residual gravity due to the deeper layers that had not been observed seismically.

It soon became evident that I would need a standard continental section and a standard oceanic section. Since elevation made a difference, I needed a standard section for sea level elevations for continents. I surveyed all of the observations of continental sections and picked up all those of elevation very close to sea level. After correction for the slight difference from sea level to the elevation it became obvious that average continental density was 2.67, and the average Moho density was 3.27, and the continental crust was 30 km deep. In the ocean I took an average of all the observations in normal ocean depths, ruling out the deeps, continental margins, island platforms and sea mounts. The density for sea water was established at 1.03, for sediment 2.30, for oceanic crust was 2.67, and for Moho was 3.37.

Using these standards Lynn Shurbert, who had helped with the standard section work, and I were able to compute sections across continental margins, filling in the ares where seismic refraction had difficulties, compute crustal sections at continental margins where we had gravity data, but there was no seismic data, for island and sea mounts, for mid ocean ridges, and even for the ocean deeps. To the surprise of many people, the sections we calculated from gravity data agreed quite well with later observed seismic sections.

At about this time, I had gone to sea on Vema since it and a lot of our scientific gear that had been greatly changed after the ship's annual overhaul. Since i had a lot to do with the overhauls, I felt that I had to go to sea with the changes and made sure that they all worked. This time, there were no significant problems.

One of the observations that was needed was a core beneath the Gulf Stream. We had many in the main ocean basin, and quite a few on the continental margin but it seemed some problems could only be settled with cores beneath the Gulf Stream. Bemuse of the speed of the Stream, 3 to 5 knots, the ship could not just be left to drift while a core was taken, as we normally did. I had been asked to try to get a core beneath the Stream. South of Cape Hatteras we had a spell of good weather and I decided to try for a core while we were in the Stream.

By the time the core had reached depths greater than 100 fms. the wire rope started to take on a large wire angle. I decided that this probably meant that the core equipment was falling in the part of the water column that was not affected by the Gulf Stream, while the ship was being dragged away at about the speed of the stream. It soon became apparent that just continuing to pay out the wire would not work, because that even though we were in water depth of only about 2000 fathoms, (12,000 ft.) our 30,000 feet of wire rope would not reach the bottom, and that extracting the core from the bottom if we just did reach it, would probably not succeed because of the large sidewise pull we would have when we started to recover the wire. Also, it was doubtful that we could tell when we had hit bottom under these circumstances.

I stopped the wire pay out, and requested the captain to get the ship underway at about 4 knots steaming in the direction of the wire angle. When the wire was again vertical we started to pay out the wire rope. I kept furnishing the captain the direction the wire rope was leading and whether the wire angle was changing. By adjusting the speed of the vessel and the direction of steaming we were able to keep the wire nearly vertical. We were steaming about 3.5 knots! We had no difficulty telling when the core hit bottom, or extracting it from bottom. When we had recovered all but 100 fathoms of our wire, we stopped steaming and continued to recover the corer. We had recovered about 30 feet of core.

The core did not get back to Lamont until many months later, and I never heard whether it solved the problem that caused the attempt to be made. So far as I know, this was the only core that was ever taken from beneath the Gulf Stream. This was 1954. We had been using the broken down pier at Pieermont. It had been used as the embarkation pier for the soldiers in Camp Shanks during WWII. The paper board company in Piermont had acquired the land leading to the pier and the pier. They were not using most of the road out to the pier or the pier. I approached them and persuaded them to allow us use the road and pier without cost. They put up a fence across the road beyond the section they were using and insisted that we keep the gate locked when we were not using the pier. They had had trouble with people using the pier as a place to fish and when they got cold, lighting a fire to keep warm. They had had to put out several fires since they had owned it.

We had used the pier as is but we could not bring vehicles out on the pier because of the condition of the surface. There were even several places where there was danger for a person on foot putting his foot through some of the rotting planks. The paper board company told me that we could make repairs to the pier if we wished, but that they would have no responsibility for the work or the cost or the repairs. Since we didn't have any crew that could undertake such work I searched around until I found a National Guard engineering unit that would undertake the repairs as a training exercise, if we would purchase the materials. We agreed to this and for a number of weekends they made repairs to the central section of the pier out to the water front. They erected rails along the sides of the repaired section to try to keep people off the remaining dangerous sections. They did such a good job that we could now back up our trucks to the rail of the ship tied alongside to unload.

The next problem was electricity. We had to keep one engineer on board to keep the ship's generator running so that work and dockside crew could live on board while the ship was in port. This was expensive, and prevented any work to be done on the generator for its maintenance.
I approached Orange and Rockland Power Co. to bring a power line out the mile of road to the pier from their nearest power lines. They investigated and advised that they would have to put in a number of poles for a power line as well as the line itself. They could only bring out the line, but we would have to get a private contractor to install a power panel and connection service. In order to do this, they had to get a signed statement that we would use a certain amount of power each year. I signed the statement, but I never knew whether we used all the power we were required to. The power company never raised the issue, so I guess they were satisfied. Perhaps the President of the power company had something to do with it, since he and I both served on the council of the Rockland

County Boy Scouts.

Shortly after we acquired Vema we had Burmeister and WAin, who had made the engines, to send a work crew to get the engines in first class shape. They did and then told me that these engines were quite old and that few people knew how to maintain them. They suggested we get a qualified engineer to be our chief engineer. I asked them for a recommendation of a suitable person. They recommended Hank Skjerding, so we hired him. After a couple of years he had got the engine room in first class condition and trained the second engineer in their care and maintenance. He came to me and said he no longer wanted to go to sea, and the second engineer could handle the engine room now, so he would like to leave. I asked him, to stay on as our port engineer to supervise the annual overhaul of the ship and the other problems to keep the ship operating. I had been spending too much time on ship problems and too little on my science studies. Fortunately he agreed, and things ran much more smoothly afterwards. Hank made sure the ships operating gear was kept in top shape, made most of the arrangements to obtain bids for the annual ship's overhaul, and supervised the the shipyard work of the winning bidder. I took care of making sure that Hank was properly informed of any ship modification s that were needed for the scientific work. When the ship was in the shipyard, I went down at least once a week to see how Hank"s supervision was working and to provide answers about any changes needed for the scientific work. A year or two later we had to hire a Port Captain to make arrangements for port agents at ports where the ship went in to refuel and revictual. Sometimes some changes to the scientific party would be made at port stops.

After a few years the engine maintenance got too high. We were able to get ONR to get us a 1200 hp. GM (1200 rpm) diesel from Navy surplus and have it installed in VEma to replace the massive Burmeister Wayne 300 rpm diesel.

We had had the teak decks caulked several times but could never get them to be watertight. The scientists berthed in the tween decks had put up shower curtains above their bunks to try to keep dry. Some accumulated more than 5 gallons of sew water in them and posed a considerable problem to remove the water. When we were replacing the engine, we convinced ONR to let us have the teak decks removed and replaced with steel decks. The deck house that had been damaged by the seas when the men went overboard, was replaced with a Steel cabin, and the after lab was replaced by a two level scientific laboratory. We had been getting quite crowded with all the additional programs in the lab. The steel decks were quite slippery and Hank found a material called polycrete, which was an epoxy which we used to coat the deck. Sand was mixed in the polycrete to make a rough surface for good footing. This turned out to be very tough and could absorb quite a lot of shock from some of the heavy gear which was dropped on it. It was easy to repair when we accidentally damaged it. On several occasions we had to repair section of about 3 ft. square. We found the steel decks under the polycrete without any rust.

Vema had a full feathering propeller on it when we got it. To reverse the ship the propeller was feat. When we replaced the engine , we had to have a reversing reduction gear put in behind the engine. This meant that the 70 foot long shaft had to be replaced. Hank was a god-send to arrange for these changes and to make sure they worked. While the changes were underway he told me they had a serious problem. The 6 inch in diameter propeller shaft had to be made in several pieces, and there was a serious problem to align them. Having had the winch problems I suggested that he use spherical aligning ball bearings like we had used on the winch. These could accommodate a shaft that was not perfectly straight. Hank obtained suitable bearings, and we never had any alignment problems.

In late 1953 I was called to a classified conference in Washington. They had established the base for firing rockets at Cape Canaveral. They wanted to establish a range of about 3600 miles for testing ICBM's. They wanted the range over ocean so that if anything went wrong they would not endanger any population. It had been decided that they would need the range to end near some land so they could track the rockets on their descent. It had been decided that a target area near Ascension Island in the South Atlantic would be desirable. Rockets were steered by inertial guidance systems, and it was understood that possible deflections of the vertical near Ascension Island might affect the final surface point where the rockets hit.

Also at the meeting, there were some geodesists of the C&GS (Coast and Geodetic Survey), the experts on deflection of the vertical computations. It was quickly established that gravity observations would be needed in the vicinity of the target area. I pointed out that the track would pass close to the Puerto Rico Trench for at least 600 miles and that the well known deflections of the vertical there might well also affect the inertial navigation. I was asked if we could make gravity observation to fulfill the needs, while the Coast and Geodetic Survey would use them to make determinations of the deflections of the vertical.. We agreed that we would.

After some discussion it was decided that we should make four tracks on a swath of about 120 miles wide on a great circle course from the windward islands to Ascension Island, and a pattern of observations around Ascension Island on various radii. Two submarines would be assigned for the purpose, each making a passage to Ascension Island, about half the observations near the island and a return passage.

On my return to Lamont, I plotted out four tracks to Ascension Island and a star pattern around Ascension Island. I estimated that it would take two months for each sub to make their trip. I sent the proposed track to the C. & G. S. They made several suggestions of changes, which I made and then sent the proposal to the Navy. It was approved and USS Archerfish was assigned foe the first cruise from early January 1955 to the end of February. USS Balao was assigned to make the second cruise from early March till the end of April. Both ships were to start from Key West and return to Key West.

Since we were to leave early in the New Year and Archerfish had Christmas Holiday in upkeep, we were able to install our gear about mid-December so that we could leave on the first operational day of the New Year. Ken Gilchrist and I went as observers. As usual, on our gravity cruises, we would set the gimbals free and lower the pendulums as they submerged the sub and stabilized, usually at periscope depth. When the word was passed that stability had been reached, we would start the pendulums swinging. After a 45 minute observation, we would stop the pendulums, notify the officer of the deck that the observation had been completed, secure the pendulums and the gimbals and develop the record while the boat surfaced. Normally we observed a time signal before and after each observation from WWV in Washington to assure that the crystal clock was keeping correct time. After the record was washed, we would hang it up to dry in the magazine, which had been emptied to accommodate our equipment. This was a compartment about 8 by 8 feet and a little more than 6 feet high below the main deck. Between observations we'd work in the ward room on the records which had been dried earlier. Usually we took six observations a day, equally spaced in time. Ken and I shared making the observations. This was a hard cruise because we were crossing a time line almost every other day for the fifteen days in transit. Going east this meant shifting our days work and eating and sleeping one hour earlier every other day. We found it hard to adapt our biological clocks. Near

Ascension Island we would make six observations centered on the island along each 45 degrees in azimuth. The observations were spaced on an expanding scale out to about 60 miles. We would add about 3 observations at the extreme ranges on two lines spaced equally between the 45 degree lines. The lines in transit required about 14 days, and the lines near Ascension required about 30 days.

Because we were working in the ward room. we caused some inconvenience to the officers off watch who normally used the ward room for coffee breaks, paper work, and recreation when they were off watch. We were careful to use as little space as possible, and to offer to leave, if they wished, and sometimes to play chess or just chat with the officers in the wardroom. A chess set had been provided for this cruise, and none knew too much about it, so all the officers, and occasionally we had a try at it. I fear skill went begging, and most of us tried for total annihilation of the opponent.

For such a long time at sea with little exercise, I worried about keeping fit. Each, morning I would go on deck and do pushups, sit ups, jumping jacks and toe touching exercises. After a while I could do twenty of each without too much strain. The sub crew, thought I was a little cracked because of this activity. No one else on board got any more exercise than their job required. It was not uncommon for the crew which did not have to stand bridge watch, to go a whole cruise without ever going on deck between port stops. One morning, after making the night observations I went into the wardroom to get breakfast only to find noone there. This was a surprise as a galley hand was always in the serving room just off the ward room to get the officers any food or drink that they might want at all hours. After waiting about five minutes, I went to the control room, usually inhabited with about 5 men on watch, to find noone there. I climbed to the conning tower to find it too abandoned. This was a weird feeling like I was the only one on board the sub, well out to sea. I went up to the bridge to find it also empty. When I looked around on the deck I saw a large group on the after deck. I went back and found out that a rod connected to one of the flood valves had come loose and that they were getting ready to weld it back in place. These valves were operated by large outboard levers. A few hours later, it had been repaired and we got under way again.

One day, in the tropics, one of the officers and I were playing a game of total annihilation chess on the gun deck below the conning tower. The captain had the bridge and was watching the development of the game, between his watch duties. At a crucial point in the game he called a surprise dive, and we had to throw all the chess men quickly in the partially closed board and rush to the bridge to climb down the hatch into the conning tower. The captain stood there laughing at us as we scrambled to get in board. He was the last one in the hatch, as was usual for the deck watch officer, and got a generous amount of water on him as the hatch was closed. So, we had the last laugh, but not too loud.

A number of men on board, including Ken and I had never crossed the equator. Though I had spent considerable time at sea, by that time, I had never had that experience. Consequently the "shellbacks' planned to have induction ceremonies as was common in the navy. As we approached the line, some of the crew came in to the ward room and placed handcuffs on me as starters. When they left, I found a paper clip and managed to get the handcuffs off. I went in to the control room and handed them to the shellbacks. They decided I was lucky and followed me in to the ward room and placed them on me again. This time, I knew how to open them and went to the control room and handed them back to them before they had even returned to their posts. Shortly afterwards, they came back to the ward room with chains and a number of padlocks and padlocked the chains around my wrists and ankles, and connected these with a chain between the foot chains and wrist chains with another short length of chain. With my hands and feet chained together and the two chains connected in front they considered me properly tethered. I found a bolt cutter and some more locks, cut the chains and changed my clothes, which I could not do with the chains in place, and locked the chains together again where I had cut them. I then walked through the boat to show the shell backs,that their chains didn't work. I returned to my cabin, changed my clothes again and walked through the boat again. After that, they gave up on tethering me, but they did not remove the chains. As a result, I had to sleep with chains tied to my wrists and ankles all night.

We had actually crossed the equator during one of the dives at night, so the captain reversed course on the surface and crossed the equator again so the deck festivities could be carried out. Of course, he was a shellback. After the festivities, which had been much more moderate for Ken and I than for the other polliwogs, we resumed our course and observation program.

When we arrived at Ascension Island it was necessary for the Captain to go ashore for the formalities with the governor. He decided the executive officer and I should go with him. He was also going to try to get some shore liberty for the crew.. When I came on deck some of the sailor's called to me to come look at the queer fish they had been catching. When I saw the fish I told them that they had better quit. These queer fish were Moray Eels which are supposed to be quite vicious and had very numerous and sharp teeth.

The method of landing on Ascension was different than any other I ever experienced. On the Anton Dohrn of WHOI, I used to make pier head leaps to the dock, so there would be someone on shore to catch lines and attach them to piles or bollards. Here, they came out in a motor launch and picked up those who were going ashore. They moored the launch to a buoy near the bulkhead in shallow water. From there you had to transfer to a skiff with a man to row you to the bulkhead. The bulkhead was about 30 feet tall of solid concrete. A stairway was indented into the face with a small landing at the bottom. One had to jump from the skiff to the landing, covered with slippery seaweed, and scramble up the steps to the top. The first three steps were also covered with slippery seaweed. The assistant governor met us at the top of the steps to conduct us to the governor.

We started for the buildings and had walked about ten steps when the three of us stopped and looked at each other. We were surprised of the quiet that reigned, having lived among humming, running machinery without cease for several weeks. One of us said listen! It's so quiet. On the way the assistant told us that a ship stopped by with supplies from Britain about twice a year. To get cargo ashore, they had to tow a lighter, (small barge), under the crane we had seen at the bulkhead. This stood about 20 feet above the bulkhead. The boom could swing out about 15 feet from shore, and a line with a hook would be lowered to the lighter. This was hooked to a piece of cargo and had to be snatched clear while the lighter was at the top of a wave. He said the waves never ceased, and they varied from 3 to 20 feet in height. The cargo had to be off loaded when the ship arrived, since the ship could only stay for the daylight hours, regardless of the wave conditions. Just before we arrived they had had a siege of waves bigger than normal. They had smashed against the bulkhead so hard they had surged to the top of the crane, 50 feet above the water level, and knocked a light off the top of the crane!

There were about 200 people on the island, and the six month visit of the ship was normally their only contact with the outside world The women on the island would have to go out to the ship, when it was in, if they wanted to get the services of a hairdresser.

The governor was pleased to see us, and readily acquiesced to giving the men shore leave. He agreed to have the bar opened at 1 P.M. for 4 hours. Normally it was opened once a week on Friday evenings for only a couple of hours.

I could see that the island was covered with volcanic peaks. I asked if I could get transportation and a chance to collect some rocks. I had read all, the geological reports (1) before the trip and realized that few rocks had ever been collected. He "persuaded" the assistant governor to take me around to various locations in a car. The few cars there looked like model A Fords, but were of some British Make. There was no car younger than 20 years. They were still using gasoline that had been left over from WWII.

Planes were ferried to Europe via Brazil and Africa in WWII since they could not carry enough fuel to make it without stops. We stopped and looked at the abandoned airport. The runway was now covered with rocks and no plane would dare to land there. The runway rose in elevation to the middle then fell in elevation on the other side. My guide told me that often first time pilots, fearing they had run out of runway by the time they approached the peak, would hit the throttles and take off again rather than complete their landing.

He also took me to a sector of the island where numerous birds, whose name now escapes me, were nesting. I had never seen so many birds at once. They would dive bomb us trying to scare us away from their nests. There was a main volcanic peak on the island called Green Mountain. When the British came to the island they had made a dew pond on the top. This was a technique borrowed from the Romans. They planted a large grove of bamboos at the mountain top and built a basin at their base. The mountain top is mostly cloaked in clouds, and the moisture in the clouds condenses on the bamboo, runs down to the base and is collected in the basin. A pipe line from the basin down the side of the mountain, about 4000 feet high, feeds them all the water that the town can use. In WWII there were so many people on the island, that the dew pond could not suffice. The air base. operated by the Americans had to install evaporators and evaporate sea water as a supplement. The plant was still on the island but had not operated since the war.

He took me around on the road between the peaks. There was no vegetation on the island except for a few short days after the "rainy season" once some years, and the top of the mountain where some of the overflow from the dew pond escaped. It was the most amazing place I had been. There was nothing but volcanic peaks. They were growing out of earlier peaks, from the ridges, from the sides of previous ones, and peaks on peaks on peaks. There were very large ones such as Green Mountain, down to small ones probably a couple of hundred feet high. There was no visible lava. The outpouring seemed to be only ash. I climbed several, alone, as my guide did not want the pleasure, and found many bombs and some lava in the center of the cones. I collected about 40 or fifty rocks in the three hours that I had the opportunity. I brought them back to Columbia and gave them to our colleague, who specialized in igneous rocks. He never did anything with them, so far as I ever knew. After a few years I quit asking him about them. I guess I wasted my time making a collection. When we got back to town, the bar was already closed so I never got a drink. From the crew's view point it was another example of how quaint scientists were.

We spent about 24 days surveying around the island and then returned to Key West. AT key west we transferred the gravity equipment, all 2000 pounds of it, to the USS Balao. D.H.Shurbet and Ken Gilchrist made the observations on the other half of the survey. They left in early March and returned at the end of April. We brought the gear back to Lamont and made our base observations. I turned our two lady computers to checking all the computations we had made during the trips, and completing the measurements and computations that we had not been able to do at sea.

We had also collected soundings at 5 minute intervals on the whole trip. On the Archerfish I had not been satisfied with the navigation that had been done by the crew, so I started to redo it carefully. The Navigating Officer became incensed and went to the Captain to intervene. The Captain satisfied him by saying that it was all, right with him if I wanted to redo all the navigation, but the ship would use their own for the ship. I found that some of the ratings were not as careful with following all the logs as I was.

I had Anette, my assistant plot up charts of all the observations to submit to the C&GS. When the soundings were plotted on charts, I took one set and contoured them and gave another set to Bruce and had him contour them as he had extensive experience with the Mid-Atlantic ridge soundings. My contour map was a conglomeration of volcanic peaks, as a result of my observations on the island, and Bruce's were a series of ridges parallel to the ridge axis where Ascension Island lies. Bowing to Bruce's greater experience I sent his contour map to the C&GS with the gravity measurements a few months after the Balao trip was over. I never found out how the computations of the vertical came out. No need to know, you know.

About 1955 we had borrowed Sir Edward Bullard's heat flow apparatus. It was a probe that was inserted into the bottom with a temperature sensor at the tip about 3 meters down, and another near the sea floor. It had to be lowered to the bottom to penetrate the sediment. After a wait of about 15 minutes to allow the heat caused by the insertion to dissipate, the temperature difference at that separation was measured. After recovering the apparatus, a core had to be taken to measure the heat conductivity of the sediments. Since these measurements were made several hours apart, and the ship was drifting, one had to assume that both measurements applied to the same location, which might not be true in some cases. It also required two lowerings to bottom doubling the station time. After making about a half dozen measurements, we returned the apparatus.

Doc assigned Langseth who had just received his PhD to develop a heat flow apparatus that could be attached to our coring operation so that the two measurements could be made on one lowering, and where the two measurements were indeed in o the same sediment. In about a year he had developed an apparatus that could be used. It consisted of an instrument chamber which could be inserted in the coring weigh which had been adapted to accept it. This provided a secure mount, and good protection for the recording equipment, which was somewhat delicate. The recorded was connected by electric cables to two to five thermistors, each mounted on an outrigger along the length of the coring tube. Thus we could make several thermal gradient measures at each core site with sediment to measure the heat conductivity contained in the core. When he had all the equipment working, we adapted it to each coring attempt on most occasions.

Thus we added additional data acquisition with only a modest addition of time to a coring station again.

At about this time,I was able to persuade the Navy to give us a new engine for Vema from war surplus supplies. It was a GM diesel engine. We also acquired a gear reduction and reversal unit of suitable size from the Navy. This was a great advance, as we had quite a lot of trouble keeping the old Burmeister and Wayne slow speed diesel working. The new engine was much smaller thus giving us much more room in the engine room. To remove the old engine and install the new one, we had to remove part of the main deck. Since we had not ever been able to keep the deck from leaking, we persuaded the Navy to let us remove all of the 2 inch wood decking and replace it with steel decking. Since we had to work on deck in quite rough conditions, we obtained a material called Polycrete to coat the steel decks. Sand could be mixed into this epoxy mixture as it was laid down to give it a rough surface to gives us better footing. This material was very successful. Although we sometimes dropped heavy and hard equipment on the deck in our work, we never damaged this surface. One time several years later when we had to make some changes, we removed the Polycrete over a section of deck and found the steel surface bright and shiny without any trace of corrosion beneath it.

We also were allowed to replace the lower lab deck house that had been damaged in the storm when the men were washed overboard with a steel Deckhouse, and to build an additional laboratory above the replacement house and the after house. This gave us an expanded laboratory space, well above any chance of flooding to contain our electronic equipment, as we'll as additional space. While we were at it, we made small two man cabins in the space in the tween decks which had held the triple bunks used by the scientific crew. This provided a much drier environment and some privacy for the scientists. All of this work was done in one of our annual overhauls. The cost was covered under ONR's facilities budget.

Replacing the engine entailed removal of the full feathering propeller and its hollow shaft, with a solid shaft and a new propeller. Since the shaft had to be about 60 feet long it had to be made in two lengths which required a universal joint between the two halves. The spherical ball bearings we had previously installed, made this an easier task.

These modifications made the ship more habitable, increased our space for scientific projects and overall, and made the ship much more efficient. It was now a formidable instrument for research. Underway we made reflection seismic measurements, soundings, 3.5khz soundings of the near surface subbottom, and magnetic measurements. At stopped stations made at least once a day, we took a bottom core, a heat flow measurement. photographs of the bottom. and on occasion took large samples at several depths of sea water for chemical analysis.

In the spring of '56 I decided that since I had taken no vacation since starting work at WHOI in '40, that I would take the summer off and travel west, instead of the program of research I normally carried out in the summers, since I had received my doctor's degree. Especially Sandy our oldest child had reached the age of 13 and I doubted that we would ever again get her to go on a trip west. I planned to go to as many National Parks and National Monuments as we could. I had bought a Chavrolet station wagon with this in mind for our future. In preparation, i bought a one wheeled trailer from Sears. The wheel was on casters and the front clamped onto the bumper. Thus It was no problem to back up. I built a roof to cover the trailer. and fiberrglassed the top to shed any rain we might encounter. I covered the side with screening to keep out any bugs. I planned to make beds for the two older children in the trailer, with Richard then six, Dorothy and I to sleep in the back of the station wagon.

We accumulated all the gear that I thought we would need on the trip, and late in the spring I took the two older kids to Bear Mountain Park, about 40 miles away to give everything a first trial. Everything worked well except for our first meal, I went to start our lp gas stove and I had no matches. Accustomed to emergencies I decided to start a piece of paper on fire from the cigarette lighter in the car. I could only get it to smolder, I could not start a flame. Finally, I had to swallow my pride and go to another camper and ask to borrow some matches.

We were surprised that although it was just May, a lot of NYC people were already moving into the park for the summer. A further surprise was a lot of them brought a refrigerator, kitchen table and chairs with them. These they established under a canvas fly to shed water. Most of them had tents to sleep and change clothes in. Basically camping meant to them, to move as much of their flat to the woods as they could just changing the city streets for the camp streets. The men, by and large, commuted into the city during the day for their job, and returned to the park at night. It was apparent that many of them were well acquainted and had been doing this for a number of years. In the morning I was further surprised when a paper seller came early to the park selling his wares, soon followed by a milk man, and a baker.

Having a Saturday and Sunday camp under our belts, despite the incident of no matches, I felt we were ready for the summer.

Sandy acquired a bad case of poison ivy just before the end of school, and had to have her left arm bandaged from shoulder to hand. Although Sandy was a little uncomfortable, we decided to leave on our trip as soon as school let out, and we did. It was late June and it had turned hot. By the first night we had reached about the middle of Pennsylvania. All of us were hot and miserable, so as soon as we had our camp site secured we all got in our swim clothes and retired to a lake nearby that our camp guide said provided swimming. To our surprise, the lake was really only a pond and the water was quite brown with suspended sediment. The lakes we were used to were not usually laden with sediment. Nevertheless, we were so hot we decided to go swimming. Sandy and Howard were already swimmers, but Richard had been only learning and wore a flotation device when we were in the water. In this case though, we had not brought his float, and none of us thought of it, only thinking of getting cooled off. Suddenly I realized that Richard was with us and was swimming without any float. So he joined the ranks of the swimmers without any emotional turbulence.

The trip across the great plains was hot and lengthy. None of us had expected them to last so long, even though I had had the experience at age nine of crossing them in summer in a Pullman car without air conditioning. We were glad to see the mountains looming ahead. Nevertheless this was a great educational experience for the kids to see the vast extent of our granaries. I had bought plastic air mattresses. on the trip across the plains the kids opted to have the seat lowered and the whole back area platform for them to share. They soon found this hard and demanded that the air mattresses be used. This started a long controversy between me and the kids, because the mats soon developed leaks. I claimed it was because they were not careful enough, and they claimed they were not rugged enough. This controversy lasted for most of the trip. Wisdom learned later says the kids were right and I was wrong. However, a large amount of time was spent on that trip with me locating and patching holes in the mattresses.

Our first National Park was Rocky Mountain Park. Our first day there we had entered, visited the visitors exhibit at the entrance and had climbed to the higher elevations and started down the other side when we located a camp ground. While Dorothy and a couple of kids got dinner ready on our lpg stove, I took Richard and found a nearby food store and bought a few items and some ice as our ice box was running low on ice. We returned to camp and put most of the ice in our ice box at least as much as would fit. I stated that we would leave the remainder on the table and add to our supply in the ice box in the morning when the present supply was diminished by the melting overnight. It turned quite cold during supper as the sun went down and we soon donned warmer clothes. We had found that my idea of bunking two kids in the trailer was not too good. Every time either one turned over, the trailer bounced up and down, awaking the other one to recriminations and worse waking all of us in the Station Wagon by shaking it. The day following the first night, I bought an Umbrella tent, which was big enough to sleep four. After that at least three of us would sleep in the tent, and the others in the station wagon. Although the tent was quite cheap, it lasted a long time and we used it for camping for at least ten years afterward, also loaning it on occasion to some of my colleagues. Thereafter we used the trailer to carry all of our baggage.

I had bought some army blankets from a government surplus store so that each sleeper had one. This first night at altitude in the mountains we all found them inadequate. We spent a fairly sleepless night trying to keep warm. We finally gave up as soon as it started to get light and finished dressing (most of us had partially dressed during the night in search of warmth) packed the car as quickly as we could and left the camp site. The last thing I did was to check the ice box to see if we could salvage some of the leftover ice. No ice had melted during the night and there was no space. The ice block on the table showed no water from melting either! We left the remaining ice for the next camper. We got underway and at the first sign of a cafeteria open, we stopped for breakfast and to get warm. None of us will forget that night!

The next day I stopped and bought two sleeping bags for Dorothy and I and more blankets so the each kid had at least two. We were set for the rest of the trip. We replaced the two sleeping bags about twenty years later after getting a lot of service out of them.

We visited Black Canyon of the Gunnison, Meteor Crater, Grand Canyon, both the south and north rims. Pipe Spring National Monument, Bryce Canyon, When we arrived in Bryce we had camped directly without viewing the Canyon. I took Richard and went to the store, while the rest stayed in camp. I told Richard we would sneak a look at the canyon first. We did. I was greatly impressed by the vista, when Richard grabbed my hand and said, "all right we have seen this, lets go". He heard this repeated to him at most impressive sights for the next ten or so years by all family members. We visited Zion Canyon, Hoover Dam, and Lake Mead. There we camped near the lake shore. There were no trees or shade of any kind. It was so hot that we all stayed submerged up to our shoulders most of the day and left after one day, although we had planned to stay two days.

I had bought an evaporative air conditioner to help crossing the desert. It was supposed to evaporate water by the air flow entering the car through a water bath, with no other windows in the car open. It was a bad failure as far as we were concerned. We soon discarded it and opened all the windows as we crossed the deserts. This was not bad, except from about 11 to 2. In that period, Dorothy passed a wet towel around for all to wipe their face and hands on from time to time. This made it acceptable. We had also bought a water bag to attach to the front of the car to cool water by evaporation for drinking. We decided this did not suit us and bought a one gallon thermos which we filled with ice and water. We used this for the rest of the trip and for many years afterwards.

When we had crossed the mountains, the roads of that time were quite winding, were narrow and had few if any guard rails. The kids and I had no trouble with this, but Dorothy held on for dear life. For years we kidded her for holding the car together while we traversed the mountains. As we approached the mountains, toward the last days of June, we had started to see snow near the tops of the peaks and in some of the shaded valleys. This snow, was covered with a lot of dust and looked quite brown. I assured the kids this was just dirty canvas that had been left on the mountains. For a short while they believed me, but soon denied it and insisted it was snow. In Rocky Mountain park, we finally could stop near some and they had a good time throwing snowballs at each other in June. On the trip from Rock Mountain Park to the Black Canyon of the Gunnison we crossed the continental divide three times in one day, visiting much lower altitudes in between crossings. Dorothy had ample opportunity to hold the car together.

In California we spent a couple of days with the Press's at their home in Pasadena. We spent a day on Long Beech where we found the Pacific quite cool for swimming. Frank took me for a sail in his sloop, and I was impressed when he called the White House from sea about a business matter. He was on the President's advisory council. After leaving the Press's we spent a day at Disney Land. When we left, Richard swore he would return to spend at least a week when he was grown up. We visited Scripps where we visited the Aquarium and the portions open to the public. I did not get in touch with any of the scientists there, because we were getting up at dawn and going to bed at dark, and I was sure they would invite us for a visit which would disrupt our schedule. I was chided for this by Russ Raitt some time later when he learned we had passed through La Jolla. From there we went south and visited Palomar Astronomical Observatory. Then we turned north along the Pacific Coast.

We found the camp sites along the ocean quite difficult to get into, because they had a system of reservations and long stays which we hadn't known about. On one occasion, we could not find anything and we had to head into the mountains to the East where our camp guide advised there was a camp ground. We found it, and also found out there was nobody there. We camped and soon had a fire going to keep warm, as it turned quite cool as dusk approached. There were two fishermen fishing along the shore of the lake in the camp site, and one fell in and got soaked. He saw our fire and came over to ask if he could warm and dry himself. He was quite drunk and I had qualms about agreeing, but feared he might get ugly if I did not, so we made room for him by the fire. He stayed and stayed. Since we only had enough food for ourselves I told Dorothy to hold off supper until he left. Finally I told him he had to leave. He was reluctant, but did go. This was the only time on our trip that I had any concern about camping out.

I had bought a camp guide before we left. It was about 4 by 8 inches and 1/4 inch thick (a far cry from the 8 by 10 by 2 1/2 inch guides of today) from a place in Kansas that claimed it listed all the camp site in the U.S. It never failed us. We did find camps sites reasonably near where we wanted to camp. At that time, most of them were free and had few amenities. Sometimes they would charge a dollar or two. Toilet facilities was almost always out houses, even in the National Parks. The camp sites were almost always a grassy field with each camper choosing where in the field to camp. We had divided up the chores amongst ourselves according to our capabilities, some blowing up air mattresses, some setting up the tent, and Dorothy doing the cooking. At least there were picnic tables available in most sites. We could stop, get our supper and set up camp within an hour of stopping. In the mornings, the reverse would occur and we would get up, have our morning ablutions, get breakfast and get packed for travel in an hour. This was much faster that most of the other campers, and we took pride in it.

After visiting Sequoia and Yosemite National Parks ,we passed through San Francisco crossed the Golden Gate bridge and visited Sequoia Sate park just north of the city. The kids had been quite impressed with the big trees, about 15 feet in diameter and about 300 feet tall. At this park they had a section of one of the trees with rings marked to show the tree's size at the birth of Christ, The signing of the Magna Charta, the Signing of the Declaration of Independence, the Civil war. This brought home to the kids the age of the trees more than anything else we had heard or discussed. On our arrival at Yosemite we were quite hot and desirous of cooling off. As we approached the camp site near the river we marveled at the people in the camp site ignoring the swimming beach along the nearby river. We quickly set up our site, changed into our bathing suits and ran for the beach and river. With our first contact with the water we must have jumped ten feet in the air. It was ice water fresh from the glaciers on the mountains nearby. We had a similar experience at Lake Tahoe.

In northern California we visited Mount Lassen. I asked who wanted to climb the mountain, and only Howard volunteered. So the next morning he and I got up at dawn and drove to the trail up the mountain. We found it still covered in snow and there was a sign stating the trail was quite difficult and it should not be attempted without proper climbing gear. Since we did not have any, we aborted our attempt and returned to camp where the others were just getting up.

We were impressed with the lava flows and volcanic breccia surrounding the base of the mountain. On the north side was a scree slope with small trees about 10 feet tall and about 3 inches in diameter. We were startled to learn they had been growing for about 800 years in the volcanic cinders. They were so stunted by the lack of water. Nevertheless they were survivors. Next we went to Crater Lake in Oregon. We had to climb the mountain on a road on the northwest side of the volcanic mountain. As we neared the top we stopped and took pictures of the car alongside a snow bank which towered about 5 feet above the car. This was in the latter half of July. At the camp ground, some of the roads had not yet been opened and they came by the camp with a snow blower clearing some of them. The snow blower was capable of chewing into a bank of snow about 6 feet deep, shredding it and blowing the snow about 20 feet in the air and about 50 feet to one side. Everyone in the camp came running to the road to watch the machine go to work. Most of us stayed on the left side of the blower, but Dorothy and Richard had crossed the road to get a better look at the operation. At about that time, they attacked a snow bank on the road and Dorothy and Richard were in the line of fire. They beat a hasty departure to our great amusement.

We continued north to the Columbia River. This is a mighty river, at least three times as wide as the Hudson. As we went east up the river we came to dams that had been built by the federal government. These supplied more electric current than the nearby cities, towns and countryside needed so Hanford Washington had become part of the U.S. atomic bomb construction in order to use the power. They used a large part of the power generated. We especially enjoyed the view inside the fish ladders. They had built windows so that you could see the salmon swimming up the ladder and jumping the weirs.

We were amazed by the scab lands towards the eastern side of Oregon. These were explained as the result of a large Lake that had been dammed up by the glacial terminal moraine at the end of the ice age. After awhile, the dam had been breached and the water from a lake the size of one of the great lakes cascaded down the Columbia River Valley in just a few days, scouring everything out of their way near where the dam burst. This included the top soil and consequently nothing could grow, and the evidence of the surging outflow was still evident although this had probably happened about 8000 years ago..

We were heading back east now and we stopped at Craters of the Moon, then a National Monument, and now a National Park. The camp ground was in the middle of the massive lava flows that formed the Monument. There wasn't even water available there, though there was an outhouse. We walked around the various lava flows, long since cooled, but still with very sharp crests. Our shoes took a beating. The volcanic bombs, lava that had been blown out of the volcano and had solidified in the air as they flew, were especially interesting to the kids. I saw lots of things I had seen in pictures in text books for the first time here. When we returned here many years later, the campground had been greatly enlarged, there was a fine shower and flush toilets, and it even had hot water available, heated by the residual volcanic heat underground.

Our next stop was four days in Yellowstone National Park. the geysers, the fumaroles, the hot pools, and mud pots provided endless fascination. I was disappointed at Mammoth Springs, because the beautiful orange and buff springs had become dull gray. It seems the hot springs had found a way to seep into the ground instead of cascading down the front slope, and the organisms that made the colors died as the front dried up. Years later, when we visited there again, the cascade down the slope had partly rejuvenated and there was some color in the hot water running down the slope. It was still much less than I remembered from my trip with mother in 1929. I remembered a hot pool called Handkerchief Pool, but I could not find it. You could put a handkerchief in the pool. It would disappear into the natural pipe at the bottom of the pool, and several minutes later it would return to the surface sparkling clean. Finally I asked a ranger where it was. He told us that it had been destroyed. A tourist had put a log into the pipe at the bottom, and the pool had blown up and soon disappeared. We all lamented the vandalism that this illustrated. I guess it was a foretaste of things to come, when vandalism became a common occurrence. At one of the hot, springs we ran into some neighbors from Palisades. After a short visit we all went on our way, still wondering at the chance of such an unplanned visit. The Grand Canyon of the Yellowstone and the twin falls almost came up to the wonder of the, geysers, pools of hot water, and mud pots.

We stopped in Rapid City, North Dakota to see the four life size dinosaurs that had been built out of concrete by the W.P.A. during the depression years. This was well before all the mania of dinosaurs had developed. Our kids were suitably impressed and we took their pictures showing their alarm under the attack of these beasts.

That night we stopped near the Badlands park for the night. We found a free park that was run by a religious sect. You had to agree to come to their evening talk if you camped there. After we had set up camp, and after supper, we went to their talk. It was cut quite short because of a sever thunderstorm which broke over the area.

I had noticed the severely dark clouds in the west, as we were setting up camp. but being inexperienced I thought nothing much of it. Howard and I spent the night in the tent, and the others preferred to sleep in the car. The storm broke about 8 p.m..after about an hour, Howard and I decided to go to bed at about 9, since the storm continued, in fact, it seemed to worsen. I guess we had dozed off, because about eleven the tent started shaking violently, which wakened us and Howard and I grabbed the tent pole to steady it since it seemed to be in trouble. After about an hour, the tent started to collapse around us. I opened the door and looked out and discovered, the clay around us had become so soaked with water that it had no strength and the tent stakes were being released in the mush.

After thinking it over a bit. I asked Howard if he thought he could hold up the pole alone while I went out to see what i could do about the tent stakes. He assured me that he could, and I put on my socks and shoes and went out in the deluge with my geologic hammer. I found that I could pound the stakes deeper in the substrate below the surface sludge, and got the tent stabilized. By the time I was done, I had an accumulation of about six inches of gummy wet clay adhering to my shoes and I had trouble lifting my feet high enough to move around. At the entrance to the tent, I took off my shoe as I entered the tent. I brought them in with me setting them down just in front of the entrance. Soon after, the storm abated and we slept until morning.

After we waked, I cleaned the accumulation of mud off my shoes with the hammer, put them on and carried Howard to the car. I then collapsed the tent, pulled up the stakes, and piled tent and everything helter-skelter on top of the roof of our one wheeled trailer. Then I got in the car after removing my shoes and drove out of the mess to high ground..

There we stopped, spread everything out and cleaned everything as well as we could, folded the tent and collected the stakes and stowed them in the trailer.

That morning we stopped at a restaurant for breakfast not desiring to make our own. That evening we had to spend a couple of hours cleaning the hardened clay off of everything After that experience the Badlands Park did not seem so bad. We wondered about the beauty of the various colored clay layers that outcropped here.

After leaving the Badlands we visited Devil's Tower then headed home. We did stop long enough to enjoy Niagara Falls. Dorothy and I had not been there since our honeymoon, and the kids never. Niagara had just had the misfortune of a rock slide which had done some damage to the powerhouse, just before we got there. This was a graphic illustration of the impermanence of even large outcrops of strong rock to the onslaught of undermining waters.

We returned home at the end of august. We had covered 10,000 miles and visited 26 National Parks and Monuments, and about four State Parks in the about 60 days of our trip. We were sated for the time being. It started us on a regime of camping and trailer trips to many parts of our country in later years, and still continuing.

In the, late summer of 1955 I had attended the International Union of Geodesy and Geophysics meeting in Rome. This meeting lasted for about two weeks. I spent the whole two weeks there. About ten other scientists from Lamont spent from a few days to the whole length of time there too. Dr. Ewing and I arranged to live in a bed and breakfast there. Doc encountered Art Maxwell at the meeting. He was a Scripps graduate student at the time, and had no living arrangements. Doc helped him get a room at the same Bed and Breakfast that we were at. The three of us had breakfast together on a balcony off our room each morning.

I remember the meeting mostly because of the trips around Rome that the IUGG had arranged. This was my first exposure to the Roman ruins. I was impressed. Several humorous incidents occurred. One was that late one afternoon a group of scientists, about eight of us, some from Great Britain, some from France, one from Belgium, and Doc and I were having a beer at a sidewalk cafe. Of course we were having a general discussion of the papers that we had heard that day. The tables at the cafe were so close together that it was hard to walk between them. A couple of tourists from the states sat down right next to Doc. The woman was just bubbling over with her impressions of the ruins she had seen for the first time that day, and kept insisting on telling Doc about them. Doc, of course, wanted to hear the discussion at our table, but being polite, would acknowledge each of her observations. At last she said "We visited the coliseum and you know what? They starved the Lions, Tigers and other wild beasts before they turned them loose with the Christians". Doc becoming exasperated replied "You know they probably starved the Christians too". The lady was shocked and never said another word to Doc, for which he was grateful.

At one of the geophysical oceanography meetings, Lamont scientists gave about half the papers at the session. The remainder were given by scientists of other countries. Maurice Hill, a British scientist, leaned over to me and said,"You people at Lamont are awesome."

After one of the meetings, I was standing in the hall and Dr. Graf, a German scientist came up to me and introduced himself. He confirmed that I was the one carrying out extensive series of Gravity Cruises on submarines. He then asked me if we used a cardonic suspension for the pendulums on board the subs. I replied that we sort of did. A Cardonic suspension is like a universal joint with the load suspended under it so that it could swing in any direction. Our pendulums were suspended in a framework that had two sets of bearings at right angles to each other on each side of the apparatus. This also let the apparatus swing in any direction that the motions of the boat might make, leaving the pendulums essentially undisturbed by the boat motions. We talked some more and he told me that he had built a gravimeter for use on shipboard and that he had tested it out on the Starnberger See, a large lake in Germany, and that it seemed to work quite well. He agreed that a lake could hardly be like being on the ocean. He gave me a copy of his paper on these experiments. I was impressed. I asked him if he would be willing to bring his meter on a submarine, if I could arrange it, to compare his readings with the pendulum measurements. He assured me that he would.

On my return to the states, I went to ComSubLannt, where I had been making arrangements for cruises in the Atlantic, and asked them if a suitable cruise could be arranged. This was a problem, because the Navy did not want foreigners on board our subs to see the kinds of improvements that we had achieved. After some time, and having referred the problem up to the Chief of Staff of the armed services, permission was granted. ComSubLant had a trip planned for one of their subs in the Mediterranean to stop at Palma Majorca, and then proceed through Gibraltar stopping at Portsmouth England for the fall of 1956, and they said we could make our comparison on that trip. I contacted Dr. Graf and we arranged for him to come to Palma, Majorca and join us and to return home from Portsmouth.

I arrived in Palma at 0100 in the morning. Their customs was closed for the night. My one ton of very delicate equipment had to be stored in a cage until the customs opened in the morning. The people in attendance could not speak English and I could not speak Spanish. By signs and other means I was able to convince them that my gear was very delicate. Despite the language problem, the baggage handlers off loaded my equipment into the customs shed. They handled the gear more carefully than anywhere I had been with it, just the same. I got a room and spent the night. In the morning I got a taxi. Again I had the language problem. The driver could not understand my English, my attempts at French, and my even worse attempts at German. I finally got him to take me to the Spanish Navy Base.

Fortunately, when we got there, the U.S.S.Becuna had arrived during the night, and was tied up at the pier. I went on board and made myself known. They were expecting me. With their help I got a truck from the Navy base to go out to the airport to get my gear through customs and brought to the ship, where it was off loaded on deck. I immediately started to get everything unpacked and moved below decks to the ammunition locker where it was arranged I would set up my gear. This was quite a job, because of the delicate nature of the gear. I insisted to handle most of it by myself. About 10 Dr. Graf Came on board. The gangway watch would not let him come below decks, so I went on deck and greeted him. When I went to the captain to arrange for him to come on board, I found out that the captain had not had clearance for him to come on board. He sent a message to ComSubLant asking for the clearance.

I had to go on deck and explain to Dr. Graf that the clearance had not come in, that he should leave his equipment on the deck and return the next day when his clearance would let him come on board. I promised to move his equipment on board (in the forward torpedo Room) so that it would not be in the weather overnight. The Captain had specified that his gear would have to be set up in the after part of the forward torpedo room. When he returned the next day, the radio message about his clearance had not come, so I had to convince him to let me set up his gear and for him to return the next day. I was quite busy getting my own gear set up as well as Dr. Graf's.

The next day, the message clearing Dr. Graf to come on board had been received and He was allowed to come down into the Forward Torped

Rroom to make any adjustments to his gear that might be needed. He was pleased that I had got it set up and the crates in which it had been shipped stowed so that on our arrival in Portsmouth he would have crates to return it to his home. He made some adjustments, and made the necessary base measurements, eating lunch on board and returning to his lodging at night. The next day he came on board and the captain and I met with him in the ward room and the captain made it clear that he could not move freely about the boat. He could only go into the forward torpedo room, the ward room and to his bunk in one of the officer's cabins. He moved on board, and we sailed in the early afternoon.

It was soon apparent that Dr. Graf was not a good sailor. All the time we were surfaced, he was seasick and spent his time in his bunk. When we made a dive he would get up, quickly get something to eat and get to his gear by the time we were stabilized at depth, make his observations, and climb back into his bunk by the time we surfaced again.The captain had no worries about his studying any of the equipment on the boat.

We made 16 observations in the Mediterranean, and about 30 in the Atlantic on the way to Portsmouth. As we approached Portsmouth I had the Captain send a message to Sir Edward (Teddy) Bullard inviting him and Ben Browne, the gravity man at the Department of Geodesy and Geophysics to come on board to meet Dr. Graf and to see his equipment. They came down on our arrival and were very pleased I had invited them. As they left, Teddy told me that a student would come down the next day and show me around some of the area in the southern part of England. I helped Dr. Graf to get his equipment packed and he left the next morning. I started to off load my gear and get it crated to ship it home. About 11 the student, Chris Harrison, showed up and took me for a tour of the nearby area. It was pleasant, but I was more worried about getting my gear packed than about sight seeing. I thanked him for his courtesy and went back to work getting ready to leave. The next day, the gear and I flew back to the States.

About a couple of weeks later, Dr. Graf sent me his observational results. He had no equipment for observing the Browne correction for his observations, so I used the ones from my observations to make the necessary corrections to his data. Then I compared them to my data. It was great, most of the observations agreed within about a milligal, and none differed by more than five milligals. This was better than i expected since he had to use the Browne corrections measured on my apparatus, as he had no way to measure it on his own. I wrote a draft of a paper and sent it to Dr. Graf. He added a section about his equipment and approved most of what I had sent. The last sentence of that paper was "It is probable that this meter would be able to make measurements on a surface vessel, if placed on a gyro-stabilized table, at least in calm to moderate seas". The paper under our joint authorship in the Bulletin Geodesique in September 1957.

I forgot this incident that must have happened in 1955, so I will include it here.

At the start of a Vema cruise, as usual just after an overhaul, I took the first leg. We were to do extensive two ship seismic work so we took on a full load of explosives at Earle, New Jersey as we departed New York.

All went well until we approached Hatteras when we ran into a severe storm. This was a surprise as we had been keeping track of the Coast Guard weather reports quite carefully. The storm kept increasing in intensity until Captain Kohler decided we had to lay too and ride out the storm. By that time we were quite near the latitude of Cape Hatteras, although about 100 miles offshore of the shoals. The storm increased in intensity until it was just below a hurricane in wind speed After about a day and a half we were being blown close to Hatteras. Finally the captain called me to his quarters and told me that there was no telling when the storm might moderate and that we were getting too close to Hatteras for comfort. He wanted me to dump the 4 tons of explosive that were secured above the wet lab roof. I persuaded him to wait a few more hours to see if the storm would abate. About three hours later he called me into his cabin again and told me that we could delay no longer that he had to insist on the explosives being dumped. I tried to argue him out of it, but to no avail. I did persuade him that the 30 depth charges we had secured on the main deck could be retained. Since he knew that many Navy ships survived without hazard while keeping depth charge on board he reluctantly agreed. I had no choice, so I called the scientific staff together and we went to the cabin top and threw the explosives over the lea side. While on the cabin top, I tried to look upwind once, but the wind was picking up salt water from the ocean and was blowing it directly into my face with such force that I could not keep my eyes open. The waves at that time must have been thirty feet high, because my eye level was about at wave top level while I was on the cabin top. It took us about an hour to throw the explosives over, as they were well boxed and each box weighed quite a lot.

When we had finished, I released the scientific crew and most went back to their bunks to survive the dirty weather. Walter Beckmann and I decided that we should check on all our deck equipment and cargo and make sure it was all right. We started on the fan tail. Shortly after we got there, a large wave came across the deck and washed both Walter and I overboard, despite our best efforts to hold on. As we were washed off the deck, below the rail, we each grabbled the lower section of the rail and held on, as that side of the ship rolled upwards. There we were hanging over the side hanging onto the lower rail. I looked at Walter and he looked at me and we laughed, then crawled back onto the deck. We finished our deck inspection and found all safely secured and went below for a shower and a change of clothes.

About six hours later the storm started to moderate, and the next morning it had abated enough that we could get underway again. The Captain called me to his cabin and told me that the ship was taking on water and that we would have to go into port and have the damage inspected at a shipyard. We sent a message back to the lab telling them of our need for a port and dry dock asking them to arrange for us to go in to either Charleston South Carolina, or Savannah Georgia.

Shortly after we got under way again, we got a Coast Guard report of the storm off Hatteras and a warning of its strength. Before the storm we got no warnings. That warning must have originated from our own report to the Coast Guard of the storm

After a while we received a message to go into Savannah, but that we would have to off load our depth charges onto a barge that would be anchored out in the bay well away from all developed areas. We complied and went into the shipyard and tied up. The Captain and I went up to the shipyard office to enquire about being hauled up on dry dock to inspect our underwater damage. They told us that all their dry docks were in use and we couldn't be hauled out for at least a week. I called Dr. Ewing at the lab and told him of our jettisoning of all the explosives, but the depth charges, and what the shipyard had told us. He chewed me out thoroughly for allowing the Captain to talk me into jettisoning the explosives. Then he told me that I should go back to the shipyard and demand quicker service. When I told the Captain, he was angry and told me that when the people had been washed overboard in Bermuda they had about two weeks in port to make repairs, and why couldn't we have at least as much. Anyway we went back to the shipyard and told them the time was too long, wasn't there some way they could help us out. They agreed that they would send divers down to inspect the bottom to determine what the problem was and then we could discuss it further. They did, and the divers reported that a seam that was riveted on the starboard side about 2 feet underwater was the trouble. That a length of seam about 20 feet long would have to have the old rivets taken out and new ones to replace them, which would draw the seam tight. They suggested that we could careen the ship enough to raise the seam out of water, where it could be worked on, by putting several 10 ton weights on the deck well on the port side. The captain didn't want to do that as he thought it would put too much strain on the ship. The shipyard said that it would be all right to do that and that they often did it on other repairs. I called the laboratory and told Dr. Ewing what the shipyard said and about Captain Kohler's opposition. He said it was up to me to convince the captain and to get the shipyard to go ahead.

When i went into the Captain and told him what we wanted to do, he became angry and told me to do what I wanted, but that he would bear no responsibility. I told the Shipyard to go ahead, and the next morning they lowered three large weights on the port side, which raised the seam on the starboard side about a foot above the water line. They had to remove the tables, benches and paneling in the scientist's mess to get at the inside of the seam. It took them about three days to make the repair to the hull. For those three days, the Captain would not speak to me, even reply to my good morning. It took another day to remove the weights from the deck and to return the scientists mess to functional order. The result was that we got out of the shipyard, fully repaired in a shorter time than we would have had to wait to get on a dry dock.

When the repairs were being made, I called the Office of Naval Research and told them what had happened and that we would need replacement of the explosives we had jettisoned. They were cooperative and told me to call back in about four hours as they would have to locate more explosives and find out where we could load them. All of the explosives that we were using we're left over from World War II and the Navy was glad to get rid of them as they had excess. It turned out that there was an explosives depot in Savannah and they could load those we needed on a barge and bring them out to the barge where our depth charges were waiting so that they would be there just in time for our arrival. I was given an Officer's name to call when our sailing was arranged.

Of course, when we arrived at our barge, the Navy barge had not arrived. We loaded the depth charges on board and the Navy barge arrived about the time that we were finished, so actually we lost no time.

About a week after sailing, the Captain and I were back on good terms with each other again. The repairs never gave us any further trouble.

We later learned that a U.S.Navy aircraft carrier and destroyer, about 100 miles from us got caught in the same storm, and that the carrier had its flight deck folded back double about 100 feet from the front end. The Destroyer had become flooded in all the passages below deck doing lots of electrical damage. It was a year before the Navy ships were repaired and went back on duty. I guess that was an object lesson of the great strength of Vema's hull.

While I was away on my western trip, the senior members of the American Geophysical Union got together and decided that about fifty (I think) years ago the geophysics community decided to have a concentrated study of the magnetic field. It was a great success, and they thought it was time to do something similar. Apparently they couldn't agree which field to concentrate on so decided to choose them all and corresponded with senior scientists in other nations. I don't know much about it, nut the International Geophysical Year was born. It had been decided to actually make it a year and a half, as some countries would take longer to gear up than others, Thus 1957 and the first half of 1958 were designated as the International Geophysical Year.

I don't know the mechanics, but the NSF (National Science Foundation) and ONR received extra money to make sure the USA would successfully hold up our share of the IGY. The steering group divided the funding into specialty areas. Most of our funding came under the Oceanography division. Some of the earthquake funding came under another division which I can't remember.

In retrospect, I believe that the IGY was a major factor in the maturation of the NSF. At least in my view, they had been struggling for acceptance with both congress and the scientific community. After the IGY they were an accepted part of the community. Lamont Geological Observatory had been struggling for its establishment also. Each particular discipline had to send in proposals for funding separately, and they were funded at different dates so that operating the ship had been a hand to mouth affair. For example, on one cruise, the ship had been near Antarctica and was heading for Capetown, and we did not have any funds to continue her cruise. Doc asked me what we could do. I replied, I guess we will have to tell them that we have no more money to pay them and give them the choice of staying in Capetown, or returning the ship to New York without getting paid. I said I was sure I knew which option they would take. Instead Doc told me to go to Washington and beat the bushes for projects that would get the ship home. I did and from NSF and ONR I was able to get funding for work in the Indian ocean, the Mediterranean and back across the Atlantic. ONR made one proviso, that none of their money could be used in the Mediterranean as the Navy had ample data there, We were able to work the ship back to home port.

I believe the IGY established Lamont as a major player in the study of geophysics on an international basis.

Several years later, the Navy had a problem in the Med. They believed the USSR had a submarine spying near Italy which the Navy had detected with their magnetometers. They claimed it was moving at about 5 knots and attempting to escape. Some people in the Navy were skeptical and believed that they were being fooled by natural earth anomalies and ocean currents. I received a call from Jim Smith, who had helped us out earlier with the proviso that we not use any Navy money in the Med. He said he was embarrassed to ask, but did we have any magnetic anomalies charted in the area of interest. After checking with our magnetics group, I called him and told him that we did, but that since they had all the data they needed in the Med, why were they asking? After some chivying back and forth, I agreed to send him copies of our data in the area of interest. With this data in hand, they decided that the anomaly they had was actually an earth anomaly and the ship which had been “”following “ it had been affected by the strong surface currents which they had attributed to the movement of the anomaly.

The IGy program provided secure funding for the ship for about 18 months, and support for the various programs persued on the ship. We also received support for an upgrade of our equipment, which was very important to us. I received funds to build three additional sets of Vening Meinesz Pendulum equipment on the concept of getting several submarines operating making gravity measurements simultaneously. One of the first things I did was to go to Holland to get copies of the plans of the VM equipment. Dr. Vening Meinesz was very gracious and took me to the university where his equipment had been built, and persuaded them to make a copy of the plans for us. We of course paid the expenses. After about a day, I returned to Lamont with a copy of the plans. I took them to the shop and Angelo assigned his best machinist to make three copies of the equipment. After about a year, because the gear was so complicated, he only had them about 3/4ths completed, (he was making three copies of everything at the same time as being more efficient) I persuaded him to stop on two and concentrate on finishing one. He did, and I took it and made base observations with it and was ready to see if I could get additional submarines when the IGY was over.

Merle Tuve had applied for funds to develop a surface ship gravimeter. He approached me and told me he had the funds and that he had heard of my measurements with Dr. Graf and wondered what I thought of his meter. I responded that I thought it had a good chance of working on a surface ship if placed on a gyro-stabilized platform. He said, that in that case he would transfer the funds for the gravimeter to me if I could arrange to get a meter from DR. Graf. When I contacted Dr. Graf he was pleased that we were interested and told me that he had a second somewhat improved meter nearing completion and that I could have it. I reported to Dr. Tuve and he transferred the money to us.

Dr. Graf had invited me to come to Germany for training in the use of the meter, and its maintenance. I went over, and spent a week at the Technische Hochschule where he worked. Dr. Graf could speak fairly good English, although he had a strong accent. Without too much trouble I could understand him. His machinist and his technician could not speak any English. I had a basic reading knowledge of German. Somehow we communicated at those times when Dr. Graf was away teaching his class and such other duties he was required to do. At such times he allowed me to use his meter to make various tests in the lab. One time, when he had been away for about an hour, I showed him what I had done. He remarked that it would have taken him a week to have his technician do as much. I guess this was, because his technician did all the manipulations of the meter under the direction of Dr. Graf., While I was able to decide what to do and immediately do it. Dr. Graf and his wife entertained me evenings while I was in Munich.

On my trip to Germany, I had picked up a bottle of Scotch in Scotland on my way, so I would have something to drink while I was there. Dr. Graf saw to it that I had plenty to drink on my visit, and I hadn't opened my bottle. When it was time to leave, I decided that I had better have had a drink from the bottle before I faced Customs. I packed the bottle on top of my clothes in my suitcase, after one drink, to shield it from damage. On the way back, I bought a regular tourist pack of scotch while stopped in Scotland. Before landing, we had to fill out a slip about all of our purchases while we were overseas. I listed only my tourist pack of scotch. When I arrived at customs at Kennedy airport, he looked at my list and asked me to open my suitcase. I did. He asked if the bottle of scotch there was included in my customs list. I replied, "Of course not, This is my drinking liquor". He smiled and passed me on.

About a month later the meter from Dr. Graf arrived. I immediately set it up and made sure it was in working order. I then took it in my gravity truck to calibrate it by making observations a t about 50 milligal intervals along the New York Turnpike to Albany, then along the Northway to the Canadian border. I decide to carry on in Canada to Ottawa to their National Gravity Base. The man in Canadian Customs was unsure what to do, but finally passed me through on my assurance that I would be back on my way south within a day. He asked me of the value of the meter and I told him $20,000. Which was close to what I had Paid Dr. Graf.

This was the first time that I needed a calibration range, so I took our Frost Gravimeter, to provide the gravity values, and to make suitable choices of observation points. With observations at Ottawa and at our National base in Washington I felt that it would make a good calibration range. On my return, the same day to the Canadian Customs, the agent told me he was very glad to see me. He had had a severe dressing down from his superior for letting me through, and that my return saved his neck. With that experience, on future calibration runs, I terminated the north end at the Canadian Border. On the Thruway and the Northway I had been pulling as far off the road as possible to make the observations. This took about one half hour. At the last stop, a state Policeman stopped to see if I was in trouble. I told him what I was doing and he told me that it was very dangerous to be stopped along the road, even through I was well off on the shoulder and that I should stop at the maintenance areas instead. These were at about 50 mile intervals which were at about 50 milligal intervals. He further warned me that I had better get authority from the Thruway Authority to use the maintenance areas or I probably would have difficulties.

On my return to Lamont, I wrote to the Thruway Authority and got a letter of permission to use the maintenance areas. I needed it quite frequently in the next few years.

I wanted more range than going north permitted, and I wanted to include our national base, so I extended the southern range to Washington and then to the top of the Blue Ridge Mountains west of Washington. This made the whole range about 1000 mgals. In Washington the National base was inside the Commerce Department building, which I could not reach with my truck, so I set a base at the southwest automobile entrance to the Commerce building. With the Frost meter I made a good connection from there to the National Base. I later found that George Woollard and his students had done the same thing and we were using the same gravity value.

I had carefully measured the mileage between each of the calibration points, and wrote a good description of the actual site so that in the future all the sites could be revisited. On the southern part of the range, the sites were all kinds of things. They had to be near the road, but well out of the traffic. A number we made in church parking lots, others at cometary entrances, and even in one case by a telephone pole identified by a number of the Potomac Phone co. attached to the pole. I made up a booklet with the full description of each site for future reference. We used the range about 12 or 15 times while I remained at Lamont. My descriptions must have been adequate, because none had trouble locating each one. I made a copy of the booklet for George Woollard and he and his students used it several times.

With the gravity meter in hand and well calibrated I now needed to arrange for the use of a gyro-stabilized platform on a surface ship. I knew the Navy had such equipment, so I contacted ONR to see if any was available that I might use. They sent me to a portion of the Navy called Special Projects. Their purpose was to test out equipment that was considered for use on nuclear, rocket firing, submarines without having to tie up the subs in testing and evaluating. I was lucky, because they had just decided that they needed to include gravity measurements and were looking for suitable equipment. I told them that I had a gravity meter but that I needed a gyro stabilized table to see if it could be used on a surface vessel. They arranged for me to go out on U.S.S.Compass Island, their surface test ship. At that time they had a suitable stable table available.

Captain Sinclair, USN retired who I had recently hired, helped me bring my equipment on board in the Brooklyn Navy Yard and set it up on the stable platform. On this cruise it was planned to have a number of emergency drills at random times as well as providing us our opportunity. There were a number of other test equipments on the ship, such as new navigation systems, also on test.

Shortly after we got under way, while still in New York harbor, the speaker called the alarm "collision, this is not a drill". This was a 500 foot long 8000 ton ship. Such ships have a turning radius of about 1 mile, and cannot stop in less than two miles while at their minimum speed. We were near Staten Island making a turn and a freighter was coming across our bow. We could not lengthen our turn because of the shallow water near

Staten Island. In about 10 minutes, we brushed against the inbound freighter, doing comparatively little damage to either. Nevertheless, information was quickly exchanged between the ships so their legal departments and insurers could get together. We continued out to sea.

Our measurements had started while we were still at the dock in the Navy Yard. I had made gravity connections between our gravity bases at Lamont, and Columbia to the dock in the Navy Yard and to the stable platform on the ship with Frost Land Gravimeter.

In 1947 we had made gravity measurements on a submarine from offshore New York out to the deep water. I had the ship follow that line of measurements as closely as possible. At the deep water end I had them turn around and go back inshore along the same line, then offshore along it again. Then we went far off shore doing other tests for other equipment. After we were finished I had them again steam back along our line on the way in to the dock at Brooklyn Navy yard.

I was elated, as after correcting for a small drift in the Graf Meter, determined by the measurements at the dock before and after return, most of our values agreed with the submarine values within a few milligals. The maximum difference was 20 milligals, in just one case. This could be explained by navigational differences. The subs positrons could not be expected to be better than about 3 mile. The Compass Island's positions were supposed to be good to about 1/4 mile. There were three systems on board, each of which claimed 1/4 mile accuracy, but which differed by as much as 1 mile from each other. I could not determine which was most accurate from anything SP (Special Projects) had so I averaged the three systems in determining the track of Compass Island. These measurements were made in November, 1957.

Back on shore, I notified Dr. Tuve of our results and the IGY office. The latter published these results in their journal announcing various results of the IGY. The following February LaCoste made measurements on a surface vessel with a gimbal mounted equipment and claimed they were the first surface ship measurements. A few years later, the Society of Exploration Geophysics gave a medal to LaCoste stating, in part, that he had made the first surface ship gravity measurements. I did not protest, as several other things that LaCoste had done merited the medal. In fact, I expected someone else to correct this matter, but noone ever did. LaCoste found that he had some rather large corrections because of the gimbal mount on a surface ship. After several years of fighting to make the corrections, he put his meter on a gyro-stabilized table and advertised this great innovation in surface ship measurement.

We found that we had corrections up to 20 milligals, from cross coupling between horizontal and vertical accelerations even on a stabilized platform in severe sea states. Mostly these corrections were smaller than 3 milligals. By 1963 we had made an apparatus to measure this correction to include with our data. I will discuss this again later. LaCoste too had to make corrections for this with his equipment.

SP was pleased with our results and immediately ordered a copy of our meter from DR. Graf. This was complicated because Dr. Graf was arranging for Askania Werke to make and sell the meters. SP asked me to keep my meter on the Compass Island and to make a cruise across the Atlantic and into the Mediterranean, stopping at Majorca and Naples and return. The trip lasted about two months and was delayed a few months by the work of installing anti-rolling and anti-pitching gear on Compass Island. I was very glad to accept. (The anti-rolling device were two fins amidships that were controlled by a gyroscope to counteract the rolling motion. The anti-pitching gear was two fins attached to the bow which were supposed to provide an area to resist the motion of the bow. On the trip across the ocean, we hit a storm and the pitching fins felt like they were causing the front part of the ship to roll to the right while the rear was rolling to the left. My bunk felt like it was in the center and I kept thinking the ship was going to tear apart right through my bunk. I lost quite a lot of sleep during the storm. At the end of the cruise, the fins were removed and they made no further effort to restrain the pitching). SP also had found a man to carry on the gravity work for them and asked me to train him in all the nuances of the measurements. I willingly accepted that assignment just asking for Captain Sinclair to come along to help me.

All went well until we were about half way across the Atlantic, when a transformer in the power supply for the Graf Meter burned out. This put us out of business. I immediately sent a radio message to Dr. Graf to send me a new transformer for our arrival at Naples. The captain of the Ship normally asked someone on the ship, usually from the civilian testing crews to eat with him. This was considered a great honor. On this trip I was chosen. This turned out to be somewhat a dubious honor, as the captain insisted that all of his food be highly spiced with garlic, which I did not like. Nevertheless, I kept my peace and managed to survive the trip.

After the transformer burn out, over dinner one night, I told the captain that I was worried that the replacement would not get to Naples in time and that the trip would be a total loss for me. I mentioned that if I were back at Lamont, I would attempt to remove the winding on the transformer and rewind it by hand. But lacking the necessary supplies I could not attempt it here. He said that the ship, because of its mission, had a big supply of materials on board in the supply department.

I went there and checked and in fact they had a magnificent supply of magnet wire, which is used in many instruments. So I took the transformer out of the power supply and started to unwind the magnet wire, counting the number of turns as I unwound it. It turned out that there were four windings on that transformer, and of course (Murohy's Law) the burned out one was the bottom one! While I was thus occupied several engineers from other projects on board, like Sperry and North American Aviation, stopped by and told me that I was wasting my time, didn't I know better. I asked them how they thought the first transformers were made. They were all hand wound, that machine winding came along much later. They said the machine winding was so much superior, that I could never get hand winding to fit into the space. I told them it was better to try and fail, than to sit around wasting time.

After about a day of unwinding and counting, I was finally ready to try my hand at rewinding it. After about two days I finally finished it having to be very careful to level wind each separate winding. The supply department had all of the sizes of magnet wire that I needed. I installed the transformer and it worked. We were back in business having lost only about four days of measurements.

When we got to Naples, the replacement transformer could not be found. Calls to Dr. Graf assured that it had been sent, but our efforts to find it failed and we had to sail without it. A long time after we returned home, the replacement transformer turned up. Since my hand wound one had been working about a year by then, I decided not to replace it and save the replacement as a spare. It was n ever used.

On the compass Island each of the projects had a well displayed logo for their company, about 20 in number. Of Course we didn't have any. While I was waiting for Compass Island to get ready for the Med cruise, I had Dorothy draft up a shield divided into 4 parts. One part showed a seismic explosion in water with a fountain of water erupting, one panel showed a gravity pendulum, another panel showed a towed fish like we used for magnetic measurements at sea, and another showed the sound paths for signals to measure the water depth. Around it she drew a scroll with the Latin inscription "Nihil Ultima Terra Scientia" which I loosely translated into "nothing is greater than earth sciences" abbreviated

N.U.T.S. I taped this onto the gravity meter where all the engineers from the other projects could not miss it. Throughout the cruise, not a one tumbled, so on the last day I told one of them to look at it as only the first letter of each word.

The SP gravity man came up to Lamont and we showed him how to make gravity observations with land meters, some of the things about the Graf Meter that you could not do while you were making measurements, and sent him on a calibration run which I wanted as a check at the end of the Cruise. When SP's meter arrived, I arranged for him to use our truck to take his new meter on the calibration range and sent one of our techs with him. On a calibration run I always insisted that we have two people, although one could do the measurements quite well. In case of a car break down, one could stay with our valuable equipment while the other could go for help. Although nothing happened on any of our trips, and we never needed the extra man, I felt much more comfortable having him there.

In 1957 the Nova Scotian that we had as skipper on Vema contracted cancer and very shortly had to be replaced. He had been very satisfactory and had obtained a good crew of Nova Scotians. Shortly after he left us, sadly he died. I started to look for a new captain. Before I had gotten far, Henry Kohler applied for the job. He, too, was a Nova Scotian. He had heard of our skipper becoming ill and quickly came and applied for the job. Henry had a bad left leg. It could not be bent at the knee. He assured me that it would in no way interfere with his duties as Captain, that he had been going to sea for years with that condition. He claimed that he had long had an interest in research and that this was his opportunity to participate. I was satisfied that he would be a good captain, so I took him down to Dr. Ewing to interview also. Ewing was satisfied and we decided to give it a try. Thus started a long collaboration that was very successful.

In 1958 the foremast showed signs of deterioration. This was because the galley stove was vented up the mast. The accumulation of soot and grease had collected moisture and caused rust on the interior.
The bow had started leaking around the bowsprit, and we could not stop the leak. Captain Kohler convinced us that he could get the masts replaced and the bow configured without a bow sprit at a modest cost in Nova Scotia so we had the work done there. I had wanted to have the masts shorter, but Henry convinced me that would be a mistake because it would disturb the meta-centric height too much. We had long given up sails, except for a small jib and a storm sail on the mizzen to help in stormy weather. A

complete set of sails cost in excess of $20,000 and they rotted on the booms. It seemed that the wind was coming from ahead no matter where we wanted to head, so the sails were seldom set. It was impossible to keep them dry, when they were always furled and we gave up replacing them as too costly and only carried a few steadying sails.

About this time, The Grace was given to us by the W.R.Grace Co. She was about 80 feet long, a two masted schooner. I went with the crew to pick her up in Boston to sail her to New York. John Ewing had a project that he thought could be well handled on Grace. They sailed a few days before we did on Vema. Vema was starting another extended cruise, and I went on the first leg to make sure all of the gear was working right. Shortly after leaving New York for Bermuda, we ran into a severe storm. Pretty soon we had to lay too as the weather was too severe to proceed. On the radio we heard that Grace, which was south of Bermuda was also having heavy weather. After about a day hove too, we were contacted by the Coast Guard. A U.S.Navy Blimp trying to get back to its base in Lakewood, New Jersey had been blown out to sea and could not make any headway towards N.J., so they had altered their plans and were heading for Bermuda. They were running low on fuel and might have to ditch in the ocean. We were the closest ship to them and the Coast Guard asked us to get underway to their position in case they had to ditch.

We got underway at full speed, despite the heavy seas for their position. We suffered heavy pitching and rolling that beat up all on board a good deal. While underway we heard Grace call for help. It seemed that they were taking on water faster than they could pump it out, despite laying too. Some other ship was sent to stand by in case Grace had to be abandoned. After about 12 hours we got to the blimp's position and contacted them on the radio telling them we were standing by in case they had to abandon ship. They were trying to go upwind into Bermuda. About this time the storm moderated some, and they were able to approach Bermuda faster than we were. We did our best to stay with them. They finally made Bermuda before their fuel ran out. We made Bermuda about a day after the blimp arrived.

WE tied up in St. GEorges. We could see the blimp at the air field across the harbor. We made our usual three day stop in Bermuda to refuel and revictual. Noone from the blimp came over to thank us for standing by under them in case they had to ditch. We had suffered some damage to the scroll work at the bow from running at high speed in the storm conditions. Grace managed to survive, completed her work and returned to New York. We decided that she was not rugged enough for our duty and decided to sell her. Meanwhile Vema continued her way southward.

I believe this was the first round-the world cruise for Vema. I was involved with the gravity measurements on the Compass Island most of the time of the cruise, and I don't remember anything unusual about the Vema Cruise.

At the start of the next Cruise, I again went out on the first two legs. The first leg was New York to Panama. We did our usual underway program but mostly got all the gear working properly. After transiting the Panama Canal we cruised northerly in the deep water along the coast of Central America. We started getting long cores. The sediment was softer than most places we had been. There was a lot of hydrogen sulfide in the core. After taking several cores 60 feet long and showing mud still on the coring weight. I decided that we could get longer cores if I could arrange the setup on the ship. 60 feet was the longest length we could accommodate with the brackets over the side of the ship which we used to set up the core rig. I decided that we could put two 20 foot pipes together, and by putting a two by four extended over the side of the ship just aft of the after house to support the additional pipe, we could move these items far enough aft that we could assemble the core head and the top 40 feet of core pipe, and by sliding the 40 feet forward could attach the additional 40 feet of pipe. Thus we assembled an 80 foot rig . Normally when we sent the core rig over, we lifted the main weight on a chain fall and a man near the bottom end of the core pipe would lift the remainder out of their supports allowing them to fall overboard. The core rig would then be suspended by the chain fall at a bout a 45 degree angle. By lowering the chain fall we could transfer the core weight to be supported by the wire rope in a vertical position. Removing the chain fall, we were then ready to make. a lowering. With the extra length of pipe we had to use two men to lift the pipe out of the supports and drop the core pipe into the water.

We had a counter on the winch that counted the length of wire that we had paid out. Bemuse the wire stretched under the load o core weight pipe and the wire itself, the amount of wire need to reach the bottom was not the same as the depth recorded by the sounder.. The sounder depth was never quit right either because the sounder used a sound velocity of 4800 feet per second to indicate the depth, while the average velocity was usually about 4850 feet per second. The ship drifting on the surface due to water currents, and blown by the wind would often cause a wire angle of up to 15 degrees. which meant we had to let out additional wire rope to reach bottom. With experience we could learn to estimate these factors so that we would know when the core rig was approaching the bottom. We lowered the rig toward bottom quite fast. averaging about 225 feet per minute until close to bottom. About the last 600 feet we would lower at speeds of about 10 feet per minute. When the trigger weight hit bottom, it would allow the core rig to free fall the last 20 to 30 feet. When the weight was released there would be a change of tension indicated by the tension meter on the winch. This indication was quite quick and small, and it was difficult to detect since the tension meter was moving because of the wave action on the ship. The winch operator and the chief scientist (and sometimes others) were watching carefully to detect bottom contact. The winch had to be stopped as quickly as possible at the signal to avoid damage to the wire rope.

Withdrawing the core from the bottom sometimes was tricky because of the suction of withdrawing the core from the bottom. Especially troublesome were bottoms with hard layers separating soft layers. With 10,000 pounds of core weight and wire over the side, and a wire with a breaking strain of 20,000 pounds, one had to be careful if you didn't want to.leave the core on the bottom taking a large amount of wire rope with it. Once clear of the bottom, we were able to wind back the wire rope at a sped of about 200 feet per minute. Great care had to be taken not to pull the corer into the head block, which would cause the wire rope to break too.

After taking several cores 80 feet long, I decided to try to get one 100 feet long. This meant extending the core pipe beyond the stern of the ship by about 5 feet. We rigged another outrigger to support the additional pipe at the transom. At this point, the pipe was about 8 feet away from the side of the ship because of the curvature of the hull. We assembled two 20 foot long pipes together, then assembled 60 feet of pipe to the core weight. Sliding the shorter length forward we were able to assemble the 100 foot long core pipe. This time it took three men to throw the pipe over.

When we recovered the rig, we saw mud on the bottom of the main weight, but not much. I was satisfied that we had taken a 100 foot core. Later back at the lab, I was informed that about 20 feet of the core had been sucked in by the interior piston when we recovered the core, so we had only taken an 80 ft. core despite all the work of trying to get a 100 foot core.

While we were in the Mid-America trench region we had been seeing a scattering layer at about 400 fathoms depth. Scattering layers had first been observed in WWII. Work then and after the war had found that it was made up of small microscopic plankton like euphosid shrimp. These critters were photo-phobic. During the day the layer moved to depth, about 400 fathoms on a sunny day. When clouds covered the sun it would rise to depths like 200 fathoms, depending on the cloud size and thickness. As soon as the cloud would pass they would again descend to their greater depth. The echo was kind of cloudy, not a solid echo like the ocean floor would give. This was probably because the critters were distributed somewhat in depth. At night, unless it was a bright moonlight night, the layer would rise to the surface. We could often watch squid feeding on them on the surface at night.You could watch the layer descend after dawn and rise as dusk approached.

While we were in the Mid-America trench area, we saw large sharp echoes appear in the scattering layer. We interpreted that as large fish preying on the smaller critters of the scattering layer. Bob Menzies, a biologist, had joined Lamont and was with us on this cruise. He had been pondering the question of eels. In the Atlantic the elvers, were about 2 inches long and would appear in the middle of the North Atlantic. They would make their way to North America and Europe as they grew to inhabit the streams and lakes and would grow into eels about 3 feet long. As adults they would make their way back to the Atlantic, spawn and start a new generation of elvers.

In the Pacific, the elvers were about 8 inches long. Nobody knew where they went or how they grew. Assuming adults in the Pacific would grow in the same proportions as the ones in the Atlantic, this would mean the eels in the Pacific might be 12 to 20 feet long. None had yet been found. Bob suggested that the large echoes we were seeing in the scattering layer might be the missing eels. We decided to try to catch one of the large critters to see. We had nothing on board that we could use to fish with, so we went down to the shop and fashioned a hook out of 3/8 steel rod. We bent it into the shape of a large fishhook, sharpened the end and raised a portion near the end to make it hard to remove once set. We obtained a flying fish which had come on board during the night. These were about 8 inches long . We impaled one on the hook we had made, attached it to the hydrographic winch wire and lowered it to the depth of the scattering layer. In about 5 minutes we saw the tension meter start to jump around indicating something was trying to take our bait. We immediately started hauling in the wire rope at top speed. The tension gauge showed that we had a large weight on the end. When we had retrieved it so the hook was at a depth of about 100 fathoms, the tension gauge went to its maximum and then abruptly returned to a negligible reading. We brought the hook on board and found that it had been straightened out. The maximum tension reading on our gauge was about 3000 pounds.That meant that whatever we had hooked had strength enough to exert at least 3000 pound force to escape our hook. Our fishing experience was ended.

Later that night, Captain Kohler called me to his cabin. He said it appears that you had a large critter on your hook. What did you plan to do with it if you got it up to the rail? I told him that there was a long tradition at sea, the fishermen brought the fish to the boat, and the Captain brought it aboard. He said, suppose it had been a 20 foot eel and it had decide to come on board, we had no gun or any weapon that could subdue such a large creature. He thought it was too dangerous and advised me that as Captain that he forbade any more such experiments.

One of Bob Menzies projects was to try to capture some creatures from the bottom of the Mid-America Trench. A Swedish expedition had captured some there, but had not recognized it until much later when examining the samples at the end of the cruise. As a result they had been damaged and could not be properly described and identified. Menzies had built a biological trawl which was designed to drag in the top few inches of the bottom and capture creatures that inhabited the bottom. We made several trawls with it and captured five or six of the creatures. They were in good condition except they had five hearts and they had exploded from the gas dissolved in the blood expanding when we brought them from the pressures of the deep, about 8,000 pounds per square inch to atmospheric pressure of about 15 pounds per square inch.Nevertheless he was able to preserve them and fully describe them after they were returned to the lab.

Later in the cruise, we were able to obtain some more from the Peru-Chile deep off South America. These were of a slightly different species. The critters were called Neopolina. The new Species he called Neopolina Vamae Ewingi.

Before the Swedish expedition none of these creatures had ever been found even in the fossil record. A biologist had inferred they had evolved from an earlier conical creature. These developed longer and longer cones until they could no.,longer support themselves and they had curled up into what we now call snails. This occurred in Devonian times, about 400 million years ago. It had been inferred that this species must have existed although none had been found even in the fossil record at that time. They had also estimated that they would have lived in a shallow water environment. We had, of course found them in very deep water. This meant that if they had been a shallow water creature, they had adapted themselves to deep water, or that they must have always been a deep water fauna. If the latter, it would be reasonable to assume that this living fossil had survived because the ocean deeps provide the most unchanging environment of anyplace on earth.

Another finding from this leg of VEma 18, was a layer in the sediment that extended all through the Mid-America trench that we had visited, about 300 miles seaward from the trench and all along the coasts of Middle America and Peru and Chile as far as Antofogasta Chile where our leg ended. This layer was at a depth of 6 to ten feet under the ocean floor. We had obtained about 20 ocean bottom cores and had been able to identify the layer as a white sandy looking layer. It was later identified as glass spicules about 1/4 inch long and about 1/32 inch in diameter. It was volcanic ash. The layer varied from about 2 inches in thickness to about 6 inches in thickness. Assuming that it would be found everywhere within the convoluted ship's track designed to study other things, it covered at least 30,000 square miles. This was a minimum, as it continued everywhere beyond our ship's track. It must have been a monstrous volcanic event. Dave Ericson, who studied the ash the most called it the Worzel Ash in his publication. When the studies were published I asked my family who else they knew who had their white ash spread extensively across the Pacific Ocean.

In 1959 the first Oceanographic Congress was held in New York. It was held in the United Nations building. There were people from most of the countries of the world, at least all those that bordered on the ocean. I prepared a paper on the geoid in the ocean area. I especially noted that the geoid was depressed in the northern hemisphere and enlarged in the southern , at least in the oceanic ares. Noone seemed to be impressed, but a few years later, when NASA announced that the world was pear shaped on the basis of the geoid determined by satellite, a big fuss was made of the discovery. I guess the comparison to a pear made the difference.

At this time. Chuck Drake and I published the structure section across the Hudson River beneath the Thruway Bridge. This was the seismic work we had done about 1950. Chuck wrote the first draft. When it was completed, we sent it to the New York Academy of Sciences for publication.

About two days later, I received a notice from the New York Academy of Sciences of a prize competition for the best paper published in the competition. I wrote them and asked them to include our paper we had just sent in, in the competition. The executive secretary replied that they could not do that since it had been sent in before we had received the notice of the competition. I replied that nevertheless, we had sent it in before the competition had started, that the time we received the notice should have nothing to do with it. When I received a reply refusing to consider it, I sent my resignation from the Academy, and I never again attended any of their meetings..

At that time we also published a paper for the rapid calculation of gravity anomalies from irregular shaped 2 dimensional shape by computer. Manik Talwani was the first author, I was second, and Mark Landisman was the third. They provided most of the computer expertise. We approximated the body by a multi sided polygon. By adding several layers of such polygons any body could be approximated. We used it to show how the computations worked on a sea mount and the Mendocino escarpment. Later Manik and others extended the method to three dimensional bodies, but we seldom had enough information about three dimensional bodies to use it.

With the help of Bernie Luskin, I had built our own stable platform for use on Vema. This consisted of a small stable platform that we obtained from the Navy, which was slaved to a large platform with servomoters. This unit had been a gun director. We adopted it so we could put the Graf Sea Gravimeter on the platform. This worked quite well in moderate seas, but could not keep up with severe conditions.

I decided that we could work in more sever conditions, if the ship rolling could be moderated. I studied ways in which rolling could be controlled and discovered a system which used the free surface of the water in a tank for that purpose. The tank extended across the full width of the ship , and was half filled with water. As the ship rolled, the water was moved laterally by the rolling, and by putting suitable baffles in the tank, a phase angle of 90 degrees could be achieved for the water in the tank. This provided a counteracting force to the rolling, and reduced rolling by about 50 %. I went to ONR and convinced them this would be good for our gravity measurements, and they agreed to allow us to spend money to so equip Vema. I went to Hank Skjerding and got him to find a ship's architect who could design a suitable tank for Vema.

The architect, whose name escapes me, studied the system and Vema and decided that the only way we could do it on Vema was to put the tank overhead above the bridge. It was approved and during the next overhaul of Vema, we had a steel tank six feet by six feet extending the width of the ship built over the bridge. When half filled with water it held about 15 tons. At first Capt. Kohler was skeptical of the system , but when it actually did reduce the roll by nearly half he became convinced of the merit. Besides, I told him, it always provided an emergency supply of water if the ship's supply ran low. It helped our gravity measurements greatly, and we were now able to make measurements in all but the most extreme weather.

A few years later, when Vema anchored overnight near Antarctica, the ship began to roll excessively. Capt. Kohler was mystified until he investigated the anti-rolling tank and found out it had frozen during the night. When he told me of this experience, I suggested to him that he fill the tank with rum, and it would not freeze up again. Besides he could have a party from time to time. He declined saying that he had enough problems without having to protect a large tank of rum.

A by-product of the anti-rolling tank was that the ship was more habitable and the work on board had to be curtailed much less often due to weather. It made over the side work much easier too, since it was just as effective when hove too as under way. A few years later, when the Navy and NSF sponsored a group of Oceanographic Ships, they were designed with anti-rolling tanks from the start. The tanks were placed near the center of the ship's motion and quite low in the hull.

In 1960 the Vetlesen Foundation set up the first Vetlesen Prize. This was to be the equivalent to the Nobel Prizes but for the fields of Earth and Planetary Sciences. The foundation awarded the first prize to our director, Maurice Ewing. There was a large cash prize and a Gold Medal. The medalist was awarded the prize at a dinner in Low Library and gave a short acceptance speech. The next day the medalist was expected to give a seminar on some of the work for which he received the medal.

It was then set up so Columbia would select future winners at about two year intervals with the help of previous medalists. If there was no suitable medalist, the medal was not to be awarded. The cash prize was to grow with time as the endowment increased with good investment management.

At about this time NASA selected a group of seismologists to design a seismometer for installation on the moon. Gary Latham at Lamont was chosen to see to its construction and testing. This took a big chunk out of Gary's time for about three years. However he felt it was worth it when the first unit started sending data back to Earth. I think a couple of more seismometers were installed on later moon landings.

We needed to make several modifications to Vema and Navy cost accountants came to Lamont to verify that we were following proper procedure in getting shipyard estimates. Our project officer was also present to establish his approval of the changes we wanted to make. All went pretty well until we wanted to upgrade our Radar to a newer one with better resolution and a bigger screen. The cost accountant objected and we had seemed to have come to an impasse. At that point we stopped for a coffee break. During the break Dr. Ewing was telling several of us about his work with the Argentineans near Antarctica. He stated that at one time they were amongst a field of 50 or more ice bergs, a pretty hairy situation. The accountant asked how the captain kept track of them all after dark, and Doc said that's what we want the new Radar for. When the conference resumed, the radar was authorized without any further ado.

We were approached by the developers of Sterling Forest to see if we would move all of Lamont to Sterling Forest. They thought that having a strong thriving operation like ours, would attract many Industrial Operations to come there. We had been needing more space, and this seemed a good opportunity to get it. They would build labs to our specifications, would help our staff members to build houses nearby, and would establish a bus system between our buildings and the Columbia Campus. They had established a large flower garden there which attracted quite a few tourists to the area. It was located near Suffern, N.Y. Which was about 50 miles from the Columbia Campus. Doc took most of the senior staff with him to look at the development and the proposed location of our buildings. After a period of several weeks of negotiations and visits in both directions, Doc put it up for a vote of the senior staff. The vote came out negative, because the staff felt that our distance from the main campus (20 miles) was already far enough. More was too much.

Still we had grown so much that we needed more room disparately. our administration group headed by Arnold Finck was especially strapped. They had used all of the front hall of Lamont Hall, and had also taken some space in the basement. They were having trouble to find place for more files needed to keep all our projects straight. The professors , senior scientists , technicians and graduate students were packed four to six to a room. There was hardly room to move in some of the rooms. I had a room in the attic, mostly covered by a sloping roof.But I was lucky, bemuse there was a space next to it suitable only for storage, so I could keep the large charts and diagrams needed for the gravity work there. even though there was barely headroom. John Ewing and a couple of his group were across the hall in a space broken up by several chimneys which passed through the area.

When Sterling Forest had been turned down, I started to look around to see how we could get more space. Doc was trying to find some donor who would help us to fund another building. We were getting nowhere. I read about a firm in New York that would put up buildings, rent the space to the establishment which needed it for a period of ten years, at which time the building became the property of the establishment. I contacted them and discussed our needs. It sounded reasonable , so I went to ONR and determined that they would have no objection to such an arrangement.

When it came to actually start making some plans we hit a snag. The builder wanted the land on which the building sat and a reasonable area around it so that if we could not continue to rent it for the ten year period, they could rent the space to someone else. Columbia refused to consider making the land available, although I had suggested that the building be built next to route 9W on the periphery of the Lamont property. This squelched the whole thing. When I went to ONR and told them that we could not get the space we needed, they suggested that they could make a grant to provide more space since the work we were doing was important to them. We jumped at the chance.

We had a Butler building designed that could be built on top of the tennis court, making the foundation work negligible, and had a building designed for Oceanography to be placed on the bluff overlooking the Hudson River .

The Butler building was cheap to build and would provide space for the electronic shop as well as the administration offices. The Oceanography building was designed to house six groups on each of three floors. Largely by my insistence, each group had a large central room, with a group of smaller rooms around the central room for a scientist to have an office space to retreat to when writing papers etc. The central room would provide a large space for handling charts and plotting cruise tracks and other operations requiring large drawings. I also felt that this arrangement made sure that everyone working in that group would keep track of the whole work of the group by passing the work in progress, frequently. The building design exceeded the amount of money available, so we had the third floor shortened by one group, with the notion that it could be added later. Construction started towards the end of the summer.

Needless to say, the additional space was never added.

Our early efforts at sub bottom profiling had proved fruitless bemuse recordings were made once per hour, and between shots, the sub bottom had changed too much. We had started a program of recording on an oscillograph reflections from the bottom from explosive shots. This was accomplished by towing a hydrophone behind the ship. Just before the shot, the hydrophone was slacked to quiet it so that the bottom reflections could be observed. After the shot, the hydrophone was pulled back close to the ship for another shot. Shots were made at 5 minute intervals. Because this was so labor intensive, we could undertake it at only select locations. John Ewing and Rusty Tirey adapted one of the Times Facsimile Drum Recorders, we had been using for some time to record the ocean bottom echoes, to record successive shots on adjacent traces. To reduce the labor, the hydrophones were slacked attached to a BT winch wire. To recover the hydrophone the winch was used to pull the hydrophone back near the ship, ready for the next shot Shots of 1/2 pound of TNT were used. With this technique we were able to add this program to our underway program of measurements.

Mark Langseth had been able to develop the heat flow measurements to operate while attached to the core tube. We added a space within the coring weight to include the recording apparatus. Conducting cables were run down the core tube to outriggers placed at three or four intervals along the tube depending on the length of core we expected to be able to take. These outriggers were about 3 inches radially from the core tube. This allowed the measurement to be made using thermistors before the heat of the tube caused by friction with the sediment could flow to the thermistor. The topmost thermister normally recorded the ocean bottom temperature. It wasn't long before this measurement was made on almost all the daily cores taken by the ship. This added another measurement to our repertoire of measurements made on stations.

About this time Vema 18 occurred. This was the first of the many around the earth trips that Vema made in the next few years. Stations were occupied daily along the track in all the traversed oceans. Underway data was taken between stations. We made numerous two ship refraction measurements whenever we could obtain a second ship. These refraction observations were the principal determinant of the ship's track.

In the early summer of 1961 the dedication of the new Oceanographic building on the Lamont Campus took place. The administration building had been added with out any fanfare a little earlier. President Kirk, Dr.

Ewing and a representative of ONR were the principals of the dedication ceremony. It was a great relief to the staff to finally have enough room to spread out their charts and diagrams.

We had obtained some of the Times Facsimile continuous recorders.This allowed soundings to be made on a continuous sheet which lasted about ten days before needing changing. Previously, with the drum recorders, we had to change the paper each hour underway during the cruise. The continuous record made it much easier to tell which 400 fathom interval was represented on the 18 inch wide recording of the depth. The many side echoes were easier to visualize too.

The outstanding event in 1962 was on a trip to ONR in Washington,I was asked if Lamont would like to receive the first AGOR. These were Navy owned vessels designed especially for Oceanography. They were to be made available to the various Institutions. About six of this design were made available each to one of the institutions in the community. Agor 3, the Robert D. Conrad was the first to be completed. It had been planned to make it available to the Woods Hole Oceanographic Institution. They had decided to build a ship more specifically attuned to their own research programs with funding from the National Science Foundation (NSF). Hence Art Maxwell, head of the Geophysics Branch of ONR, had made the offer of the Conrad to Lamont. I replied that I would have to confer with Dr. Ewing first, but that I was sure the answer would be yes, if we were not asked to give up the use of Vema in exchange. We felt it would be hard to match the efficiency of Vema. It would also be necessary to increase our support from ONR and NSF so that we could make use of both ships.

When I reported back to Dr. Ewing, he was elated about the chance to extend our work, and we were assured that our support would increase to assure we made full use of these facilities. This would mean twice as much underway data, along about twice as much track and a doubling of the bottom cores, photographs and other bottom measurements. I reported back to ONR that we would be very pleased to undertake the additional work that Conrad would afford us.

We now had a number of additional problems. One was that the deep sea winch on Conrad was slower than the one on Vema and more cumbersome and took up much more space. The one on Conrad was a winding machine. this meant that two driven triple slotted wheels about three feet in diameter were placed in line with each other on deck, with the wire rope fed from one to the other and back to the first three times. This provided enough friction to recover the wire rope. A second machine called a winding machine below decks was supposed to operate in tandem with the winding pulleys maintaining a slightly greater sped so the the wire rope was kept under tension between the two devices at all times. The wire rope of course had to be fed through a small port in the deck to the lower deck to the winding machine. We protested this setup and requested that it be removed and a copy of the Vema winch be installed on deck instead. Our request was denied, and we agreed to do our best to make the system work.

There were three major work areas on the ship. A cabin on the main deck which was intended for work with wet samples such as water bottles, cores etc. and a second on deck which was to be used for the electronic equipment and various recording devices . The lower lab, on the lower deck, was supposed to be used for equipment maintenance, construction and such work that did not require easy access to the main deck. The electronics lab had been equipped with stainless steel tables, with one shelf underneath. These were excellent tables, but totally useless for electronic gear which was mostly housed in vertical racks.

Back at Lamont we went to work feverishly to build a set of equipment that would duplicate that we had on Vema. Naturally, we could not resist making some improvements as we rebuilt the gear, causing some problems later since items were not interchangeable on both ships and we had to keep track carefully when spares and replacements were required in later years. About five years later, when in the course of improved methods, nearly all the equipment was replaced on both ships, this problem disappeared.

Finally the ship was completed and was scheduled to go to sea for the Navy acceptance trials and shakedown. Art Maxwell from ONR came aboard and the shipyard crew of installers went along to operate the ships equipment required for the scientific observations. After about a day at sea, the ship came to a halt and the lights went out. Investigation turned out that the fuel ballast system was allowing water to get into the fuel which caused the diesel engines to quit. All of the engines were fed fuel from the 1200 gallon day tank, which was filled about once a day from the fuel ballast tanks. These were tanks which were open at the bottom to the sea so that as fuel was removed water replaced it maintaining the balance of the ship. This system had been used for a number of years on submarines. On Conrad the rolling and pitching had mixed the fuel with sea water. I don't know how this was handled on submarines, but on Conrad it caused all of the engines to stop. The water was removed from the line filters at the engines and the engines restarted, only to fail again.We couldn't even send a radio message ashore requesting a Navy tug for help getting back to port. The engineers gave up.

John Ewing had a lot of experience in the service with diesel engines, so he and I went to Art Maxwell and told him that we thought we could disconnect the day tank from the fuel system, filter the water out of the fuel in the day tank and get enough fuel to run one generator and one engine to get back to port. Art went to the Navy people and urged that this be tried. Since no alternative existed, and no one wanted to drift around the Atlantic until another ship came along, our offer was accepted. We succeeded and were all glad to return to Jacksonville.

After we limped into port with water in the fuel, the shipyard installed a Laval separator. Its purpose was to take the fuel from the fuel ballast tanks which was contaminated with water and separate the fuel from the water. The good fuel was then pumped to the 1200 gallon day tank. All eight diesel engines on board were fixed so they only operated from the day tank. All of the fuel systems of the engines had to be taken apart and cleaned out to make certain all the contaminated fuel was removed. After this was complete, we again went out on the trial cruise which had been aborted by the fuel fiasco.

This time all systems on the ship were tested and were satisfactory except the winding machine that was supposed to take up the wire rope as it was recovered. When we got to the shipyard they called the manufacturer and told them there were problems. They answered that it was just those Lamont people that were prejudiced against the system. The shipyard superintendent then told them that he was holding several bolts that had sheared off, in his hand. That had nothing to do with the Lamont people. They were the bolts that were supposed to hold the winding machine below decks in position. This was dangerous bemuse the trial cruise had been only a short cruise. On a longer cruise, if the remaining hold down bolts sheared off, the 6 tons of machinery would be loose below decks and could tear the ship up and even go through the side of the vessel causing it to sink. They increased the size of the hold down bolts, and added additional ones too. The Navy accepted this modification and accepted the ship from the shipyard.

It was up to us now, to complete outfitting the ship and to get a complete crew. On the acceptance trials some of the crew were from the shipyard to supervise that everything was working the way they expected it to and to have their own people there in case the Navy acceptance crew found anything wrong.

We still had work to do to install our equipment in the labs and to check it all out. This took about two weeks, even though we had done a lot the last two weeks before the acceptance trials. During this period, Ed Newhouse who we had hired as Doc's assistant, hired a crew. Just before we were ready for sea, the maritime unions appeared and demanded that the ship be manned by union crews. They had found out that the Navy intended to build several ships for the oceanographic fleet, and they wanted to insert themselves, although none of us had previously been bothered by them. They picketed the pier on which we were docked, and the Navy building in Washington in which ONR was housed. We did not want anything to do with the unions and requested help from ONR. ONR declined to help saying that we would have to work it out for ourselves. They could not take a position opposing the unions. Shortly all of our crew resigned. It was clear that they had been intimidated. We acquired another crew, which appeared one or two at a time. We had to register them with the Coast Guard. AS soon as we did, they were approached by the unions and resigned after only a day or two. Newhouse and I had to cross the picket line each day. The pickets were quite aggressive, but stopped short of any physical efforts. The organizer of one of the Unions insisted on talking with me. I refused saying we wanted nothing to do with the Unions. With much difficulty, we finally assembled a crew and sailed for a short cruise at sea before returning to Lamont. The Unions kept trying to intercept our crew, and we lost a couple of crew members that did not want to antagonize the unions. Nevertheless we got to sea and started to work.

We had never had any trouble with unions with Vema. This was because she had been registered in Panama when we bought her. Panama had right to work laws and their ships were free of union help. Early on, we had looked into reregistering Vema in the U.S. and found it was virtually impossible. There were so many changes to the ship that would be required that it would be cheaper to build a new ship than to make them, and we did not not have that kind of money. Also it would require a special act of congress which we had been assured was next to impossible to obtain. We had been thoroughly satisfied with the operation of Vema, and the government had made no moves to require us to do it. So we had left it as it was.

In the spring of 1963 the submarine U.S.S. Thresher was lost near the Gulf of Maine. I first became aware of this tragedy, the night before I was to leave for the annual meeting of the American Geophysical Union in Washington, D.C. I received a call from one of the New York newspapers advising that the Threher was overdue and presumed lost in the vicinity of Georges Banks near the continental shelf. It had been suggested to them that the submarine might have run into an uncharted sea mount causing the loss. The Thresher was making a test dive after completion of major overhaul work at Portsmouth Naval Base. They wanted to know what, I as an Oceanographer, thought of that. I told them that was impossible. That the steepest slope we had ever found in the oceans was less than 45 degrees, and the submarine would be aware of any sea mount through its sonar and soundings so that they would have plenty of time to avoid any uncharted peak. I also pointed out that the area in which they were working had been extensively well charted and that it was almost inconceivable that an uncharted sea mount could be there.

I later learned that Gordon Hamilton at our Sofar Station in Bermuda searched their records and noticed a signal at about the time of Thresher's loss. He then acquired records from some of the Sosus stations and triangulated on the signal and determined that it was very close to Thresher's dive position. It was the signal made when the hull imploded from the pressure as it sank.

At the Geophysical Union meeting the first day, I was approached by a representative of the Geophysics Branch of ONR to attend a meeting in their office, within the hour, about the Thresher loss. By that time the Navy had not heard anything from Thresher for several days and had decided that she had sunk. Art Maxwell chaired the meeting. Representatives from Hudson Labs and WHOI were also there. There night have been others, but I don't remember anyone else. Art stated that the Navy considered the Thresher lost and wished to initiate the search for her immediately. He also stated that the Navy through ONR had supported us for a number of years, and that it was up to the research community to return the favor. We all agreed we would do what we could. We agreed to make one ship available to search the area with whatever tools we had available. We pointed out that there would be large expenses involved and asked who would pay them? Art advised us to use any ONR funds we already had available and that they would be replaced in due course. We were to get ready as quickly as possible and sail for the area of the test dive, whose coordinates were given to us, as soon as we could. A Navy Captain would be in charge of the search area and would assign us search areas as we arrived. Captain Andrews turned out to be that man.

Swede Momsen was also present. He headed Special Projects Branch at

ONR. He promised that the group that found the Thresher would be given a $300,000 grant without any strings about what it was spent for.

Chuck Drake had attended the meeting with me. After the meeting, we returned to Lamont that day and started to get Conrad ready to sail. We got together senior staff members in magnetics, sounding, underwater photography and explained what was happening. Chuck Drake, Sam Gerard, Jim Heirtzler, and I were the senior staff members that would go to sea. We each collared a few technicians and or graduate students from our staffs to assist and we each obtained anything we thought might assist the regular equipment we had on board for the search. Sam who had been working with large moored buoys put several on board with appropriate mooring gear. These were to prove important.

The second morning of our return to Lamont we sailed for the search area. We arrived to find a Destroyer with Captain Andrews on board nearby. Chuck and I went on board and Captain Andrews assigned a search area to us. We went there and started sounding in the area. On our way to the search area, we had rigged a magnetometer to tow near bottom, since Jim had computed the probable signature and determined that we could not detect the sub that way unless we were within about 100 feet of the bottom. Normally we towed the magnetometer on the surface far enough astern to be free of the ship's own anomaly. Atlantis from WHOI with Brackett Hersey in charge, the Gibbs from Hudson Labs, with Bob Frosch in charge and a ship from NRL (Naval Research Laboratory) with someone in charge I 'didn't know and whose name now escapes me, were soon on the scene.

We were each assigned an area about a mile apart in the vicinity of the location at which Thresher dove. Spring and early summer in the Gulf of Maine is a foggy time and we were soon maneuvering with gear near the bottom 8000 feet beneath us in a fog. This was quite hazardous as we were unable to maneuver quickly. We soon realized that we needed some better way to navigate than Loran. After about 10 days Capt. Andrews ordered us to return to Boston on Friday, prepared to sail again Sunday night to return. We were commissioned to obtain radar repeaters for installation on buoys in the area for better navigation. The company that made them was located near Lamont. With difficulty, we obtained four units before sailing Sunday, and obtained some junk automobile engines for buoy anchors. On our return to the area we set three buoys outside what was considered the probable area that the sub might have reached on its disabled descent to the bottom. We mounted a radar transponder on each buoy. The batteries on the transponders would have to be replaced about each two weeks. We thought our navigation problem was solved, but we soon found that a couple of the ships radars were tuned to a different frequency. Captain Andrews arranged for the Navy to provide suitable crystals and made a quick trip to Boston to collect them. On his return we could all now get returns from the buoys from our radars and our navigation was greatly improved.

The fog continued and our hazardous surveying continued. In this period I could only sleep because I was thoroughly exhausted. Each minute the fog horn mounted on my cabin bulkhead was sounded for 10 seconds

Our magnetometer failed because the commercial lead in that carried out the signal to the towed fish and the return to the instrument package before sending the signal back to the ship, leaked. After the third try, all of which leaked, I got disgusted and went to the machine shop on board and fabricated lead ins like those we had used in our deep instrumentation in the period of 1938-40. This cured our trouble and we had no further trouble with our deep towed magnetometer. We also towed a camera that Ed Tthorndyke had developed at Lamont which had not yet been tested. This camera could take about 300 pictures without having to be recovered to replace the film and batteries. It had an electronic flash to give light. It was triggered by bottom contact. We would lower until bottom contact was made, recover it about 100 feet and lower again about 30 seconds later to take another picture.

We found maneuvering in the area quit difficult because there was a current of about a knot, and we were only able to move at about 2 knots if we wanted the equipment to remain near bottom.

We divided our staff into two watches of six on and six off. Chuck and I each took one watch on the bridge keeping a track from the radar transponders and recording the times at five minute intervals. Jim and Sam each supervised a watch for the deck operations of the camera and magnetometer and the lab watch on the sounder and magnetometer. Jim had computed the expected signature of a pass over the submarine and had posted it beside the magnetometer recorder so the watch would know what to look for. Captain Andrews had us go into Boston After 5 P.M. Friday night and sail Monday morning so that we would be on ;location by daylight Monday. He said this was necessary to keep our watch standers sufficiently rested to be alert. After about two weeks, the fog finally let up and we were more comfortable maneuvering near each other, with our radars tied up in the navigation, so they could not be used to avoid collisions.

About the third week there, I had been scheduled to be best man at my father's second wedding. Captain Andrews insisted that I transfer to his destroyer return to Boston so I could reach Lake Mohawk in time, for the wedding and return to Boston for the destroyer to return me to Conrad to continue the search. I was gone just over one search day.While I was gone, the ship party decided to lower a bottom dredge and see what they could obtain from a half hour of dredging. They recovered some envelopes of o-rings with Threshers number stamped on them which was certain proof that she had sunk nearby.

About this time we recovered a bottom picture that showed a gas tank, similar to a large oxygen tank, standing upright on the bottom. This was the type of tank that Thresher had used for storage of certain gases that they required. This picture was published in Time magazine which had a short report of the search each week. About this time too, the NRL ship cut the nylon rope from one of our navigation buoys and it started to drift off. We noted it moving from its station and radioed the NRL ship and asked if they had cut it off. They swore they had never come close to the buoy and they were not responsible. We had to recover our towed equipment and chase the buoy down. We recovered it and found a greenish grease on the nylon mooring line where it was severed. We cut a piece off the nylon line and saved it.

It was desirable to reset the buoy as nearly possible to the original location. Our anchoring method was to attach a scrapped automobile engine weighing about 300 pounds to a piece of chain about 30 feet long which was attached to a nylon rope about 5/8 inch in diameter. The Nylon rope was measured to be about 10 % less than the water depth, so that the buoy would keep the chain stretched off bottom so the Nylon line would not chafe off. By being stretched taut it provided the smallest possible surface wander. A piece of wire rope about 30 feet long attached to the keel of the doughnut buoy was attached to the upper end of the nylon rope to avoid chafing the nylon rope on the buoy structure. To set the rig we put the buoy in the water, stretched the nylon out in a straight line until we were towing the buoy about 8000 feet astern and we then steamed to the proper position as determined by the Loran coordinates at which we had set the original buoy. On reaching that point we lowered the anchor over the side and cut it loose. As the anchor sank, the friction of the nylon rope pulled through the water towed the buoy into position. Whom it settled down in position, we put over our dory and installed the fourth radar transponder in place. We soon received radio messages from the other ships congratulating us on resetting the buoy so close to the original location. Then it was bock to work on the search.

From time to time representatives from each ship were called together to confer on plan changes, and compare notes on their search techniques. At the next one, Chuck wrapped the piece of Nylon line we had saved and gave it as a gift to the NRL man. He did not open it until he had returned to his ship and we soon received a radio message stating that they admitted that they had severed the buoy line after all. The grease was the same as that on their wire rope.

At the end of the fifth week, we were replacing a radar transponder on one of the buoys when a large wave moved the dory away from the buoy just as I was carrying the transponder aboard and I grabbed the stanchion on the buoy and in the process dropped the transponder overboard. Fortunately by that time we had an extra and we were able to replace it.

As we were returning to the ship, as we approached ,the scientist who had developed the film shouted that we had two pictures other than bottom and they looked like wreckage. I came on board and looked at them and agreed they looked probable. One showed a rectangular piece that was slightly out of focus, and the next frame showed what looked like a thick piece of plating folded back at about 90 degrees. By that time we were headed into Boston for our end of the week rest. I radioed to ONR and told them that I thought we had two pictures. Would they meet us at the dock. About an hour later I received a radio message asking if we were sure of the pictures. I replied that we were quite sure. Just before we reached the pier in Boston, Chuck came to me and convinced me that the out of focus one was our own trigger weight which had somehow lodged in the framework of our camera so just the weight was visible in the picture. Being so close to the camera it appeared quite large.

When we docked, Captain Andrews came on board and I told him the awful truth. We showed him both pictures and assured him that we thought the other one was real. He said that there were reporters and others on the dock and we could not disappoint them so he gave me a brief case and told me to give him fifteen minutes to get back to his ship and then to come over the gangway with the briefcase where he would meet me. We would then go on board his ship. The news people of course showed me bringing a brief case to Captain Andrews and surmised we had some pictures. The experts could not identify the other picture and concluded that we did not have any picture of Thresher.

The Navy was having a Court of Inquiry about the Thresher loss in Portsmouth New Hampshire at that time. Captain Andrerws had me escorted to the court where I testified that we had not yet obtained pictures of the Thresher. On my return to Boston, I left for Lamont. I went in and told Doc the story and offered to resign since I had brought embarrassment to Lamont. He refused and told me to get back up to Boston and find the Thresher. I reported this to Captain Andrews and he concurred that I should go back to sea. At the meeting of the scientists just before we sailed he stated that as an incentive he would give a case of scotch to the ship that found Thresher.

We returned to the area and returned to work. We had been assigned a neighboring area to that we had started with after about three weeks, as had all the other ships working with us. After a few days we were on a track and decided to come around to port to reverse the adjoining track. Because of our deep tow and the strong current we couldn't make the turn. Finally we decided we should make the turn to the starboard instead. As we came around onto the new Track we got a magnetometer signal just like the one Jim Had posted near the magnetometer recorder. We quickly noted the coordinates and reported to Captain Andrews. At the end of the week, while we were on our way to Boston we received a request that each ship send their top scientists to a meeting at ONR. At the meeting we displayed our previously calculated signal, and the recorded one from the search area. It was generally agreed that this was conclusive evidence by the committee except the NRL group. They claimed they had recorded a magnetic signal earlier which noone would accept as definite. When asked to show it the committee could not see any likelihood that it could be a ship signature.It was concluded that we had found the Thresher and that the Archimed, which was under contract to ONR, would next be sent down to have a look.

We all returned to the area and the Archimed made a couple of dives, without seeing the wreck. They were having trouble maneuvering near bottom because of the currents. Before one dive, Captain Andrews radioed us and asked if I would like to go down on one dive. I replied, I would go if I could provide some service, but as just a tourist I would decline. It was dropped. At the end of that week. Captain Andrews decided that all the survey ships were no longer needed and that the Archimed would continue to dive So we were all sent back to our normal activities after turning over all the records, pictures, charted tracks and other data to Captain Andrews. We of course left the taught moored buoys with the radar repeaters in place for the Navy to locate the Archimed dives.

What they ever found later with Archimed, I don't know. We were never informed. ONR never replaced the money that we had spent in the search claiming that we had spent about the same money as we would have spent in our regular work with Conrad and they would continue their support of our regular work. Swede Momsen never produced the "free" contract either. Captain Andrews did not provide a case of scotch either. A couple of years later, after he was retired from the Navy he had accepted a job at Catholic University instructing underwater sound. He asked me to come and give a lecture on Sofar to a seminar. He paid my travel expenses and gave me an honorarium of $100. Perhaps he considered that fulfillment of his pledge. I don't know. We never discussed it.

About six months later, when I was on sabbatical in England, I was called to the U.S.Embassy and was given a citation for Meritorious Public Service by the Navy. A U.S.Navy Captain presented the citation and a lapel pin indicating the distinction to me on the Embassy steps with my fancily with me.

I later heard that the court of Inquiry had concluded that they could not determine the cause of the loss, but it was most probably due to some new welds that had been made on the recent overhaul which leaked salt water. The salt water sprayed onto an electrical swichboard. This in turn caused the nuclear reactor to shut down. A safety feature on the reactor prevented it being returned on line for a period of fifteen minutes. So the submarine was taking on water, getting heavier and could not speed up so that their dive planes could take them back to the surface. The ship would implode from the pressure.

Another time I heard that the Navy was making plans to recover Thresher to make certain what had caused it to sink but was forestalled by Navy Families. Many prominent naval officers had relatives in the crew, and they did not want their families to have the wounds of their loss reopened again. The plan to recover the wreck was dropped.

A number of years later I received a call from the Navy History office asking if I knew where the Thresher data was stored. I told them that I had heard it was stored at the Washington Navy Yard, but that I did not have any personal knowledge of whether that was true. I never heard whether the data was found again.

In rethinking over the whole Thresher episode, I conclude that the Oceanographic community bears some blame for the loss. When we show topographic sections we show them in 10 to 1 vertical exaggeration. This makes small slopes appear as nearly vertical walls. For example, the continental slope near New York has a slope of about 3 degrees. With 10 to 1 exaggeration it appears nearly vertical . We keep this exaggeration in mind in our own thinking. It is doubtful that it is so clear to our Navy friends in their thinking.

There was no reason to send Thresher into 8000 foot water depths for her first trial dives after an extensive overhaul. With a test depth of the order of 1000 feet, she could have made her first test dives after overhaul in water depths of 1200 feet or so. Had she done so, she provably could have settled on bottom when she started taking on water until they could have restarted the reactor and taken various steps to recover themselves, or at least could have waited until help was sent. In deep water they could only agonize as they approached imploding depth.

During the winter before the loss of Thresher, I had decided to take a sabbatical leave. I had been a professor for thirteen years and had been offered a sabbatical leave each year after the sixth year. I had refused because I thought the Observatory needed every able bodied person to make it succeed. At this point I felt that its success was assured. I had applied and received a Guggenheim fellowship that would supplement my half-salary paid on sabbatical leave. The support of Dr. Bucher and Dr. EWing was, I believe, pivotal in receiving the fellowship. I had written Teddy (Sir Edward) Bollard and was welcomed to the Department of Geodesy and Geophysics at Cambridge University for the year.

My family was a problem for this venture. Sandy had completed three years at Cornell, and Howard had completed one year at Rochester Institute of Technology studying photography. Richard was in high school, and Bill was in grammar school. Photography had been Howard's great love since spending two summers helping in the photo lab at Lamont, and a year as photographic technician on board Vema. I decided that Howard could ill afford to break up his schooling so early in his career, but that Sandy was far enough advanced to take year off to be with us in Cambridge.

Accordingly Sandy, Richard, Bill. Dorothy and I left for Cambridge England towards the end of September. Howard remained at the Institute living in the dormitory. I had rented my house at Lamont to a vising scientist for the year. I had arranged to rent a house on Alfa Road in Cambridge, for the year, from a Cambridge Professor who was attending his sabbatical away. We arrived in Cambridge late in the day so we went out to dinner at a local restaurant. When I ordered gammon steak, without thinking, I asked for it rare. Of course you don't eat ham steak rare, and

I knew what it was from the various trips to Europe that I had made in previous years. When the waiter corrected me, it became a family joke that I have suffered through in the succeeding family years.

Since I was expecting to do a lot of writing in Cambridge, I hired Sandy as a typist to type for me. Sir Edward offered to help with her salary, but I declined saying I had enough in my fellowship to pay her salary. At first she was offended at how low her salary was. I had set it to be just below that of, the Department Secretary so that I would not cause local difficulties. Sandy protested until she compared notes with the Department Secretary, and also found that it was a standard salary locally for such services. Of course she lived with us and paid no rent, so she was well off, locally speaking. Teddy put me in a vacant room in the Department telling me that someone from Scripps would be joining me in that room soon. He too would be on sabbatical. He told me that Sir Harold Jeffries would soon contact me to have dinner at his college and to become a "Sometime Fellow". That was the name they gave to people visiting for a year or so that would be welcomed as a guest for that time. In due course Sir Harold contacted me and had me to join him at his college for dinner and introductions. I was invited to attend once a week for dinner and after dinner wine. I attended religiously although Sir Harold never attended with me again. Apparently this was a courtesy that Teddy arranged for each long time visitor.

I found the dinners interesting, although the food wasn't the greatest. The fellows sat on a raised platform at the front of the room, while the students sat in three long rows made up of five or six ten foot tables placed end to end. The students, about 100 in number, all sat on benches, while the Fellows sat on individual chairs. There were about ten Fellows including me. The servers brought large platters and bowls of food which were placed at the head of our table each one serving himself and passing the items along to the next fellow. the fellows were placed in the order of their seniority. I was last to be served and sometimes there would be nothing left on the serving plate or bowl. In that case I received nothing of that item. I soon also found there would be no seconds. Some of the Fellows ahead of me really heaped up their plates with large servings, which is why sometimes I got none of one item. Dessert was served in the same way.

All of the students and Fellows, except me, were attired in Academic Robes for the meal. Some of them looked quite ratty. I suspect they had been used for several generations of students.. The students food arrived a little later than that of the fellows and everyone started eating as soon as they were served. When the students were finished their meal, they left immediately, so the dining hall thinned out quite quickly, but there were patches of students left here and there. When the Fellows were finished, we went into another room, which was quite splendidly furnished and were served after dinner wines. These were served in carafes mounted on wheeled carts. There were three carts attached to each other with a different wine in each. These were passed around the table three times. Each one took one of the wines each time it passed if he wanted. Meanwhile a general conversation was held, usually about college affairs and students. Sometimes the subject was on news events or recent findings. Most times I just listened as I was not involved in much of the conversation. Each Fellow was in a different field of study, so there was little talk on studies, other than discussions of outstanding, or poor students. Monthly I received a bill for my food and drink.

Cambridge is organized quite differently from our Universities. Courses in various fields, e.g. geophysics, geology, chemistry etc. are held as lecture sessions in university buildings, which incidentally are not heated. Temperatures would get down in winter to just at or below freezing at night, and would rise to the 50's most days. The students were enrolled in colleges, about ten in number. Each college would have about 100 students about 10 tutors. The colleges were named for their sponsors mostly of many years ago , such as King's, Queens, Emmanuel etc. The only recent one is Churchill which specializes in the sciences. Each college usually spread around the spectrum of disciplines. The fellows of the college acted as tutors for a group of students studying in his area of discipline in that college..

The educational system was quite different from ours. Students attended lectures on specific subjects at the central university. They could attend or not as they saw fit. Each student was assigned to a fellow. Each Fellow had meetings with each of ten students. In this capacity they were called tutors. They would meet with each student for a few hours once a week. At these meetings they would quiz the students on various areas of their studies. Based on these quizzes they would recommend additional reading or exercises for the student to help fill in the gaps in knowledge, or to extend their work in one or more subjects. At the end of the academic year, those students who were ready, in the opinion of their tutor, took an overall exam in the area of their study. E.g. geology, physics, biology or whatever. The exam lasted a couple of days. On the basis of that exam the student was flunked, passed with distinction or passed with extraordinary distinction.

I usually arrived at the Department, from the house we were renting, by bicycle just after the maintenance man unlocked the doors (about 8 o'clock). The rest of the students and staff would usually arrive within the hour. At 10 All the students and staff would meet in the common room for coffee (instant) and cookies. This usually involved a discussion of topics of the day, or some work that was going on. After about half an hour, everyone would return to their desks and work. I usually bicycled home for lunch with Dorothy. The boys and Sandy usually had a bag lunch. Often the students would kick a soccer ball around after their bag lunch. Sometimes I would return and join them. Although I had played soccer about eight years in grammar school, I was well below their standard of play. In the afternoon we would meet in the common room for tea and small sandwiches, about 3:30. Again conversation was the order of the day. After tea we would all go back to work. People would start to leave for home about 5:30. I usually left about 6.

I had ordered a new Hillman Minx station wagon to be delivered to me in England while I was still in N.Y. Up to that time, I had always bought used cars. I inquired and was told that there were no usable used cars in England, because everyone kept their cars until they would not run anymore, before buying another. When I ordered my car I was asked what color I wanted. I replied I didn't care. When I told Dorothy she said it was all right as long as it wasn't red. Naturally it turned out to be red. I took delivery of the car in London on my arrival. Since I expected to ship the car home on our return it had been delivered with a left hand drive, as we used in the U.S. We packed all our luggage in the car in the center of London, and I had to drive to Cambridge, about 50 miles away. Of course, I was driving on the wrong side since the English drive on the left side of the road. It was quite an experience driving for the first time on the "wrong" side of the road in London Traffic. We survived, although i had trouble at roundabouts (traffic circles) as for awhile I wanted to go around to the right, while all the traffic was going around to the left. I soon found another trouble too. They did not post traffic signs until you reached a corner instead of sign in anticipation of that corner, that I was used to. Several times I became a traffic road block while trying to decide what direction to go at an intersection.

Bob Fisher was the second occupant of the room in the Department. He and his wife and son arrived by air. They too had ordered a car for delivery in England. His was not ready yet when he arrived. He asked me if I would drive him to an American Air Base, where his baggage would arrive so he could bring it to Cambridge. I agreed and we took off. On the Air Base, I reverted to the right side of the road. Shortly a large truck appeared directly in front of me. We both stopped, and the Sergeant driving sat there with a large grin on his face until I discovered I should be on the left side, and backed up so that I could get onto the proper side.

Bob was working mostly with charting soundings, so he had a large drafting board in our office, with many rolls of charts on his desk, which he had to unroll and attach to the drafting board frequently as he followed a ship's track in his plotting. I was working, mostly, on my book of Pendulum Observations at Sea on Submarines, so I was moistly at my desk. We talked for short spells from time to time but mostly we paid attention to our own work.

One time, Maurice Hill, a geophysicist whose age and background was as close to mine as anyone in the Department, invited me to sit in on one of his tutorial sessions. I was impressed.

Another time he asked me to give a lecture to the department about the work at Lamont. I had thought that it might come to that, so I had brought slides with me. After the lecture, to my surprise a number of students came up to me and told me that they had thought most of the techniques we were using had been introduced in England recently , but that we had been using them for a number of years. I pointed out that most of our work had been reported in the Transactions of the American Geophysical Union which was contained in the Department Library. They replied that it had become so enlarged with papers that they called it the Yellow Peril (its covers were colored yellow). I would have thought their tutorial system would have had them reading these publications, but it was apparently not so. Their professors were presenting the techniques to the students without informing them that they had mostly been developed in the U.S.A. hence the students had the impression that they had been developed locally.

In the fall Teddy Bullard arranged for Bob and I to be invited to a meeting in London where all of the universities presented the research projects that they were following. The meeting was in the evening and we had to wear Tuxedos. Here again we saw a lot of research being pursued without any reference to where the technique originated which gave the impression that it had been developed locally. For example. Tony Laughton showed his underwater camera and underwater pictures without any hint that the camera was an almost exact camera to that he had used in his year of training at Lamont, and its use and observations were exactly as he had learned them on Vema.

The year before my arrival in Cambridge they had, uncharacteristically, a week of below freezing weather. Because the water pipes were only buried just beneath the surface, the y froze, and the department had no water for about ten days. The staff had to walk about a quarter mile to the neighboring Astronomy Department for their needs. The Astronomy Department had been built more recently and its water pipes were buried somewhat deeper.

In the house I rented, I noticed all the piping went through the walls and most of it was run outside. When I inquired about this, I was told that it was necessary so that one could make essential repairs.The walls were brick plastered directly on the bricks on the inside. As a result the walls generally had condensation and seepage making the walls damp most of the time. There was no insulation in the houses. I noticed in the new construction that was still true. That was strange since the British had done most of the research work about insulation.

Another interesting plumbing feature, was that the bathtub drained through the wall to a downspout which in turned drained into the street gutter. I suppose this was so the neighbors could tell that you bathed regularly!

On weekends I usually washed my car. At first, I did this from a bucket of water as there was no external water spigot. I soon arranged to adapt a hose to one of the bathroom fixtures, running the hose out the window to the street, where I could wash my car. A neighbor immediately accosted me and told me that what I was doing was illegal, as there was a water shortage. It seemed one could use a bucket of water for such a purpose. I reverted to the inconvenience and the bucket. It seems that Cambridge had a perennial water shortage.

Since most points in England were within about 500 miles of Cambridge, we started taking a drive each weekend to see the countryside. We soon found that we could cover only about 300 miles per day maximum in contrast with the 600 miles per day we could cover in the states.

On one of our weekend trips we decided that we could smell gasoline. This was dangerous as Dorothy still smoked at that time. I took the car to the local dealer. He kept it for a day and reported that it was fixed. The following weekend we could again smell the gas. I left it with the dealer again for a day. On the fourth occasion of smelling gas he said that they could not find anything wrong and that they would have to replace the gas tank. I was then told it would take two months to get a replacement tank. In disgust, the following weekend, I decided to check it out for myself. In the back there was a plate screwed down which covered the fill spout. I removed this and discovered the fill pipe was not continuous. One part was welded to the frame, while the other part was attached to the tank. A piece of hose was used to connect the two which had hose clamps to keep the joints tight. These clamps had never been tightened. The gas sloshing around when we drove was finding its way out of these joints, hence the smell in the vehicle. When I tightened these clamps there was no more problem . So much for the vaunted excellence of British Mechanics.

There was a camping store in Cambridge where I could buy used camping equipment . Hence we outfitted ourselves for camping.The camp sites in Britain were pretty undeveloped. Usually those listed in our camp guide were just a portion of a farm pasture. A lot of road straightening had been done, which left lots of oxbows near curves which we soon found out made good camping sites. Some camp sites, especially near beaches owned resident trailers (locally called caravans) which they would rent out by the day, week or month. Sometimes we would stay in one of these instead of camping.

Richard wanted to join the "magic" club where members learned various sleight of hand tricks and practiced on each other. These were held at night so we wouldn't let Richard go alone, so he convinced Sandy to go with him. Soon Adrian Browne started to show up at our house, an acquaintance from the club. Soon, he and Sandy started dating. He was a student in his next to last year as a student in Emmanuel (called Emma) college. One weekend we went camping on the beach near Wales and Adrian went with us. In the morning, we rose and had breakfast shortly after dawn as we usually did back home. As we were finishing, Adrian appeared, bleary eyed, and asked what everyone was doing up in the middle of the night. He soon learned to change his ways.

In the spring break the boys had from their schools, we drove north into Scotland. Most of the roads were one lane. When you met another car heading in the other direction, one of you had to back up to a turnout, to let the other one by. We visited the Devils Post Pile along the north shore, and several Lochs including Loch Lomond. We then took a ferry to Ireland and spent a few days touring there. While there Richard kissed the

Blarney Stone. To do that you had to lean over the edge of the roof while the guide held you by the belt so you could reach it. I guess it worked, because Richard now earns his living giving lectures. None of the rest of us wanted to lean out over a 100 foot drop to kiss the Blarney stone.

On another occasion Howard appeared at our door as we were just leaving for the beach off Anglia. We included him in our camping trip and soon learned that he had arrived in England on a cargo ship, passage free, that had been arranged for him by Ed Newhouse. He stayed a week, and returned to the States on the same ship.This must have been on his spring break. Needless to say, we were surprised to see him, but very pleased.

Shortly afterwards I received a letter from Dr. Ewing that Lamont had agreed to use a union crew on Conrad. Apparently Newhouse had arranged with the unions for Howard's trip. He found the trip boring, since they would not let him do any work, because he was not a union member, and there was noone else on board who wasn't busy. I wrote Dr. Ewing that I would not go to sea on Conrad so long as they had a union crew. I would be glad to do everything possible to get Conrad equipped to go to sea. If that was not satisfactory that I would have to leave Lamont. He replied that would be no problem. Lamont was immediately billed by the Union for the pensions for each crew member. Later one of the crew became sick and sought medical help. After more than a yea trying to recover, the doctor advised he could never again go to sea. He applied for his union pension. It was denied him, although he had been a union man for many years, because he had not been to sea in the previous year. Such is the way of unions.

Cambridge University had a library that had functioned for several centuries. I decided that while I was there I would look up the early origins of geophysical papers. Before 1900 there was only a very occasional paper that would be so classed. Afterwards, one or two papers began to appear. None of these had any references given. If some expression was needed, it was given as thought the author had just invented it, even though it had appeared previously in some one's publication. In this period, most of the people working in the field knew each other, and usually corresponded frequently. About 1920 one or two references started to appear on papers. Soon numerous references were given by each paper. Now of course it is rare for a paper not to have many references. Most authors today only know a few of the people whose references he gives.

Shortly after I arrived in Cambridge, I was notified by ONR that I was to receive a citation about the Thresher search and asked me to come to the U.S.Embassy in London. On the appointed day my family traveled to the Embassy and on the steps, in the rare (for London) sunshine, a U.S.Navy Captain presented me with a Meritorious Service Citation. We celebrated with a roast beef dinner at a famous London restaurant.

Shortly before I left Lamont I had agreed to get various geophysicists to write a book suitable for knowledgeable laymen about their field of expertise, for them to publish. I had contacted a number of people, and had agreement from a few to give it a try. I decided that I would write about gravity , after supper while I was in Cambridge, and decided to call it "The Structure of the World by G". I wrote several chapters in the fall and then found my hands getting too cold as the season progressed. I dropped the effort, and I found the others had too.

There was no heat in the house except a fire in the kitchen to heat hot water, which without insulation on anything kept the kitchen acceptable. There was a small electric heater in the bathroom that kept that room warm. Dorothy said that during the day she would stay in the kitchen until the last possible minute, and then make a mad dash for the bathroom. About six she would build a fire in the fireplace in the living room so there would be some heat when Sandy and I got home. We all learned to adopt the heavy clothing the Brits all wore. The Fireplace had been as large as we normally had in the States, but sometime, a few years ago, it had been reduced in size so it was only about 18 inches long and high. Small sticks, high priced, could be bought to burn and also soft coal.This fire could keep you toasty in front while your back felt quite chilly.We soon learned to turn in early.

At Christmas time, Maurice Hill arranged to get us an invitation to the Christmas choir in King's Chapel. This was a choir of about 100 boys that could sing like angels, before their voices changed. The acoustics in the chapel enhanced the singing. It was a most enjoyable occasion.

In early January I received a notice from customs that they had a package for me. I paid the duty and finally received the package. It was six sterling silver goblets for after dinner liqueurs from the Staten Island Shipyard where I had supervised the overhauls of Vema for several years. I have no doubt that I paid more duty than the shipyard paid for the goblets.

Early in 1963 George Woollard came by Cambridge. He came by to see me and asked me if I would be interested in moving to Hawaii to initiate an Ovceanographic Geophysics program. After thinking it over I told him no, that I thought Ewing and I could get more good geophysics done together than separately. Ewing later told me that he was very pleased with my decision.

Later in the year Stanley Salmon, the assistant to President Kirk of Columbia University made a special trip to Cambridge to see me. He asked me if I could come back about a month early, about September 1st, since Dr. Ewing wanted to go on a sabbatical leave. I agreed and he told me that I would be called the acting director for that year.

This meant I had to change my plans, as I had planned to tour the continent in August, the month when most Europeans were also going on vacation. I decided that I could leave for a tour of the continent about the middle of July, returning to Cambridge about mid August. That would leave two weeks for us to get ready to return to New York.

A few weeks later, Sandy told us that she did not want to return with us that she and Adrian wanted to get married. We argued with her pointing out she had one more year to finish college and marriage could wait. She was determined and insisted that she would have a mental breakdown if she and Adrian were separated that long. We finally gave in. We decided to give them a wedding gift of money which they could use for their wedding with any left over that could be used any way they wanted.

Sandy arranged for a church wedding and a reception at a Cambridge pub. There was one awful part to it. I would have to wear morning clothes, including a gray top hat, as the British did in their weddings. The father of the groom would also be dressed that way. I even managed that. After the wedding, a photographer was taking wedding pictures. He wanted one with the wedding party and the parents of the groom. Adrian's father gave me his umbrella to hold while the picture was taken. With the umbrella that I had, I stood on the lawn of the church with an umbrella in each hand propped against the lawn behind me. The photographer snapped an unposed picture of me. Katy Nafe when she saw it said I looked like Jiminy Crickett.

After the wedding, Sandy and Adrian drove to London where they took a plane to Germany where they had arranged to obtain a car to take a camping trip through the Middle East for their honeymoon. They did not return before we left for home.

In mid-July we took the ferry at Dover for France, with our camping equipment. Our first stop was Paris, The camp was on the outskirts and was quite full, but we found a place. We toured the sights of Paris for several days. We soon found that we could not spend too long a time in museums because Bill became too restive. From Paris we went to the Mediterranean coast. The boys had a great time body surfing in the waves. Then we went on to Italy to see Rome and Venice. Then we went on through the Austrian alps into Germany, where we stopped and visited with Dr. Graf and his family. They had us to their home. Dr Graf spoke passable English, but my German wasn't up to the task. Dr Graf's wife and daughter could not speak English, and Dorothy, Bill and Richard could not speak German. Despite that we had a great visit and the two families got on famously.

On the way north in Germany,on an autobahn, a fan belt broke on our car and I had to pull over to the side of the road. On the advice of the motor club in England I had obtained a kit of spare parts, since they told me it might take a month to get a part sent to the continent. There was a fan belt in the kit, and I had to change the fan belt with cars whizzing past my fanny at speeds up to 90 miles per hour. Needless to say, I was glad when it was done.

From Germany we went into Holland and Denmark via Switzerland. stopping along the way to take in the sights. Dorothy loved the pastries that we could get in Denmark. We took the ferry to Sweden . After a couple of days in the southern part, we drove to Norway. That was a beautiful country only surpassed by Switzerland. After a few days we returned on the ferry to Denmark, then back to Holland and France where we got the ferry back to England. When we landed there we found their papers headlining the terrible heat wave they were experiencing. It was all the way up to 85 F. We quickly packed up and shipped most of our things home, leaving the camping equipment for Sandy and Adrian to dispose of. We drove the car to London where we found a bed and breakfast near the airport. I drove the car to the agency that was shipping it back to New York, for me, and left it. The next morning we took the Underground (subway) to the airport and caught the plane to New York. We decided on our trip to the continent that Europe was the place to go to admire man made things, while the Western U.S. was the place to go to admire Nature's things. We preferred the latter.

On my return to Lamont, I found that Manik Talwani and Denny Hayes had solved the cross coupling correction. They had made a small computer that was included with the gravimeter. They also added an accelerometer which measured the accelerations perpendicular to the boom of the Graf gravimeter. With this, the computer calculated the cross coupling correction, which was in turn added to the gravimeter output. I had intended to concentrate on this problem on my return, but this made that unnecessary.

I had asked Chuck Drake to follow the expenditures on all Lamont contracts, while I was gone, as various of our scientists were not too careful and would overspend their funds. Since now most of the work was under a single contract, this would mean someone else would have to spend less than they had budgeted. This would cause internal strife. I had been doing this before I left and calling people and getting those who were overspending to cut back. I found Chuck had not done this and we were overspent by about one million dollars. I immediately called all investigators and made them stop all spending except for salaries. This was, of course, unpopular. By the end of the year we had made up the over expenditure. It was just in time too, because when our contract was up for renewal, the Navy decided that extending the contract had gone far enough, and they would start a new contract for the renewal that year. Had they done this a year earlier, they would have found that we were overspent by the one million dollars. It would have caused serious consequences for Lamont, and Columbia. As it was, the Navy never knew.

Maurice Hill had become Senior Editor of The Seas. This was to be a two volume work on all the work done at sea in the past decade or so. He recruited several assistant editors, and I became one of them. This required contacting people to write up their work and keeping after them until their manuscript was accepted. This was not too hard in my case, as all the people in my area were anxious to have their work included. When the books appeared, the editors received a small fee. The few hundred dollars I received the next five or so years was welcome.

I had contacted John Wiley & Sons to print the volume I had finished while I was in England. It was entitled "Pendulum Gravity Measurements at Sea 1936-1959".I spent a lot of time on the details of getting this volume printed. I also received a few hundred dollars in royalties from this over the next few years.

Columbia was planning a celebration of its 200 year existence, from its start, then known as "King's College". As part of this they wanted to close 116 street that crossed the middle of the campus. Although I don't know the machinations, they had to get the agreement of Robert Moses who held some position in the government of the City of New York. He refused to give the necessary permission unless the five or six acres of Lamont property on the New Jersey side of the state line was deeded to the Palisades Interstate Parkway. I objected, but this trade was made anyway, and Columbia closed 116th Street and made it part of their main campus.

In that year also, one of the Rockefellers complained that our Oceanography building was too visible from the train, across the Hudson River, on which he commuted to New York. Columbia insisted that we would have to paint the building so that it would blend in better with the landscape. I took a trip across the river, and walked the train tracks to see what the problem was. In fact the building could only be seen for a short distance from the train tracks. Despite this observation, we were required to comply with his request (demand?). When I inquired what color we should make the building, I was referred to the Palisades Inter State Park Commission, on whose board of directors Rockefeller served. When I called the Park Commission they said they would have to study it and would let me know about the color. About two weeks later they called and told me we should paint the building sand brown. When I told them that was the color of the bricks already, they said then we should have it painted dark green. We did that. I still feel that was an expenditure that we should not have had to make.

Anschutz had taken over the Graf Ses Gravimeter to market. They had reengineered it slightly to make the heat controlled space more uniform. We were able to get one for each of our ships. One of them did not work properly and they sent an engineer to Lamont to find the trouble and fix it. He decided that the trouble was in the power supply, but after a couple of days work, had not found what was wrong. In discussing it with him I suggested that a particular resistor must have failed. He insisted that German resistors were made so well, that this could not be the trouble. After he left that evening, I replaced the resistor with one of ours and the trouble disappeared. The next morning, when he arrived, I told him what I had done and that it was fixed. He would not believe me and spent the whole day checking it out. At the end of the day he came to me and admitted that my repair had fixed the trouble.

In the early 1960''s the Mohole Project had been proposed by a group of scientists, mostly from Scripps, I believe. We did not believe in it, because it would be too expensive for too little gain of scientific information. Nevertheless, the project went ahead and congress appropriated money for a start. The project grew, and the budget grew, with no hole drilled, so large that congress cut it off as too expensive. In 1963 WHOI, Scripps, Lamont and the University of Miami had formed JOIDES. This stood for Joint Oceanographic Deep Earth Sampling. The idea was to join together to drill the sediments in the oceans, a more modest and less expensive project than the Mohole. We soon found the Caldrill , a drill ship, was transiting from the west coast to the east coast. We made an arrangement for it to stop for a month on the Blake plateau to drill several holes for Joides. NSF furnished the funds . Lamont was chosen as the operating agency, and I was selected as chief scientist. I could not spend much time on the drill ship, and Sam Gerard spent the whole time on board while the drilling was taking place. Six holes were drilled in water depths from 25 meters to about 1000 meters. I went on board when they were drilling hole #2 in water depth of 45 meters. The drilling had intercepted an aquifer with artesian water. Our drill pipe 30 feet above the ship's deck,, which was about twenty flowed above the water line, flowed fresh water which was drinkable. I know, because I did. Since we were about 30 miles off the coast it meant there was a lot of usable water beneath the continental shelf. So far Florida has not availed themselves of this resource. The various holes bottomed in the Eocene to Paleocene strata at depths of up to 300 meters subbottom. A good picture of the strata beneath the Blake Plateau resulted. With this successful operation the Joides organization was off to a good start.

I was glad when September of 1964 arrived and Dr. Ewing took over the director's duties again, so I could spend more time on my research.

In early 1965 I had a party at my house. Guests included staff members and some of our nearby neighbors. One of the neighbors was a lawyer at Texaco. In talking with him I mentioned to him that we were being inundated by oil company representatives visiting and asking to see data we had in our files. It took a large amount of the time of our staff to find the data they wanted, since they could not find it for themselves. I was worried because most of the salaries of the staff were paid on government grants and contracts. He asked me why we didn't charge the oil companies for this service. I replied that I didn't know how to do that since when they came, they often were led to ask foe more and more, so that in effect one of our staff had to spend almost the whole day helping them. While we were pleased that they were interested in what we were doing, it was still a worry. He asked why we didn't invite companies, that had used our services, to pay an annual fee and have someone to get what they wanted for them.

The next day I brought this up with Dr. Ewing and we agreed to see what could be done. Soon after,r we talked to Creighton Burke, who worked for Mobil, and Hollis Hedberg, who worked for Gulf, both of them very good friends of ours, about it. They said that they did not see that there would be any problem with it. Thus our Industrial Associates was born. We sent a letter to each of the companies that had used our data extensively and invited them to join our Industrial Associates. We said we would charge a fee of $50,000 per Associate as we had an extensive collection of records. We would hire one person for their contact with us. Through that person, they could have a staff member visit and our staff member would find whatever data they wished to see, and make copies for them. If they did not wish to visit, they could call us and ask for the data of interest copied and sent to them. Twice a year we would have a meeting which we would invite them to, to tell them what new data we had accumulated, what projects we were operating, that they might be interested in and ask for their suggestions of how our operations might be improved.

As I remember it, Gulf was the first to join after about a three month wait while they made their decision. Soon after Mobil also joined, and pretty soon we had ten companies signed up. At our first meeting we had mostly Company presidents and Chief Geologists present. Some brought another company representative with them. We provided an overview of the areas of our research, in which we thought they would be interested, and our cruise plans for the next few months. In each discipline we had the person in charge to explain how we made our observations, what equipment we used and to explain how the data was recorded. At lunch time we all retired to a nearby restaurant with which we had arranged for a light meal to be served buffet style. Our staff and our visitors intermingled and great discussions often resulted. We called the meeting for 9 A.M. to give them time to arrive without undue strain, and we closed it at 4 P.M. so they could easily make their planes back to their offices. Some of them stayed for an hour or more for additional discussions, and a few stayed over the next day to obtain some data they were interested in. We had persuaded one of our graduate students who had just finished his degree, to take the Job as our representative. This gave us an additional post-doc who could do research when he was not occupied with IA work. He found the contacts he made very interesting and useful and the system prospered.

Our IA members kept growing as the word passed in the industrial world about it. It grew until we had more than twenty members which even included several foreign oil companies. Usually each member had two or three of their staff at each of our called meetings.

Soon after forming the IA. we started to record our gravity and magnetic date and course and speed observations on the same record as the sounding data, so that they could be readily correlated mentally. The I A members immediately spent some time with our staff learning how we arranged this as it would help them too.

About a year after jointing IA, Mobil decided to outfit a ship and asked for our help in advising them how to set up their ship efficiently. Gulf, too, decided to outfit a ship of their own and asked for our advice. Of course we helped them as much as we could.

Much later several of the oil companies consolidated by merging, or purchasing others and we had a problem. They would not consider a doubled fee, and we had to accept them as one company. We just had to accept this, as we could never think of a way to get around it.

After a few years, the other Oceanographic Institutions started their own Industrial Associates programs. Several of our associates told me that their programs were inferior to ours.

Soon after forming the Industrial Associates we started having problems with some of our neighbors. They complained about the much increased traffic from our enlarged staff using the entrance road through the village, and because they said some of our staff drove too fast. One neighbor complained about our testing equipment which made some noise. I decided to have an open house to which we could invite our neighbors on the belief that if they knew some of the exciting things we were learning, they would be more tolerant of our staff. This proved so successful that we made it an annual affair and it grew. Now they have about 2000 visitors at Lamont on open house day. The staff mostly has to hold their data demonstration on the lawns because the number of people have become too large to get into the rooms and the halls became jammed up. Nevertheless we had to open a new gate directly on 9w to relieve the traffic past the neighbor's homes.

Two major items remain in my memory about 1966. The first was the sixtieth birthday party that I organized for Dr., Ewing. The second was showing the accuracy of satellite navigation and adopting it for our research at sea.

For his sixtieth birthday I decided that Doc should have a birthday party. I invited all of his former students that I could reach, as well as Arnold Finck, our business manager, Angelo Ludas, our shop foreman, and Hank Skjering, our marine superintendent. Twenty two students could make it, some from Washington, at least one from Texas, one from Colorado, and one from Pittsburgh. Of course the bulk of them came from the New York area. I hired the Rockland County Country Club for the evening and we had drinks before dinner, a nice steak dinner and a good dessert. While the dishes were being cleared from the tables, and the various courses were being served, I called on various people to tell a story of their experiences as a student of Doc"s. I started with the earliest student, and worked up to the most recent. We heard many stories from those that had experienced them, Many of them stories often repeated in legend but this was the official version. Many, of course were humorous. At the end they served a birthday cake with one large candle on it. At that time Doc gave a rebuttal of the remarks that had proceeded, denying his embroidering the various versions of the stories we had heard over the years.

After dinner we retired to the lounge and were served drinks, and visited amongst ourselves. Doc circulated around and visited with each one. As people left, several told me that they would not have mused it for anything in the world. It was expensive, but well worth it.

In reviewing our progress in surface ship gravity measurements in the fall, I had decided that our navigation was now the biggest error in our gravity observations, and that we would have to find a better way. The Eotvos correction, which could amount to several tens of milligals (earth's gravity is about 980,000 milligals) was caused by the east-west component of the ship's speed. Increasing gravity foe eastward motion, and decreasing gravity for westward motion. This was caused by the increase or decrease of the earths rotational speed which amounted to a centrifugal acceleration

A paper appeared authored by a group of Johns Hopkins scientists about the geoid determined by satellites. Several papers on the subject had appeared earlier, but the ground truth, my measurements at sea did not agree with those early ones, while the Johns Hopkins one agreed. I called them up and suggested that I come to visit them so we could compare our measurements in more detail. They welcomed my visit. I had brought George Bryan with me bemuse he was interested in the subject. We had a great visit with them, and as we were leaving, one of them said you can navigate with this system too, you know. I immediately perked up my ears and we returned to discuss navigation by satellite with them. The system was called Transit.

The satellites were in polar orbit and circled the earth in about an hour and a half. This meant each pass was about 22.5 degrees of longitude east of the previous pass.There were five Transit satellites flying, positioned so that at least three were directly on a line of sight from any positron at one time. Signals were sent from the satellites at specific times and the Doppler (change of frequency between when the body was approaching you, or departing you) could be used to calculate a positron on the earth. The satellite was also programmed to send to the receiver all of the information needed to make the computations.

They had made tests on land and proved that the positions determined were better than a quarter of a mile in accuracy. They had been working on this system for the Bureau of Ordnance of the Navy. Neither they or the Navy could determine the accuracy ate sea, which was the desired objective. I got excited and assured them that we could determine the accuracy at sea and help both them and the Navy. I explained that Conrad was scheduled to make a grid in the Caribbean in about a month where we would be making gravity, magnetic, sounding, and bottom structure measurements on about five tracks North-South, and five tracks East-West. This would make 25 crossings of tracks where the gravity, magnetic and soundings could determine the discrepancies of the navigation between the tracks crossed. I then asked if we could get a SatNav receiver for the trial in a month. They said they could not let us have one, that all that had been built were controlled by the Special Projects of Bu Ord. I said we had worked with special projects getting surface ship gravity going and maybe I could persuade them that we could help them on this. If so we would have to install the SatNav in Jamaica where Conrad would be arriving in about three weeks. Would they be able to help us install the gear? They replied that if the Navy would let us have the gear they would provide a technician to help install it on the ship. They pointed out that the computer for the fixes wasn't yet working, that the data would have to be returned to the lab for computations. We said we could do better than that, if they would give us the program we could send the data by radio, make the computations at Lamont and return the fixes to them in about a day. They said that sounded good to them.

George and I left and went to the airport to get our plane back to New York. While we were waiting for the plane we were discussing this new idea. Finally I said that it did not make any sense for us to go to New York and then turn around to come back to Washington which was only 50 miles away. We should just go on to Washington. George said he couldn't as he had a date for the next day, so I decided to go to Washington alone. I caught the bus from the airport to Washington, going to my brother-in laws apartment, unannounced, where I was welcomed. Over dinner I told him of my mission. He was in charge of the Antarctic Program for the National Science Foundation. He told me that they were supposed to get the first

SatNav that was available, when the Navy released it, for their ship working off Antarctica. I pointed out that we could help the Navy get to the point of releasing it to them. He assured me, that I was just dreaming to think I could get the Navy to provide us a unit with only three weeks until Conrad would get to Jamaica.

The next morning, I made it a point to be at ONR when they opened. I explained the situation to Art Maxwell and Feenan Jennings. Art then told Feenan to take me to Special Projects in Bu Ord to see what we could arrange. At Special Projects we talked to the project officer. I pointed out that the Navy would all be in port for Christmas starting in early December, while our ship would be at sea continuing operations. That we could make the necessary observations over the Christmas holiday and return the Sat Nav to the Navy in early January. He considered the situation and decided that we would have to talk to his superior the Chief Scientist of Special Projects. Once again I explained the situation. After asking the project officer his opinion, which was that it looked like a reasonable chance to obtain results they did not know how to get. The Chief Scientist said that he concurred, but that we would have to talk to Admiral Smith, C.O. of Special Projects. Again I had to explain to Admiral Smith that Conrad would be in Jamaica about 20 November for a three day port visit, and that we could, with Johns Hopkins aid, install a Sat Nav there. The ship would sail on 24 November make the necessary observations and reach Panama on 24 Dec. for a three day refuel and revictual, where we could remove the Sat Nav and get it back to the Navy in time for their sailing early in the New Year. With some discussion with his staff who had accompanied us to see the Admiral, he decided that he would give his authorization We returned to ONR where I thanked Art and Feenan for their help and I returned to Lamont.

On arrival at Lamont I sent a radio message to Conrad indicating the planned experiment. Doc was Chief Scientist on board. I explained that we would probably be able to show that SatNav was good to about one quarter mile accuracy on approximately a three hour schedule, compared to Sextant accuracy of about three miles at the time of morning, noon and evening sights, with deteriorating accuracy in between. I asked Doc to have the captain make arrangements to have assistance of a welder for the masthead assembly for the installation. I told him that I would ship the navigator to Jamaica and make sure it arrived before the ship got in, and that a Johns Hopkins technician would be on the dock too, to help make the installation.

I called Johns Hopkins and told them of our arrangements and that we would need their technician to be on the dock in Jamaica on 20 November, when the ship arrived. They had already been advised by Special Projects to arrange to remove a set of gear from a Navy Ship and package it for shipment to Jamaica. Manik Talwani was relieving Doc as Chief Scientist for the next legs of the Conrad cruise. He was excited about the new test as it promised better results on the planned survey in the Caribbean. The technician from Johns Hopkins accompanied the equipment to Jamaica thus assuring that nothing became lost.

All went like clock work. Conrad arrived in port on time, Manik was waiting on the pier with the technician from Johns Hopkins and the equipment. The arrangement for the welder had been made with the ship's agent by radio from sea. The equipment was installed and the antenna arrangements made in just two days rather than the three days we had expected. Doc saw the ship off before he returned to Lamont.

Manik started sending us daily Sat Nav data for computation soon after the ship sailed. Jim Dorman and Manik Talwani had installed the program for the reduction of satellite data in our computers, before Manik had left for Jamaica. Jim Dorman took the data as soon as it arrived from the ship and made the computations to return the fixes to the ship. That part did not work out, because there were too many errors in digits of the several thousand sent from the ship for each fix, that the computations did not work out.

When the ship was nearing Panama, I went to Special Projects to see Admiral Smith. I told him of the trouble that we had had with the radio transmissions, and that we were planning to take the original data as soon as Manik returned from the ship and make the computations with good data. We estimated that this would take about two weeks. I requested that we keep the Sat Nav on the ship for the next leg so that we could make additional observations after we saw the results from our data analysis. He allowed that we could keep the equipment for a couple of more legs.

When we reduced the data, not only for the Sat Nav, but also for the gravity, magnetic, and soundings we had a good case that the Sat Nav data was accurate to at least 0,2 mi. As soon as we had these results I called Johns Hopkins and told them and I went to see Admiral Smith to show him all of the data and the resulting grid of ship crossings, as well as our analysis of the accuracy. He was very pleased and called in the Chief Scientist and the Project Officer and asked me to repeat what I had showed him. When everyone was clued in. I begged Admiral Smith to let us keep the

Sat Nav for the rest of the cruise. To my surprise he told me he had anticipated my request, not really expecting to get the Sat Nav back in January, even as he was granting the first request. I pointed out that the improved gravity data would make it worth while. He granted his permission and I went back to Lamont quite happy.

At the spring meeting of the American Geophysical Union I arranged for a Johns Hopkins scientist to explain how the Transit system of navigation worked, and Manik Talwani, Jim Dorman, George Bryan and I gave a paper delineating the results of the grid of observations we had made with it, demonstrating an accuracy of at least 0.2 mi..

Johns Hopkins meanwhile had been working on a special purpose computer to reduce satellite data on board ship. They had a couple of working models by late summer. I went to Admiral Smith and persuaded him to allow us to have one of them for Conrad. We were able to install it in Conrad at a port stop in Europe. Doc again was going as chief scientist for a planned detailed grid in the vicinity of the Peake and Freen deep, a part of the Mid-Atlantic ridge.

When Doc returned to Lamont he could not have been happier. The Sat Nav data could be computed with the new computer within about a half hour after the data from the satellite was received. This made the grid survey not only more accurate, but it made it possible to adjust the track while still in the area. He also had been able to return and find a feature, that turned up in the survey, as the data was plotted on board, so that he could take a core and obtain bottom pictures before leaving the area. We had never been able to do that before. Our attempts usually resulted in losing at least a half a day of ships's time trying to relocate the desired site, and usually much more.

A new day had dawned in sea research!

With that data in hand, I went to see Admiral Smith. I showed him our new results and told him that we would have to have Sat Nav for both of our ships. Since all the sets of Sat Nav were assigned to Navy Ships and Johns Hopkins could not build any additional sets for us without interfering with their work with the Navy, could he let us keep the set we had until we could build sets of our own. Johns Hopkins had advised us that if the Navy authorized it, they could give us a set of plans, and advice that we might need for their construction. I also asked to keep the Sat Nav that we had for some of the important observations we were expecting to make in the next year, some of which were important to the Navy. He pointed out that the plans were all classified Confidential. I countered that was no problem since we had been cleared for that classification, and the people who would work on the equipment were all cleared. He gave permission for us to keep the set we had for another year and would allow us to receive the plans for the sets if ONR would take the responsibility for them. This was easily arranged at ONR and in a few weeks we received the plans which took up the whole space of a four drawer file cabinet. When I saw this, I was dismayed to think how long it would probably take us to build our own equipment.

There was another complication. Johns Hopkins had warned us that the radio section of the receiver was particularly touchy, that even a small displacement of a wire could make it inoperative. They advised us to get Magnavox that had built that section for them to build ours for us. This turned out quite easy to do. ONR had provided us with the funds to build three sets of equipment and to get spares for possible future repairs. We needed three sets, One set for Vema, one set for Conrad, and a third set for our group that were operating on an Ice Island T3 in the Arctic each year. The head of our electronics department and I got together to try to determine what we should get for spares. Finally we decided to build four sets of gear, keeping one set at Lamont . Whenever either ship or the Ice Island would have a failure, they could determine in what part of the system the failure was in and we would send out a replacement from Lamont, returning the failed section to the electronics shop to repair, since they would certainly have the needed expertise to do it.

After a few years,the other oceanographic institutions decided that they too needed SatNav. There was a National Committee of Oceanography that met about each three months to consider the developments in oceanography and the common concerns. One Saturday morning I received a call from the committee, which was meeting in Rhode Island, asking me to join them that afternoon for consideration of the needs of satellite navigation. I agreed and got ready and took off for Rhode Island. I was asked to tell them about how the SatNav Worked. I explained its operation and evaluated the improvement in our operations and our data with it. They decided that each of the (about) ten ships in the countries fleet should have satellite navigation. They asked whether we would be willing to build copies of ours for them. I declined saying that to do so would take us about two years of the time of our electronics department while our needs from our shop could not be provided. I was dismissed.

After considerable discussion, ONR agreed to negotiate to have sets of equipment built for all the ships in the fleet. They approached Johns

Hopkins and Magnavox about how to achieve this. They replied that they would not build duplicates of the set we had since so many improvements had been made, that a new design should be constructed which included the computer as an integral part of the equipment. I don't know the details, but ONR managed to get Motorola to design and build ten sets of equipment. It took another three years before the other ships obtained their gear. When we saw the result, we asked to get copies for our needs, because the new one was much smaller and much easier to operate. This could not be done, bemuse ONR had only the ten made and they claimed they had no further funds to have additional copies made.

We had been upgrading our equipment so that the data recovered by the SatNav was automatically entered into the computer. Previously we had to manually enter the data which had to be 100 % accurate or the computer would reject it. The data was then computed and the Latitude and Longitude was printed out from the computer.

Soon, the whole geophysical community wanted to obtain Satellite Navigation. In order to do this, the Navy would have to declassify the transit system. I went to Admiral Smith and argued that the improvement in all national data was to the Navy's advantage because in all kinds of ways it influenced Navy Operations. I later learned that Paul Fye, Director at WHOI had also urged him to declassify the system for the general use of the public. Whether that had anything to do with it, I don't know, but about a year later the Transit system was declassified and soon SatNav was available from public corporations.

The Navy could not get the transit system adopted as the Navy"s navigation system because of inter-bureau conflict. The transit system had been developed by the Bureau of Ordnance, while the Bureau of Ships had been working on Loran C. This was a low frequency Loran system which a worldwide system of Loran transmitters (about ten in number I think) that would give positions to about a quarter of a mile too. Frequently the two systems disagreed by about a mile or so. This was soon tracked down to be due to the use of different geoids as the shape of the world by the two systems. These systems were both used for about ten years. A few years later the Air Force developed the Geographical Positioning System which displaced both the Navy systems because it had the much greater accuracy of about a few meters, now down to centimeters, and positions could be obtained as and when needed. Before the Gulf War, the system was degraded for civilian use to about 0.1 miles, while the services could get the higher accuracy. When the results of the Gulf War became general knowledge, public pressure (and probably also the end of the cold war) got the degraded signals removed. This system used satellites in high stationary orbits above the equator to provide precise navigation all the time on a world wide basis.

In the summer of 1967 we had arranged to make seismic refraction measurements with Texas A & M. in the Gulf of Mexico. Shortly after we got started, their ship developed trouble and had to return to port for repairs. Rusty Tirey and John Ewing were on board with me, as chief scientist. They were developing a reflection system of recording sub-bottom structure on a drum recorder. This involved firing a 1/2 lb. charge of explosives just beneath the sea surface. A hydrophone towed astern was slacked, so that the hydrophone was motionless in the water when the charge fired and for the next 10 seconds when reflected sounds could be received. The hydrophone would then be recovered to a position close behind the ship. A drum recorder was triggered as the charge was fired and the reflections from the bottom and sub-bottom were marked on the rotating drum. In this way successive reflections were plotted closely adjacent. Shots could be fired at about 5 minute intervals. Previous attempts at reflections had been tried at 1 hour intervals, but it had been found that things changed too fast to make much sense of the results. With the five minute intervals, they were able to make good sense of the reflections.

We decided to use our time in the Gulf of Mexico to test the system, while we were waiting for the Aliminos to be repaired and return. We encountered a region in which knolls occurred. These knolls were about 100 feet higher in elevation than the bottom about 2000 fathoms (12,000 feet) beneath us. The reflections showed that these knolls had upthrusted layers along their sides, so we called them diapirs. Away from the knolls, the layers were very close to horizontal. We discovered about 20 knolls while we were waiting. We were still finding more knolls when Aliminos returned and we had to continue with our seismic refraction work.

About a year later we returned and found a hundred more knolls in a region draped around the Campeche Bank a promontory of Mexico. The pattern in plan looked like a bowling pin whose top had been bent over. We published a paper in which we suggested that they were diapirs and probably salt domes, because of their great similarity to salt domes well known in the subsurface along the Texas and Louisiana coasts and continental shelves. The oil companies were skeptical since they believed that salt domes could only occur in shallow water areas.

We'll return to this subject again later.

We had used the former garages, seven in number, as a core storage area for the bottom cores that we were accumulating in all of our cruises. This was becoming a great "library" of bottom sediments that was being used more and more by scientists from other institutions, as well as by our own group. With two ships we were acquiring cores at twice our former rate and were running out of space for storing them. The cores were split and the halves stored in metal trays. We tried to sample from only one tray, keeping the second one intact for other investigations that might be initiated in later years. When samples were made available, if the study did not destroy the sample, we required that the sample be returned for possible use by other investigations.

We appealed to the National Science Foundation and ONR for funds to build a building which would be more suitable for core storage, and much larger for the storage of cores to be taken in the following years. ONR had supported much of the work in which we had taken cores. NSF and ONR conferred and decided that this was a reasonable request, and funds were provided. The core building was completed in 1967 and we held a dedication ceremony alongside the new Laboratory. The Laboratory was large enough to hold about ten times the number of cores we had taken, up to that time. Later when the Deep Sea Drilling Project was started, Lamont became the repository for those cores taken in the Atlantic Ocean, and the Laboratory had to be further enlarged.

The dedication ceremony was held on Oct. 12, 1967. Brackett Hersey, then a part of CNO (Chief of Naval Operations) spoke for the U.S.Navy, someone spoke on behalf of the National Science Foundation and Dr. Ewing spoke on behalf of Lamont Geological Observatory. The Assistant Governor of the State of New York also participated. After the ceremony the approximately 100 guests were given a tour of the new Laboratory and displays of about ten projects which depended on the cores .

The former garages were soon made available to other projects, and the offices which we had added above the garages for some of those studying cores, were made available to other investigators, while those studying cores were accommodated in the new Core Lab. This relieved the crowding of our staff that had been occurring, for awhile.

After the successful drilling operation on the Blake Plateau, Joides organized and made a proposal the the National Science Foundation for an ocean drilling project in the world wide ocean sediments.

The NSF when it was deciding to make the necessary grant, requested that Joides name the operating Institution and they should make the official request for funds at the Executive Committee Meeting, I was substituting for Dr. Ewing, while he was on sabbatical leave. Chuck Drake was taking my place on the Planning Committee.

We had been negotiating with a number of oil companies about Lamont running a drilling program in the deep ocean funded by the companies. It was looking like it was going to come into being, just as the NSF grant also was maturing.,

In the Executive Committee meeting the subject came up of which Institution should be the operating unit for the NSF grant. Paul Fye of WHOI spoke up and dropped out of the running, expecting us to volunteer. Since it looked like we were going to have the other drilling project, I decided that we could not possibly operate two drilling programs simultaneously, so I dropped out. Miami dropped out leaving Scripps the only group that wanted to be the operator. So it was decided, and Scripps took over. I later heard that Paul Fye was deeply disappointed in me. He never mentioned it to me, and I never told him why we had dropped out.

It later turned out that the oil company supported drilling program foundered, because the oil companies wanted the drilling results kept from the public for two years while they had access to them. That did not matter to us. as it would take two years after the results were in hand, before we could prepare and submit a paper for publication. Nevertheless Columbia would not let us sign such an agreement, and the oil companies would not fund it without the agreement. Hence that never came to pass.

Scripps, as the operating institution, negotiated with Global Marine to set up the Glomar Challenger for the drilling program. Since it was not practical to anchor the ship in a fixed positron for a number of days while a hole was being drilled, a system to keep in positron had to be devised. This involved putting a transponder on bottom that could be activated from a signal from the ship. With four hydrophones mounted on the ships hull the signal from the transponder could be monitored. With the base lines on the ship known, a position could be obtained relative to any three transponders. The fourth hydrophone was redundant which could be used if one hydrophone failed for any reason. With this information, a computer could determine the ship's position over the transponder. If the ship was moved by the wind, or currents, the various propulsion units could be used to maintain the ship's position within a about 20 feet. This was adequate for drilling in the bottom in deep water.

Scripps outfitted the ship with scientific equipment so that as much information as possible could be obtained on shipboard. This . information was used so that the chief scientists could control the drill samples taken. The planning committee helped Scripps to plan the shipboard scientific equipment needs.

Glomar Challenger was being made ready for sea in Orange Texas. The planning committee decided that the first drilling leg would take place in the Gulf of Mexico, moving out to the Atlantic Ocean and ending in New York. The second leg would drill a transect across the Atlantic, and especially the Mid-Atlantic ridge, since the age of the sediments on the flanks of ocean ridges was one of the most important geological questions at that time. Dr. Ewing and I were chosen to be Co-chief Scientists from the volunteers for the first leg, because we had been involved in most of the research on the deep Gulf of Mexico.

In 1968, Glomar Challenger was nearing completion which required sea trials of her modifications. Doc was tied up at the time, so I was the one to accompany the sea trials. After leaving Orange for sea, it was decided that the first drilling should take place beyond the Sigsbee Scarp. After a few hours of trials on various courses and speeds, it was decided to find the Scarp. I was asked to advise where the nearest position of the scarp was. While I had crossed the scarp in several places, to my knowledge, the scarp had never been located along its whole length. Consequently I could only advise them to head south and we would eventually cross it. After several hours of travel, we had not found it, the test party decided we would drill a hole where we were in about 500 fathoms depth.

Since bottom sound beacons were in short supply then, it was decided to put a buoy tethered to the bottom over on which to maintain position while the drilling equipment was tested. By the time the buoy had been emplaced, a front had passed and blown us away from the buoy. The storm decreased visibility to only about a quarter mile, and the buoy was not in sight. The captain tried a search pattern to try to get back to the buoy without success. Realizing that conditions were so bad, I had parked myself next to the radar screen to keep track of the buoy. This was difficult because the sea clutter was severe. The captain would come over and try to find the radar return from the buoy, but soon lost it. I could keep track of it since I could pay strict attention only to the radar screen. Finally the captain decided that we had lost the buoy. I wasn't supposed to have anything to do with the ships navigation, nevertheless I told them on the bridge that I believed that I was following the buoy positron on the Radar. In desperation, they decided they had nothing to lose so they would try to use my directions to get back to the buoy. In about 45 minutes we again sited the buoy. While the drilling equipment was readied to go down through the moon pool (a hole in the center of the ship through which the drilling would take place) the weather fortunately moderated and the visibility improved and the sea state started to drop.

The dill rig consisted of a drill bit which was attached to a very heavy section of pipe about 20 feet long. Above that a "bumper sub' Which consisted of a heavy piece of pip slotted so that vertical motion of about 10 feet could be allowed without lifting the drill bit from its drilling position. this was needed to allow for the vertical motion of the ship in the waves. Above the bumper sub the drill pipe was attached. It consisted of 4 !/2 inch o.d pipe with a wall thickness of 1/2 inch. Three lengths of 30 foot long drill pipe had been screwed together to make a 90 foot "stand" of drill pipe. The drill pipe was stored on racks on deck made up in stands. A stand of pipe was raised in the drill derrick and attached to the end of the bumper sub. Then the whole drill system was lowered about 90 feet and secured to the deck temporarily while another stand was raised into the drill tower and screwed to the drill pipe. This operation was continued until the drill bit touched bottom. The driller then lowered the drill as far as its weigh would push it into the bottom. Then the pipe was rotated by a hydraulic motor attached to the drill at the ship. Drilling takes place due to the rotation and thee weight of the bottom assembly. The ship actually holds upwards strain on the drill pipe lowering it so that the pressure on the drill bit is maintained at the level decided by the drilling superintendent. Normally, after drilling 90 feet, another stand of pipe would have to be added to the drill pipe In this test hole after drilling about 30 feet, we sent a core tube down the center of the drill pipe attached to a wire line. Drilling continued for about another 20 feet, then the core tube was released from the drill stem by sending a messenger down the wire line. After the messenger did its job, the wire line was recovered with the drill core. The core barrel was lowered to the deck opened, and the core encased in a plastic tube was recovered.

The core was then taken to the scientific laboratory where it was split in half and photographed alongside a meter scale as a permanent record. The various scientific investigations were then carried out on the core, that could be done on the ship. The most important ones for the drilling, was the sampling of a minuscule amount that could be examined under the microscopes. The assemblages of the globegerina, small carbonate shelled creatures about the size of a head of a pin, and discoasters, even smaller shells of silica could be observed. These usually could establish the geological age of the core section. With that information the scientists could plan when to take the next core.

On the sea trials, after one core was taken to show that everything worked properly, the drill was recovered and we returned to Orange .

The ship yard took care of any discrepancies that had been determined on the sea trials in about a week. Then the ship was outfitted with scientific equipment, supplies, food and fuel in about another two weeks, and the drilling program was ready to go.Then the scientific party of about 25 joined the ship at Orange.

Before I left to join the ship, I received a call from a geologist at Shell asking me to stop by their Houston Office on my way to the ship. When I got there, they took me into tie inner office, which they assured me that noone except those owned body and soul by Shell had ever been in before. They asked me whether it was true that we were going to drill one of the Sigsbee Knolls on which we had published a paper. I said yes. Then they showed me a lot of geologic sections and assured me that Salt Domes could not occur in deep water as they were a shallow water and land phenomena. And if they did, they could not have a cap rock since cap rocks were formed by the percolation of fresh water. They further said that if there were a cap rock, there could be no oil in it since oil formed at a temperature and pressure that could not be achieved beneath and ocean. They showed me a lot of data that they were convinced proved their points. I thanked them for their seminar and went on to join the ship.

A satellite navigator had been installed on the ship in the scientific laboratory. Since nobody on the ship other than Doc and I had any experience with Satellite Navigation, and Doc had never operated it himself, it fell to my lot to get the satellite fixes and to train others in its use.

When we left Orange, it took about eight hours to get to the Sigsbee Knoll we had chosen to be the one we would drill. The captain was totally amazed that we could drive up to the knoll and stop and say drill here. He had plenty of experiences of twelve to twenty hour searches with oil drilling of the chosen drilling sights after arriving at the approximate site. A bottom transponder was lowered and the ship located over the central peak of the knoll, and the drill started down within the third hour of our arrival.

The drill was "spudded in" by lowering the drill string until bottom penetration could no longer occur from the weight alone. This meant that the top 20 feet or so could not be sampled. We started drilling recovering cores at depths which the reflection records showed different layers. At a depth of about 250 feet sub-bottom we cored a layer which had all the constituents of cap rock of a salt dome. A little deeper we cored a sedimentary layer that had crude oil in it and at the bottom of the core a small amount of salt. So just about 24 hours after my visit to Shell, we had drilled into a salt dome with cap rock and an oil accumulation. The smell of the oil was so acute that everybody on board knew we had drilled into oil as soon as the core came to the surface. We immediately sent a message to land that we had drilled into a salt dome on the knoll, that we were now calling Challenger Knoll after the ship's name, and there was cap rock and oil there.

We soon received an urgent message from NSF to cease drilling immediately and to seal the hole as well as we could. They were worried that there might develop an oil spill. This would be infinitely embarrassing to the government since they had been trying hard to get legislation passed to outlaw oil spills! Several geologists on board were affiliated with oil companies and wanted, as we did, further drilling to delineate the potential of the knolls. The drilling superintendent, however, said he took his primary orders from NSF, so the hole was plugged. The core's from this hole were thoroughly studied by various oil companies with each one's proprietary techniques and the results were published in our publication of this leg of the trip. Many more studies were made than anyone in the academic community could have made..

One of the cores that we took before reaching the cap rock started extruding itself from the core barrel soon after it reached the deck. Creighton Burke, on board through the good graces of Mobil, and I tried to catch the extruded sediment, but there was too much, so we drilled holes in the plastic core liner to let the gas escape that was responsible for the extrusion. We believed this was methane gas and proved it by lighting the gas with a match. It would not sustain burning, but would burn as long as a match was held by one of the holes. We then caught some of the gas in a one gallon jug, which we then sealed and sent to one of the oil companies for analysis. It turned out to be methane as we had believed. I kept asking where all the gas was coming from, but noone had any answer. We noted what looked like frost on the sediment, and as it melted, the gas evolved. Much after this leg, when a group at Lamont had determined that there were gas hydrates in ocean bottom sediments, it was evident that this was methane hydrate. Gas hydrates lock up about 8 molecules of gas for each molecule of water and looks just like frost. It has almost the same properties as ice, taking almost the same number of calories to melt as ice does. There is no doubt that this methane hydrate was causing the extrusion of the core. Unfortunately, we lost most of the core as it was extruded in a pile on the deck before we could get the extrusion stopped.

The small quantity of oil was probably more thoroughly studied than any oil sample taken previous to this one. The oil companies probably contributed about 100 pages of analysis of the oil to the final report of that leg.

A follow up message from Drilling project advised us that we were above reproach for having drilled and found an oil show. Afterwards, the drilling project formed a committee of oil geologists to examine the plans for ocean bottom drilling to make sure that in the future, no oil would be sampled. They did their best, but still oil shows were found in a number of subsequent holes in the next few years. One of the scientists drew a cartoon of the ship on the surface, a drill string snaking through the water and drilling into the bottom with the word Reproach displayed on the bottom near the hole, thus we were drilling "above Reproach"

We drilled about four more holes in the Gulf of Mexico which provided much good science, but no more oil or salt domes. The leg continued into the Atlantic where we drilled about three holes on features that we had found on Vema. Then we made our way to New York where the second leg was planned for departure. Our two month leg terminated there.

On this leg I had to evolve the procedure for the shipboard reports of the drilling. This procedure was adopted for subsequent legs.This involved a description of the drilling of each hole, the reasons for the choices of the cores that were recovered, reports of all the shipboard measurements, that had been made including each scientists interpretation of the results of their measurements, a description of the core and finally the overall scientific summary of that hole, and the conclusions of the scientific party of the results. Scripps had provided a typist who would type these reports almost as quickly as they were written. I had the responsibility for the primary descriptions and the final summaries., besides my part of core geological description.This, of course, meant keeping after individuals for their contributions. Between that, maintaining the satellite navigation, and being on the drill platform when each core reached the surface, I kept pretty busy.

Life on the Glomar Challenger was pretty easy, compared to the ships we were normally at sea on. She was quite stable and had easy movements. While drilling, the bow was kept upwind to assist in stable positioning. There was a quite spacious lab where most of the on board scientific work was carried out. There was a lower lab where the core photography and core storage took place. There was a lab where the geophysical equipment which was sometimes used for core location was maintained as well as the Satellite Navigation equipment. There was an office, where reports were written and later typed and all records, were filed.

Since drilling was continuous with coring occurring at intervals, four meals were served daily. One at 8 A.M., one at 12 noon, one at 6 P.M. and one at midnight. One could get a snack from the galley any time as requested, and there were at least four kinds of pies , four kinds of cakes, and ice cream always on a table in the mess hall to take as desired. The scientists lived in quite spacious, (by oceanographic standards) two man cabins. There was a lounge for the scientists, and another for the drilling crews that were off duty.

It had been the consensus opinion that the sediments on the ocean floor made a continuous record of events from the origin of the basin. It was soon apparent that there were as many interruptions as encountered on land. By about leg three, coring was attempted on a continuous basis. This meant slower drilling, as taking a core interrupted the drilling for about a half hour for each 20 feet of core. In general , only about 80% of the section cored was recovered. At this stage of the drilling, one could only continue drilling as long as the drill bit lasted. If it was necessary to change a bit, because of a more difficult layer, or a worn out bit, the whole drill string had to be recovered, a new bit put on, then a new hole drilled as quickly as possible through the section already sampled, so additional section could be drilled. Since this was very time consuming, this techniques was only occasionally used. By about leg 12 it was realized that reentry of the hole was desirable, and by about leg 20 a technique had been developed. But we did not have this available to us.

On our entry into New York Harbor, we had to pass under the Verrazano Bridge. Our drill tower was so high, it would just clear the bottom of the bridge. The captain made sure we came in at low tide to have an extra margin of safety. As we approached the bridge, it sure looked from the deck like there had been some miscalculation and that we would hit it. However we cleared it by about 6 feet.

A reception awaited us in New York to celebrate the successful end of leg 1. The scientific crews were changed and Leg 2 got underway a couple of days later, with Art Maxwell as one of the co-chief scientists.

I remembered these incidents later so I will relate them as a flashback.

At the time we were working in the Gulf of Mexico with the Texas A. & M group. Vema put in to Galveston for refueling and revictualing. I was Chief Scientist.

The first evening we were in port, several of us were too tired to go ashore, so we were talking together in my cabin. I had a bottle and was sharing a drink with the others. The Captain had a rule of no alcohol on board, to avoid drinking problems. However, he would look the other way as long as you were discreet, if you brought a bottle on board and had an occasional drink.

During our talk, I remembered that I was to make a telephone call to Dr. Ewing. So I left the others in my cabin and went on deck to go ashore. The gangway was way forward, and we were close to the dock, so I chose to jump across from the rail to get to the shore phone. There had been a heavy dew, and the rail which was covered with stainless steel, to avoid damage from our work, and my rubber soled shoe made a perfect Cutlass Bearing, so my feet slipped instead of propelling me to the dock, I fell on my stomach on the dock. There was nothing to grab hold of, so I bounced back into the water between the ship and the dock. The mooring lines were too high for me to reach, and the ship and the dock were too high for me to climb back either to the shore or the ship. After recovering my breath, I shouted for help, several times. Noone heard me. Apparently, the ship watch stander was somewhere else making his rounds. Even though I was in the water with only the hull between me and the group in my cabin, they could not hear me either. I tried several times to elevate myself enough to grasp the mooring line which sagged between the ship and the dock, without success. I decided that I was in a fix. There was no ladder to the dock that I knew of, and repeated shouting failed to catch anyone's ear. I decided to make an all out effort to catch the mooring line, so I submerged quite deep and swan vigorously to the surface kicking hard with my feet. I just was able to catch one finger on the line. My weight on the line caused the ship to move closer to the dock so the line sagged, so with further vigorous kicking I managed to shift my grip until I had a whole hand hold on the line. I rested from my effort. Then I was able to get my other hand and my leg across the line and climbed up until i could get back on the ship.

The harbor was full of trash and fuel oil, spilled from various ships, and I was covered from head to foot and was a mess (Nowadays harbors have been cleaned up and such a plight could not occur). I went to the door of my cabin doorway, reached in and got a towel and went forward to the shower which I entered clothes and all. Needless to say the group in my cabin were startled. After I got cleaned off, I dried and draped the towel around me and went back to my cabin and got dressed. I was also bombarded with questions. After I explained to them what happened, we all had a good laugh, and I went ashore across the gangway and made my call.

Afterward in thinking over the incident I realized what a hazardous situation I had been in. Further thought also reminded me that I could have swum to the bow of the ship where I could easily have reached the chain to the dolphin striker, beneath the bow sprit, which I could have climbed back onto the deck. I never thought of that though while I was in the water.

Early in the 1960's Dr. Ewing devised a code system so the chief Scientist on each ship could radio every other day giving the ship's position and a report of how the various projects were faring. These were all numbers which were given in a regular sequence that reported the progress of the ten to fifteen projects that were underway on board. This was inexpensive and served to keep us on shore aware of each project on the ship, and served as a stimulus to the Chief Scientist to make sure each scientist or technician was functioning properly. It worked well generally. On one cruise, when Bruce Heezen was Chief Scientist, there was less and less to report other that the soundings and coring that Heezen was particularly interested in. Because the gravity gear was not working, I opted to relieve Heezen in Tahiti, the Port to which they were heading. When I arrived there late in the evening, the ship had still not arrived. Shortly after breakfast, the ship came in and I went on board. Heezen disappeared until the ship was just about to sail, so I didn't learn much from him about the state of the equipment on board.

I went to work on the gravity equipment and by working all three days the ship was in port, I was abler to get it working by the time we put out to sea. I soon found out that the scientific staff were unhappy and disgusted because their projects weren't producing any results. As quickly as I could I went to each one to find out what was wrong. Usually, by advising them I could get them to get their gear working again. In one case I had to help with one set of equipment that I knew little about, since the grad student's efforts were lackadaisical. After a couple of days, we got it working and he got a lot of data on the rest of the cruise. He had anticipated having to finish the cruise, without getting any data for his thesis, which would mean he would have to go along on another cruise, delaying his work and his attempt to get his advanced degree. Much later, when I met him at a scientific meeting, after he had his degree and left, he told me that he had thought I was an awful ogre to make him work to get the gear working, but that if I hadn't he probably would have dropped out.

I not only insisted that each one work to get their gear working, but that each one get a spare of any gear that was used in the water working if possible, so that if anything failed again, they could immediately make a replacement and continue to get data. It is a certainty that any gear that goes into sea water will fail eventually. Meanwhile they were to work on the failed item so that it would become a working spare. Soon there was a marked improvement in attitude of the Scientific Staff.

About 1964 we had a great tragedy. An explosion on Vema killed John Henion. This resulted from our program of firing small charges at about 5 minute intervals on the reflection program. When it was reported to us by radio, the Captain requested instructions about the body, recommending burial at sea. The alternative would have been putting the body in the deep freeze and returning it to Lamont at the end of the cruise, or shipping it back from the next port. The ship was off the South American west coast at the time, and the facilities in those ports were quite limited.

His wife decided that burial at sea made the best sense, and we passed the message. Several months later, she regretted this decision, but it was too late, he had been buried.

We obtained as much information as possible from the ship about the accident, but could not determine what had gone wrong. I recommended to Dr. Ewing that we form a committee to investigate further. He appointed me as chairman, Capt. Sinclair (Navy Captain retired helping me with gravity work), and Rusty Tirey (with demolition training in World War Two in the U.S, Navy) as committee members. We could not find out enough about the accident by radio so we had to wait to interview scientists returning from the cruise .

The conclusion was that John had violated the safety precautions we had established, and also those developed in the services. He had the box connecting the detonators, another containing the burning fuse, we were using to initiate the explosion, and the TNT blocks all lined up on deck near his shooting station. Instructions had been to separate each of these items by at least twenty feet. He apparently had been making up several charges in advance and lining them up to use next to each other. This too was a a no no. No charge was supposed to be made up until the previous one had been fired. Either a spark from lighting one charge, somehow lighted the fuse of another, or the charge after being lit somehow was deflected back onto the deck next to another waiting its turn. Since noone else was on deck at the time, we could not determine exactly what happened.

This was incomprehensible to us, because John had been trained in the marines in explosive safety, and he had spent a month on a previous cruise with one of us, that had been firing charges, so that he well knew the precautions we insisted on. He was the only one on board who was responsible as he not only was the shooter, but he was the Chief Scientist on that leg who was supposed to make sure that safety precautions were followed. It happened soon after he had a night's sleep, had showered and had his breakfast before he relieved the shooter on the previous watch, so he could not have been exhausted. .

The reflection program was continued, but everyone was alerted that the safety precautions <u>must be followed</u>. John Ewing, in charge of the reflection program, was determined to make sure that such an event did not happen again and set several people to work to devise a different, less dangerous source for the reflection work.

About a year later Bernie Luskin and Paul, whose last name escapes me now, developed an air gun. They had heard of a molding process that used compressed air to drive the two parts of a mold together to press a piece of metal into a desired shape. For instance, many metal ash trays were made in this way. The air gun had a chamber in which the air could be pumped to about 3000 pounds per square inch. The lid was held closed by a strong magnetic field from an electromagnet until the pressure had risen to the desired level. A sudden interruption of the current to the magnet caused the lid of the chamber to open and the air pressure was then abruptly dumped into the water. The lid was constrained in its opening and the resumption of the current in the magnet would close the gun before water had time to flood the chamber. This made a bubble similar to the one created by an explosion whose expansion created the sonic wave desired. The gun was about 10 inches in diameter and about one foot long and was made of aluminum, so that sea water corrosion was almost no problem. This gun had an additional advantage in that it could be fired at about 20 second intervals, instead of the five minute intervals that we had been using with explosives. This made the record density much greater which improved the results greatly.

Thus our reflection work produced records of comparable record density to our sounder records. The sub-bottom records became much better for use in interpretation of the bottom layered structure.

John Ewing's group now added another great addition. We obtained some sonobuoys that the Navy had used in submarine detection. We would put one of these over at suitable times. Their signal could be received for about 30 miles as the ship progressed. The sounds from the air gun were detected and transmitted by radio to the ship. We would put one over the side when the bottom layers were seen to be quite parallel to the ocean floor, and the refraction data that could be recorded would provide information of the sound velocity in the layers which improved our ability to interpret the reflection records.

The air guns greatly improved our records on the second cruise where we determined the outline of the area of salt domes near Campeche in the Gulf of Mexico. The section across the knoll, that we later named Challenger knoll, gave us the information needed so that I could later direct, with satellite navigation, Glomar Challenger directly to the desired drill site.

Shortly after we had developed the air guns, Bernie Luskin and Paul _____ left us to form a corporation to make air guns for use in geophysical exploration for oil. Later on they sued us to get us to pay them a license fee for using air guns on which they had obtained a patent. Common sense ultimately returned and we got a free license.

About 1966 I went to a Society of Exploration Geophysicists meeting in Tulsa, Oklahoma. Three things were featured in that meeting. These were the use of Sea Surface Gravimeter, Air guns, and Satellite Navigation in their work. All of these had been initiated at Lamont.

The following incidents must have happened just before the Glomar Challenger leg 1.

I was going to join the ship at Le Havre in France. Laney Kohler, the Captain's wife was also, so we managed to get on the same flight. When we got to the ship, after I stowed my clothes and checked the situation with the departing Chief Scientist, I went forward to check in with Captain Kohler. He insisted that I have one of his famous martinis. He kept a three gallon jug of them in his personal refrigerator. He never had a drink himself while on board, but he often offered the chief scientist ONE. He also used them to grease the ways for port entrance formalities. While we were talking I said to him "Henry I brought your wife with me, so now you will have to quit helling around on shore". He replied "Those days are long gone, Joe, and thank god too".

While ashore in Le Havre I bought some cooked mussels from a street vender. I got very sick the next day when we had sailed. Of course I thought it was sea sickness, but when it didn't go away in a day or so I just suffered. After being too sick to eat about three days Laney told Henry he ought to give me some medicine. He told her he would when I asked him too. Nevertheless, Laney prevailed and I was soon better.

When I left Lamont Doc had told me it was very important to dredge rocks from the MAR (Mid-Atlantic Ridge). Dredging usually took a long time, because the dredge had so much water resistance it usually took several hours to lower to bottom, and it was very difficult to determine that it was on bottom. This didn't suit me, so I decided to attempt another way to dredge. At first I thought that I would attach the dredge to the core head, which weighed about 1400 lbs. so that it could get to the bottom quicker and would give a better signal when the core head was on bottom. The dredge would have to have a chain connection of about 50 feet from the core head so that when the core head was just above the bottom the dredge would drag along the bottom. After thinking about it a while I decided that the dredge tethered by a long chin would probable twist around the wire rope while it was being dragged to the bottom, and thus would never reach bottom properly. I had to think of a way to deploy the dredge and chain after reaching bottom. While pondering this problem, I also thought, why send the core head to the bottom without getting a core. A core from the same location that rocks were dredged would be a dividend.

Finally I decided to attach the dredge by a five foot piece of 5/8 inch diameter rope to the core release mechanism. threaded through a hole in the core attachment. When the core reached bottom, the core release would operate dropping the core, and the rope attaching the dredge would break dropping the dredge on bottom near to the core. By coiling the chain attaching the dredge to the core head in a potato sack inside the dredge, the chain was prevented from tangling on the way to the bottom The chain was kept in the sack by a lacing of cord over the mouth of the sack. This light cord would break when the dredge started to separate from the core head to which one end was attached.

To operate it , we lowered the rig in the normal way that we used for just coring. After the core was taken and the dredge dropped to the ocean floor, we would ease the core just out of the bottom and we would drift. After drifting a few minutes we would see the tension meter start to jump indicating that the dredge was being dragged along the bottom. We decided to drift about one half hour hoping that we would dredge some rocks. Watching the tension meter one or the other of us would say"see that jump. that was a bigger rock". When we were finished dredging we would start recovering the core. When it was secured to the chain fall of the ship's A-frame, the dredge would still be hanging 50 feet beneath the ship.

We arranged a whip (a 3/4 inch diameter rope fed through a block attached to the cross trees about 100 feet above the deck) . By attaching the whip to the chain, we would take the dredge load so we could detach the chain from the core head. Then we would lift the dredge up and swung it on deck. This arrangement worked very well and we took about ten cores and dredges on the ridge. One time we recovered a rock so big it was only jammed in the mouth of the dredge. We feared we would lose it while bringing it on board, but we managed to save it.

Most times we would recover ten or more small boulders about 6 inches in diameter just smaller that the mouth of the dredge, and even smaller boulders. None of the rocks appeared to have been broken off by the dredge, so they must have been already broken off and weathered and laying on the bottom. Back at the lab they were astonished about how many rocks we had recovered. I never heard what they had learned from them, as their study took so long, that I was engaged elsewhere.

Another incident that I should have included in the discussion of making the first measurements of gravity on a surface ship was interesting. At a scientific meeting about a year before those measurements, I was asked when we could give up using submarines and measure gravity on a surface ship. I replied probably not in my lifetime. So you see how small my crystal ball to see the future was!

In 1969 I had a request from Columbia in South America to make some gravity measurements along many of their roads. I persuaded Bob Wall to take our Worden Meter and to go and make the measurements. He was there about a month. He was returning on a Sunday, so I went down to Kennedy Airport to pick him up.

His plane came in on time and I saw him going through customs and immigration from the upstairs gallery. He finally completed these essentials , and started toward the door. There was a desk along the way that he had to pass. Just as he was passing it, I saw a guard approach him and take him into a nearby office. I went downstairs to the exit doors of the arrival area, but the guard at the door would not let me in. I talked to him and told him that I was the supervising professor of one of my students and that I had seen them take him into an office. Could I go see what the problem was, perhaps I could help.

He told me to wait and he went into the office. After a few minutes he came back to the gate and said he would escort me into the office. When I got there I told them that Bob was my graduate student and that I was a Professor of Columbia University and asked what the problem was. I was told that their radioactive detector had indicated radioactivity in Bob's equipment. I laughed and said of course there was. The Worden Gravity Meter was made of quartz and was able to measure gravity to a part in ten million. Quartz has the property of accumulating an electric charge which would distort the measurement, so a small amount of radioactive sand was placed in the bottom so that the radiation would discharge the electric charge. They asked to see my credentials.

After they decided I was really a professor at Columbia University they accepted my explanation and told us we could leave. We started to walk out past the desk where Bob has been stopped, when one of the guards quickly stopped us and told us we would have to circle around the desk since their detector was located there and that our Gravity Meter had given such a large indication they thought the needle had been bent. We left without any further complications !

Later Bob Wall and i went to calibrate a new Sea Gravimeter on our northern calibration range in August. At the end of a hot day we found a small motel in northern New York and arranged for a room. Bob decided to take a shower while I made the evening gravity observations. He soon came out disgusted saying he could only get a slow drip out of the shower, so he was going for a short walk.

After he left, I finished the observation and went in to look at the shower. I too, desperately wanted a shower. I decided that the head was plugged up with lime concretions. Since we had my tool box in the gravity truck, I took the shower head off and drilled out the concretions in the head with the portable electric drill we had with us. I replaced it in the shower and took a satisfying cool shower. When Bob returned, he noted my hair was wet, and I told him that I had had a very satisfying cool shower.

Didn't he want one. He outrightly refused and wouldn't go near it again.

That fall the second oceanographic meeting was held in Moscow. A number of the people from Lamont attended. On my arrival I found that most of the attendees were arriving from many countries around the world about the same time, towards the end of the afternoon. We were all bussed to the same hotel. There was only one desk to handle the arrivals and assign rooms, which meant there were probably about two hundred people waiting in the lobby for their chance. Intourist, which was handling the arrangements was in no hurry, and each one took ten to fifteen minuted to process. All of these people and their baggage were in an imperfect line around the lobby, much disgruntled. It appeared that I would have an extremely long wait. About that time, one of the scientists from our west coast, with whom I was acquainted, had reached the desk and had completed his registration except for one detail. He had to have a room mate. He came back along the line asking if anyone wanted to room with him. I quickly stepped forward and volunteered, so I was ushered to the head of the line and was soon registered and installed in our room.

We learned later that the Intourist desk closed at 10 P.M. (local time of course) leaving more that half of the scientists to spend the night in the lobby. My roommate and I rarely saw each other, since we were attending different sessions and we got up and went to bed at different times.

We were assigned to tables in the dining room which sat ten people. Each person had to sit in their assigned sit and nowhere else! I knew all of the people assigned to our table. Our waitress would take our orders for breakfast, for example, and would disappear for about 40 minutes. She would finally arrive with all of the orders completely filled- no second trips for her. Needless to say those expecting fried eggs and toast found them congealed in fat, stone cold. Other meals would arrive in various states from stone cold to piping hot, apparently depending on the order that they were dished out. Complaints were fruitless. None of us was tempted to convert to Communism.

The gang at our table spent the whole two weeks, we were in Russia, trying to get a smile form our waitress. No matter how comical some of them were, there was no smile!

We were not allowed to leave our floor with the key to our room. We had to turn it in to a woman at a desk near the elevator, who looked more like a jail guard than anything else. We all became convinced that each room was bugged because of things that were said to us by various

Russians, that they could not have known otherwise. One evening a group of us congregated in Creighton Burke's room and had a bull session. Naturally we discussed the various papers we had been auditing. We added many derogetory remarks about Russian papers we hod heard, from time to time for the benefit of the auditors.

One evening I went out the front door of the hotel to find a group of Russian women pouring hot tar and repairing the street in front of the hotel. This was so astonishing to me, that I stood and watched for a while. Soon there was quite a group of us on the steps watching. Some even took pictures. I had not even brought my camera, because of the stories I had read in the press of Americans arrested for spying, who had taken pictures in Russia. Nothing happened, however, to the picture takers.

I don't remember anything much about the scientific papers that I heard. They were quite a mix of papers, some excellent, some good, some poor, and some awful. This was typical of international meetings in which I took part.

One afternoon, when there were no meetings of much interest to me, I went to the book store at Moscow University. I discovered a book that looked interesting from the diagrams and was about to buy it, when I discovered it was a translation of the Ewing, Press, and Jardetsky book.

One evening I was invited to dinner by a Russian named Boulanger, who was also involved in gravity measurements. Obviously, he must have been left over from Napoleon's march on Moscow. His English was poor, and my Russian was nil so we had a hard time talking. He had a very forbidding appearance, but he was quite gracious on this occasion. I had brought with me a reprint of results of our first four submarine gravity cruises, and I gave him a copy, expecting that he would give me a reprint of some Russian gravity measurements. However, he did not.

By the end of the first week, the atmosphere was so forbidding, and the meals were so bad, I was ready to go home. I couldn't wait to go home. But no change in travel plans were permitted, so I had to finish the stay as planned. The people on the plane I traveled in when I left, cheered when it was announced that we had crossed the border out of CCCR.

Right after the International Congress, Time magazine reporting about the congress had a long interview with Bruce Heezen about magnetic measurements that he purported to have made. DR. Neil Opdyke, a senior scientist at the Observatory, came to Dr. Ewing and protested that the work that Bruce had reported had been done by one of his students under his direction. Doc quizzed him at length and ascertained that his story was true. Doc then called Bruce in and challenged him on the report. Bruce claimed that he had instituted the student to do the work for him. Doc told him that he was convinced that Opdyke's protest was true, that it was unethical and that he would have to do something about it.

After Bruce left, Doc called me in and discussed it with me. After a while, Doc said I guess I will have to kick Bruce out of the observatory. I cautioned him, that if he did that, he had better tell Dr. Kusch, who was provost of the University, first, as Bruce would certainly go directly to him and protest.

Doc took my advice and went to see Dr. Kusch. He told Doc to wait, that he would have a talk with Heezen. Apparently Heezen convinced Dr. Kusch, that he was a professor with tenure, he would not accept removal from the Observatory and would not resign.

I don't know all the steps that followed but Dr. Kusch told Dr. Ewing that Heezen had gone to the American Association of University Professors and they had told Dr. Kusch that they would fight on Heezen's side against any action taken. DR. Kusch told Dr. Ewing that he would have to let Heezen stay at the observatory as the University did not want to have an altercation with AAUP.

Reluctantly Doc let him stay on in his room at the Observatory, but did not include him in any observatory plans or operations. A few weeks later, Heezen started removing all of the sounding data from the observatory to his home. I guess Doc was away, because I had to tell him he couldn't do that as it had been acquired on government grants and contracts with the Observatory, and was Observatory property. He ignored me and continued his activities.

I called Dr. Kusch and told him that I would have to report that Heezen was stealing government property from the Observatory. He asked me not to take any action immediately. I told him that I had an appointment that day that I could not break, but that I would have to call our contracting officers in Washington first thing the next day.

When I returned from my appointment late in the day, I had a message from DR. Kusch telling me to call him before I did anything else. When I called Dr. Kusch he told me that I was to take no action to leave the matter between him and Dr. Ewing who was returning that day. I told him that as I had done all of the contract negotiations I felt obligated to take immediate action. He told me that DR. Ewing, as Director of the Observatory, was the only one authorized to act in the Observatory's interest and that I had better leave it to Dr Ewing and Dr Kusch. I got the strong implication that I would have serious trouble if I acted. I decided that I had nothing but trouble ahead of me if I ignored his warning, so I waited for Dr. Ewing's return.

As soon as Doc returned, I told him what had transpired. He told me to let him take care of it, so I did.

I don't know what finally happened as after that I was not informed of further actions. I do know that Heezen convinced the Contracting Officer's that he had to work on the records at home and took many more to his home. Heezen maintained his office at the Observatory, but had little interaction with any of the staff, to the best of my knowledge.

Heezen no longer was able to get any of his projects included on our ships schedules. He started to get ship time from other institutions for his desires.

About this time we had outgrown our machine shop. All this time we had to conceive and build all of our instruments and equipment. Very little that we could use was available commercially. With a shop adequate for a seven man machine shop force, we were finding ourselves cramped to operate, equip, and instrument our two seagoing ships, as well as the requirements of other projects under way at the Observatory. Any equipment put into the sea is inevitably lost damaged or corroded so replacements had to be built. Also, improvements desired would often make existing equipment obsolete. We obtained a grant from the National Science Foundation to construct a new machine shop which could accommodate about fifteen machinists.

We were able to equip this shop with several larger and more capable machines, and a separate room for welding. This made it possible to keep the cinders and other dirty results of welding from the more delicate instruments being built in the rest of the shop, for the first time.

Unfortunately the new shop, which took more than a year to design and build, met two new problems. The first was that commercial companies for the first time started developing and making available many new instruments for deep sea work. This was a result of the great expansion of oceanography by the recognition of this inner space impact as well as the success of the programs of the developments we had participated in the past twenty years. The second was the effect of the bean counters (government cost accountants). We had always had a part of our budget set aside for shop support of all of our programs. The supplies and materials needed could always be readily available as our work requirements dictated. Now the bean counters decided we would have to have each area of research, budget for their own needs as they arose and include these budgets in our contract and grant requests. This requirement slowed and almost stopped our instrument equipment development. The idea now had to be conceived, proposed in our annual request for funds in each discipline. This added at least an additional year to the development of an instrument. Most of our scientists, rather than waiting another year to proceed, often changed the direction of their research to avoid these delays. By the time funding was obtained, they found themselves preoccupied in other directions of their work. The net result was the new shop mostly built and improved instruments previously developed and much innovation was curtailed.

Nowadays, I believe, most instruments are built and purchased commercially. The new machine shop never developed to its full capacity and shipping and receiving that had been increasing started using a part of the new machine shop space.

Another item that should have been included about 1963 follows. I had been approached by a Canadian Government agency about making gravity measurements on Lake Erie. I replied that we could do that but that we thought we should also bring along a bottom profiler equipment and detail the structure of the sediments for about 100 ft sub-bottom. I told them we would need about three people to make these measurements. They agreed that this sounded good and in July we arrived in St. Catherines, Ontario to install the equipment. After we go underway in Lake Ontario, we quickly entered the Welland Canal and transited to Lake Erie, where we started a grid of North South Lines about two miles apart.

When we had arrived we found it was difficult to find a space on board to install the stable platform and gravity meter. We finally had to choose a space under the ladder from the weather deck to the lower deck. The space was quite tight but we managed it. While making the installation I had to get directly under the ladder behind the stable platform to connect the electrical circuits for the equipment. The power could not be turned off while these connections were made. We hung the power panel on the bulkhead opposite the ladder and I was making the connections when i inadvertently touched one of the terminals and got a small shock. Naturally I jumped back, which made me bump my head on an ladder.. That made me recoil which made me touch the terminal again. I made three or four cycles before I could get clear. Bob Wall, who was going to make this project his thesis got a good laugh watching me.

In lake Erie we found that flies were numerous, and the vessel was inadequately screened, so we had to learn to live with fly bites. They were a great nuisance, but we survived. In the first week we burned out one of the vacuum tubes used in the stable table and found that we only had one spare with us. We contacted the shore and requested that they purchase several replacements as we could not be sure when we might need another one as there were several of that type used in the circuit. They had trouble finding spares, because these were tubes no longer commonly used, but they did find two and notified us that they would get them to us at Port Ewen where we would be going in to resupply. When we got to port Ewen, we got a surprise, as the ship was docked on Friday and all hands left for the weekend, leaving us there as we had no transport. The cook also left, so we had to fend for ourselves in the galley, as Port Ewen was quite a small place and had no restaurants or motels. It was a long weekend until Monday when the crew returned. This pattern of operation continued for the month it took us to survey the whole lake.

We were glad to get away from the flies on our return. It didn't take us long to off load and get on the road for Lampnt. I offered to pay for the vacuum tubes they had procured for us, but they refused. At any rate Bob had obtained the material for his thesis.

Back at Lamont, I got a call to come to the University to meet with the Government Contract Division of the University about contract funds. When I got there I found out that representatives of all U. S. Navy government contracts at the University had also been called in. We were advised that the University had been collecting what was called fee of about 3 % on every contract for the past twenty years. This was a fund that was allowed for all the small things that could not be charged directly to the Government Contracts but were necessary to carry out the work. The whole fund totaled about $1,000,000. It had been decided to now make this fund available for expenditure. We were asked how we thought it should be spent and how apportioned since it had been earned on our contracts. After some discussion, it was agreed that the fund would be divide up in proportion to the total Government Contract funds that had been accumulated by each part of the University., for that group to spend on their work as they thought fit. This would mean that Lamont and Nevis, the two largest contractors would get about $100,000 each..

When I returned and told Dr. Ewing of this windfall, we agreed that we would not spend it, but would keep it for emergency use. However we never heard any more about it . When I inquired about it I got nowhere, and it never materialized.

Soon it was learned that the University funding was in a bad way. It seems that most Universities, for a number of years, had been investing some of their funds in the stock market and their funds had grown significantly while inflation too had been rising significantly. Columbia, however had maintained most of their funds in fixed interest vehicles and hadn't been able to keep up with inflation. Finally they had moved a significant portion of their funds into the stock market just in time for the stock market to fall, which now compounded the problem of Inflation. We guessed we knew where the fee funds went.

One time I had sat next to Professor Rabbi in a faculty meeting. We had had some time to visit and I complained to him that we had built this great successful Observatory mostly with contract funds and Government Grants and the University had contributed almost nothing. Meanwhile, the University got the credit. He replied that I should look at it that while the University had contributed little, that they had not got much in our way.

In the mid sixties, the student riots occurred, and students occupied the President's Office and others in Low Library, making quite a mess. It spread finally to the graduate students, and even to a few of the Professors.

One morning our students congregated in the seminar room of Lamont Hall and our staff joined them. They complained that they had too little to say about how the programs were run at Lamont and they should. I started to read an editorial from the New York Times that had quite well summarized the students discontent and discredited its basis. I was not allowed to finish reading it by shouts and loud talk which overwhelmed my voice, so I quit and sat down. About then most people left. When people had calmed down after a few days, we called them together and told them that we had come to teach, and we thought they had come to learn and that we intended to get on with our research. If they wanted to quit, it was o.k. with us,but we were going to carry on. Nothing more occurred at Lamont, and the work went ahead.

At the time I was presenting a course one day a week at the Geology Department in Schermerhorn Hall on the main campus. The students had made protest lines around all of the buildings, and many of the Professors would not cross the lines Nevertheless, I continued to go to Schermerhorn Hall and hold my class. I would cross their lines pretending that I didn't even notice them. One of the students, in my class, got up and started to protest. I told him that I intended to continue to give my class lectures, and that students could attend them or not as they saw fit. However, at the end of the year they would be held accountable in the final exam for the material. They all stayed and no other protest was made.

Finally President Kirk had enough. His wife was having a serious fight with cancer at home, and the conflict on the campus and the trashing of his office became too much for him, so he resigned. Andrew Cordier took over while a search was made for another President. Cordier met with student representatives and made concessions that stemmed the protests. What they were I don't remember, except one was that the University would discontinue all classified work.

At the time, the only classified work that Lamont had going, although we had had a considerable amount over the past years, was the work at the Sofar Station at Bermuda. We were informed that we would have to terminate it. We had a staff of about twenty people at the Bermuda Station, most of whom had been working on the project for nearly twenty years. They were to be thrown out of their jobs and the work terminated. We felt that, if a University or any of its people had expertise that was needed by our Government, they had the obligation to provide it. We could not justify in our minds disbanding the Bermuda Station.

Dr Ewing, Gary Latham, JIm Dorman , Arnold Finck and I met at the University club, downtown, where Doc was a member, and decided to form a corporation to carry on the work at Bermuda. Doc had had considerable contact with George Rowe, who was a lawyer and a director of the Vetlesan Foundation, since Doc had won the Vetlesen Prize. Doc got in touch with George and he guided us in the formation of a corporation to be called Palisades Geophysical Institute. To this day he helps us with our legal problems, but now, we pay him for his service. At the first meeting Doc was elected President, I was elected as Treasurer, add Arnold Finck as Secretary. Before we could approach the Navy we had to have some funds that we could show financial responsibility. The Vetlesen Foundation gave us a grant of $25,000 for this purpose. Doc persuaded Lewis Weeks, a geologist that was retired from Exxon to become one of our Directors, and Hollis Hedberg, a geologist of Gulf Oil, partially retired, to be on our board of directors so that we were not just a Columbia University group. Our intention when we formed the corporation was to hold the fort until Columbia University came to its senses and resumed classified research as well as unclassified research. Now in 1999, this has still not happened and PGI continues the work started at Bermuda.

Hudson Laboratories was also being closed by Columbia University and we hired Frank Mongelli, who had been contract officer there, to be our office manager and contract officer. He hired office space in Nanuet. He obtained a desk, chairs and files from government surplus and we were in business. Frank and hIs wife refinished the office furniture and set up a filing system. The Office of Naval Research terminated their contract with Columbia University and wrote a new Contract with PGI. The work in Bermuda continued without any disruption.

We soon found out that we were spending about $25,000 a month. We had to pay our bills , and we could not expect to get reimbursed by the Navy for about a month after we sent in our bills. Frank and our CPA and I went to a bank in Nyack, whose President knew about Lamont and Doc and I, and asked for a line of credit. We discussed the matter with him and one of his vice presidents. They asked what interest rate we expected to pay. I answered the prime rate. The vice president started to protest vigorously, but the president smiled, stopped him, and said o.k. we could have a line of credit for $100,000. After we left the bank, our CPA could not believe we had got it at the prime rate. He said that was unprecedented for a small corporation.

We actually never used the line of credit, as Frank has always made sure the bills were quickly submitted to the Navy and he followed up on them every couple of days. The bank has since been taken over by the Chemical Bank in New York. We annually get a $100,000 line of credit, but we have still not used it.

About this time we were asked by the Navy if we would attempt to detect the splash down of a missile in the deep ocean. We sent Vema to the Tongue of The Ocean in the Bahamas, and they fired a missile to land about 25 miles away from us. Vema put hydrophones in the water and could detect the splash down of the missiles. This led to a classified project at Bermuda to detect where missiles landed that were fired down range as much as 5000 miles. At first we recommended that they include a small explosive charge as the splash signal was weak. Over the years we have improved the system and now detect the splash signal. We have greatly improved our accuracy and our methods of operation. So far, we have not missed a single missile splash down. The Navy of course knows where they aim them, but the data we give them allows them to know precisely where they land. Thus they can check up on their accuracy. We only know the vicinity where they might land, so that we can be in the right region of the ocean. This has become the main project of our group now.

Andrew Cordier took over the presidency when Dr. Kirk resigned. This was to be an interim arrangement while the University searched for a new President. At his welcoming reception, I invited him to visit Lamont and see our establishment and our operation. He said he would, but he never did get there. I guess he was too busy with the difficult financial position of the University and the ongoing student difficulties.

In late 1969, Doc was invited to the Doherty foundation and was asked to give them a report on Lamont and the work going on. When he was finished they told him they were considering giving Lamont a gift of several million dollars of endowment. Would he give consideration to what Lamont would do with the income of such a fund, assuming the income on the endowment would be of the order of $250,000 per year.

Doc reported this to me and asked for my recommendations. Gerry McCoy, retired from Exxon had been coming in most days and I had found him a good source of wisdom. So, I spent about a day talking with him about what Lamont could do with such an uncommitted income. We finally decided that the Senior Scientists of the Observatory had been considering themselves as secondary citizens since they did not have tenure in the University as the Professors did, so that we would propose that the income be used to guarantee them support when or if they could no longer get a grant or contract support. In those years when they were able to get support, the grant money would be freed to be spent on nascent projects not yet well enough developed to receive Government or corporation support. In times when neither of these needs were urgent, we would propose that the money be used for whatever the Director found urgent but lacked sufficient money for at that time.

I reported a couple of days later to Doc what Jerry and I had come up with and he said he would use it. Doc went back to the Doherty Foundation and asked them to restrict the income to be used in the way described above. They then told him that the endowment would be set up with these conditions. It would be provided a third each year for three years and would amount to $5,ooo,ooo. They further stated that they were unsatisfied with the way Columbia had managed their own endowment funds, so that they would put it in a trust managed by the Chemical Bank. I believe the trust was set up in early 1970.

I pointed out to Doc that we had tried for years to get the Lamont family to provide us with an endowment without success, and that we should show our appreciation for the generous endowment by renaming the observatory as the Lamont-Doherty Geological Observatory., He agreed and it was soon accomplished.

Many years earlier I had started to call the Lamont mansion Lamont Hall. It was picked up by most people and had stuck for that building. The new building on the tennis court was always called the Administration building, the Seismology and Biology building was called the Seismology Building, and the building on the cliff edge was called the Oceanographic Building. The new building for the core storage was called the Core Building. Noone had given any thought of calling these latter other than the name of their uses.

Doc had instigated the start of a small biology program because of out interest in the biological contribution to the sediments that we had been coring ever since the coring on the Mid Atlantic Ridge,supported by The National Geographic Society, just after the end of World War II. The work was started by Dr. Burkholder. He was near retirement age and soon decided to leave and Doc had recruited Oswald Roels to come and carry on the biological work.

Soon after Roels arrived, Sam Gerard and I published our scheme to pump cold water from about 600 fathoms depth ashore and use it to condense water out of the humid ocean air near shore. The idea first came to me while I was on a submarine cruise and we put in for a short shore leave in Nassau in the Bahamas. Since we were to be there for three days I rented a hotel room so that Lynn Shurbet and I could work on our gravity records without cluttering up the Officer's Mess on the submarine, which was being used to entertain shore visitors. It was hot and humid so I obtained a pitcher of ice. It was delivered in a glass tray about 1 inch deep. In a four hour period so much moisture had accumulated on the pitcher and run down into the tray, the tray overflowed. This got me thinking that since the ocean water was only a couple degrees above freezing at a depth of 6-800 fathoms, that if we could bring it ashore and run it through a suitable condenser, fresh water from the air humidity could be accumulated. I discussed this with Sam Gerard and we started thinking in larger terms.

We realized that on seashores near to deep water, such as ocean islands, and continental shores like Florida, a pipe line could be brought ashore and the water caused to flow in a condenser arranged along the seashore so that the condensed water from the atmosphere could be collected in bulk. With a suitably large system, the air flowing ashore through the condenser would also be cooled and a large area behind the condenser would be reduced in temperature, and reduced in humidity. A good place to live!

Sam and I published this scheme in Science. Roels apparently read it and realized that the water at such depths would also greatly be enhanced in nutrients. He organized a project to demonstrate the biological idea proposing to grow clams and oyster in ponds near the shore furnished with the high nutriment water from our condensers. He established a test operation on the Island of St CRoix. With his crew he ran a plastic pipeline about 3 inches in diameter to the deep water and pumped the nutrient rich water into a couple of tanks on shore. He showed that clams, oysters and other shellfish prospered. Their survival rate was much greater than in nature because they were protected from predators. They also matured much faster on the high nutrient diet than in the nearby shallow water. A small percentage of animals did not keep up in growth, and he started to cull these out. He soon was feeding these culls to lobsters instead of just discarding them.. The lobsters prospered. The water from his small diameter pipeline was too warm from the heat picked up in crossing the shallow water area around the island to demonstrate our condensation program.

Roels started looking around for funds to start the scheme on a much larger scale as a demonstration project that the condensation and biology program could be economically feasible. One of the Rockefeller brothers was interested and we met with him and some of his staff to consider a demonstration project. The meeting progressed well and it looked like the funding would become available, until they asked if Oswald, Sam , and I would quit our employment to work full time on the Project. Oswald was willing to, but Sam and I were not because of our other projects that we would have to give up and my commitment to education. As a result, the project was never funded. Roels continued his work at ST CRoix for a number of years.

About 1970 Bill McGill was recruited from the University of California to become the President of Columbia University. At his welcoming party with the faculty, I invited him to come to Lamont to see our fine location and see our operation. He replied that he had seen the Scripps campus and nothing could compare to that and that he didn't need to visit us. I was flabbergasted!

Shortly afterwards I was working at sea on Vema near Puerto Rico when I received a radio message from Doc that Sam Gereard was a Co-Chief Scientist on board the Glomar Challenger, the Deep Sea Drilling ship, and was due in an area just south of Puerto Rico to drill an important hole for leg 9. The data they had in the area wasn't sufficient to make a good choice of a drilling site and ordered me to go there and make a topographic, gravity, magnetic and seismic reflection survey of an are of about 5 miles on a side. The drill ship was expected to reach the site about three days after we could reach it.

We immediately interrupted our ongoing work and went to the area. On the way I organized our technicians so that the survey could continue on a 24 hour basis. I planned that we would make tracks about 1/4 mile apart on north south courses over the 5 n mile wide area. My son Richard who was on board to operate the satellite navigator was to obtain fixes as often as possible from the satellites . I would keep the dead reckoning plot using the satellite fixes to adjust the track to the track made good. The gravity, sounding, magnetic, and reflection technicians would provide me readings at about fifteen minute intervals. I would make a separate plot for each along the adjusted track as quickly as they could be plotted. I would contour each plot as we went along.

After two days in the area we had completed the plan, with none of us getting more than an occasional catnap. I finished the plots about three hours after completing the observations, just in time as the drill ship arrived early. We launched a boat and I took the plots of the data to the drill ship and showed them to their scientists. I also recommended a drilling location. They thanked me and I returned to Vema and our interrupted cruise plan. I never learned where they located that drill hole. I doubt that they appreciated the effort our crew had put into that effort.

On February 13, !970. Bill Bryant and I sailed as co-chief scientists on Leg 10 of Glomar Challenger from Galveston, Texas. This was the start of the second phase of the Deep Sea Drilling Project. Unfortunately Bill Bryant was ill for most of the leg and I had to carry most of the load.

The first hole was no 85 which was drilled at the base of the Campeche Scarp. After a penetration of 212 meters, the drill failed to advance further and we decided that the drill bit had been demolished. This was confirmed when it was recovered, so we replaced it and drilled another hole 85A slightly displaced. The drill became stuck at about 300 meters and could no longer be rotated. Since we did not want to lose most of the drill string, it was decided to blow the advanced section of the drill off with explosives. So the core barrel was recovered and an explosive charge was sent down. When attempts were made to explode it, it misfired. Now we had a problem. We could recover the explosive charge, but a misfire could possibly fire sometime later. We waited an hour for safety reasons. Meanwhile the Ship's Captain, the Drilling Supervisor and I had a meeting to decide what to do. It turned out that none of the drilling crew had had any experience with misfires. I told them that I had had extensive experience firing explosives having fired at least 2000 or more shots, I also told them that I had experience disarming misfires, having done so at least ten times, disarming charge from half a pound of TNT to 250 pound depth charges intended to be fired electrically. I volunteered to undertake the disarming of the charge when it was returned to the surface. I told them that the proper procedure was to minimize the number of people potentially at hazard. Thus the driller would have to be at the drill controls, and I would be at the pipe surface to handle the surfacing charge. All others would have to leave the drill platform until the charge was disarmed. Since it had been intended to fire the charge electrically, I would first cut the wires near the charge and and short them together so that no current of any kind could reach the detonator. I would then have to remove the detonator from the charge. At that point, there would be no further danger and everything could be examined to determine the cause of the misfire.

They decided that they did not want to take the chance of the charge firing while it was at the drilling platform, and they would just reverse the drill torque, unscrewing the drill pipe at some undetermined point, on taking the chance of losing some of the 4000 meters of drill pipe below the drill floor.

This attempt was made and the drill pipe separated about 500 meters above the drill bit. This meant about 200 meters of drill pipe was left above the ocean floor. It undoubtedly could not remain there unsupported and would fall over bending somewhere above the sea floor.

The scientific party speculated on the paleontologist of the future who unearthed this worm(?) 500 meters long bent at nearly a right angle at about 300 meters from one end. Of course there would no longer be any pipe left only a hole of rust in brown sediments.

We drilled 12 more holes to depths of 150 to 900 meters depth at preselected sites designed to evaluate the Gulf of Mexico basin, without further major problems in the remaining two months of our cruise.

Since Bill was ill, I made it a point to be on deck each time a core sample was brought up to the drilling platform. This was necessary, because at that time core recovery could only be obtained at selected depths. The decision had to be made from the results, of the scientists studying the globegerinas and discoasters and the results of the previous geophysical surveys of the sights. This decision had to be made in the half hour after the previous core had reached the deck and preliminary samples had been rushed to the lab for examination beneath the microscopes. This meant that I would often not get more than an hour or two of sleep between core recoveries.

Towards the end of the cruise, as I was eating, I was called to the drilling platform. Since it was not time for a core recovery, I was sure there was some serious problem. After I had climbed the four decks to the drilling platform I found all of the drilling crews and the drilling superintendent waiting for me. They presented me with a medal they had made in the machine shop, making me an honorary Roughneck. The Roughnecks were the very skilled men who operated the equipment on the drill platform to disconnect and connect the drill pipe sections. This was a very dirty and dangerous job and required great skill. I was very flattered with this honor from such hard working men.

On my return to Lamont, I found that Ewing had persuaded a foundation to provide funds for building a geology building on the Lamont Campus. This would allow all of the department to be united at one place. Some graduate classes would still be taught on the Morningside Campus, but the majority would take place at Lamont. We chose architects and I was plunged with DR. Ewing into an intermediary between the architects and the Department. This was a busy time as I was also busy writing up my part of the final descriptions of Leg 10 on Glomar Challenger.

In the summer of 1971, Doc was told that having reached the age of 65, he had to retire as Director of Lamont. Doc and I had talked on several occasions about this happening and I had indicated that I would like to succeed him as Director, and he indicated that he was sure that could be arranged. But this was not to be, we were told that a search committee would be formed to select the next Director.

In view of this imminent change, I thought it would be great to start a tradition of having portraits of each Lamont Director hung on the campus. I contacted Steenland, Press, and Hamilton and asked if they would join me in paying for a portrait of Doc to present to Lamont. We were the group that had originally advised Ewing to remain at Lamont instead of going to MIT. They agreed.

I found a local artist, Jerry Van de Hoef, who agreed to paint Ewing's Portrait from pictures of him that I provided. She knew Doc and admired him very much and she undertook the job. As it turned out, Isabel Steenland's wife made the presentation to Lamont after we had decided to move to Galveston.

Interest on the first installment of the Doherty endowment became available. The authorities at Columbia, who had problems because they had refused for years to invest some of their endowment in stocks, while other Universities had and prospered greatly. Columbia finally took this step just in time for the stock market to recede and their endowment income had considerably decreased. We were told that the Doherty Income would thereafter be used to pay the part of the professors salary of the Lamont group that had been formerly paid by Columbia for their teaching duties. We protested, to no avail so we decided that we had no recourse but to resign since the conditions of the Doherty endowment that we had negotiated would not be met.

The plans for the Geology building had been completed and were ready to go out for bid. With our departure this did no happen, and a new architect and set of plans were obtained. The location of the building was also changed. We had planned for the building to be across the entrance road from the Seismology building. The building was finally located next to the core lab, between the core lab and the new machine shop.

At this time, Doc was approached by the University of Texas, Galveston branch, to form an Institute of Geophysics in Galveston. They would welcome all of the staff and students that would come. Doc called the staff and students together at his house, and explained the offer and invited them to join him and me in that move. After much deliberation and discussion, only Jim Dorman, Gary Latham, and Tosi Matsumoto decided to join us in the move.

On a trip to Washington, while the above debate was going on, Doc and I visited NSF. A scientist in the earth sciences division told us that the earth sciences were well represented on the West Coast and the East Coast, but that there was not good representation on the Gulf Coast. He assured us that such a group would be well supported by NSF. We did not reveal that we were considering a move to the Gulf Coast. As it turned out, NSF support never materialized.

The Galveston Years

I was the first to move to Galveston in June of 1972. The others joined me in July and August. Several months before moving to Galveston I had made a trip to Galveston to arrange details of our move. I brought Dorothy with me so that she could look around for housing. While I was conferring with the staff of the MBI (Marine Biomedical Institute). She started looking around with a local Real Estate Representative. In the evening when we got together she told me that she had seen nothing that was suitable and that she was discouraged. I suggested that the next day she should consider lots and building a suitable house. We agreed to meet at lunch with some of the staff of MBI, having to leave to catch our plane about 2:30 P.M.

When lunch time came, Dorothy did not show up. We finally went ahead with lunch figuring she could catch up when she arrived. We finished lunch and I was getting worried that we would miss our plane when Dorothy showed up.

She told me that she had found a lot on Lake Madeline, a salt water lake, that she thought was suitable and had made a deposit on it. She had also found a builder and told him what kind of house we would need. He had stated that he had a series of plans that might serve our needs and that could save us the cost of an architect if they suited us. She had arranged for him to send us copies of the plans that he thought would do. There was just one thing, I would have to cosign the purchase agreement as they would not sell only to a wife. I cosigned . The MBI rep who was negotiating with me, was astounded that I would sign without examining the property myself. I told him that Dorothy would have to spend more time there than I would, and if she was satisfied, I would be too. We left to catch our plane to New York.

About the end of the week copies of about ten house plans of about the size we needed arrived. We studied them and picked out one that suited both of us. It was a single floor ranch house. What I especially liked was that it was a brick veneer house and that the roof came down on all parts so that any necessary upkeep painting could be accomplished from a step ladder. We suggested only a few minor changes in the plans. The builder indicated that the changes we wanted were ok and would not require redrawing of the building plans as he could accommodate them as the building progressed. He also sent a building contract. On my next visit MBI helped me to arrange a building loan with a local bank and I signed the contract and returned the builder his copy. The house plan had to be submitted to the city planning committee. They objected to one thing only, the garage had a hip roof on the street side. They wanted this changed to a peak roof. I got the feeling that they were making an objection to help the builder. The change in plans would require repricing the construction. This of course would mean there was a part of the building where the eaves were too high too reach with a step ladder. Nothing I said made any difference, we could not get a building permit unless this change was made. Finally the builder asked me to go aside with him. He said he could build it with the change for the same price and that the Planning Board would probably not grant the permit unless I accepted the change. Reluctantly I agreed.

Later the building loan was converted to a mortgage loan when the house was completed several months after we moved to Galveston. It is a little premature here, but while on the subject, I must say the house and its construction turned out thoroughly satisfactory. There was only one thing that we disliked. The front door was in a small alcove in the front of the house, and the concrete tilted slightly towards the house so a puddle developed right in front of the front door when it rained. I soon fixed that by getting some epoxy, like we had used on the deck of the Vema, which was colored almost the same as the concrete. I spread it in the alcove so that there was a slope outward toward the driveway, and there was no more puddle.

The house was thoroughly satisfactory in every way. We used it to entertain visitors to our Lab on frequent occasions and found it adequate. When we sold it , when we left seven years later the investment had almost doubled.

We had two cars and had to plan a way to get them both down to Galveston. Dorothy didn't think she wanted to drive that far, so about two weeks before our move I drove one car down with my sailfish on the roof rack, and arranged with Nelson Steenland, who lived in Houston, to leave the car there until we moved down ourselves. When we arrived, Nelson indicated that I should pull the car into his carport. Shortly after stopping the car, to my great embarrassment, the transmission overflowed on the floor. It had overheated in the high summer temperature in Houston after the long drive down. I asked for a rag to wipe up the spill, but Nelson insisted that his yard man would take care of it, which only increased my embarrassment. MY son Bill had ridden down with me to keep me company.

We returned to New York, and on the appointed day the movers arrived and loaded our furniture for our move. After the furniture left, the three of us drove the other car to Houston towing our travel trailer.
Our house was not finished building, so I had rented a small motel apartment with kitchen facilities for us to live in, for the about tow months it would take to complete the house. The day after our arrival in

Galveston, We went to Houston and picked up the other car, thanked Nelson and Isabel for their help, and returned to our apartment. When the furniture arrived, we had them store it in one of the bedrooms in the motel. It quite filled it up and made our stay crowded . It was mitigated though, since the motel had a swimming pool and Bill and I took care to see that it was not neglected each evening when I returned from work.

My extensive library had been packed in boxes and I had the movers deliver them to the building that we were to inhabit on the campus of the Medical Branch of the U. of Texas at Austin. This was a campus of about ten acres. The building, that we were to initially move into, until a new building could be built for us along the bulkhead of the main harbor, had been emptied so that it could be renovated. For some reason this had taken quite some time, and the building had been used for storage. The second floor rooms on one wing were emptied, painted and had shelves installed for our libraries and offices. These were fair sized rooms about 20 feet square. Doc would have two rooms when he arrived, the rest of us each would have one room, leaving several rooms for additional staff and students that we soon acquired.

The day after settling in our new apartment, I went to the building, where my office was to be, to find the painting of my office was not yet completed. When the painting was done, office furniture that had been discarded elsewhere on the campus was installed to equip our offices, and telephones were installed.

We had expected that ONR and NSF would transfer at least some of the contracts, on which we were principal investigators, to Galveston. That was not to be. I later heard that Columbia University had lobbied that no contracts be moved. That meant that we had no facilities and no contracts or grants for our work. We did have some funds available from the U. of T. at A. for some work.

For the first three months in Galveston I was working on the final drafts of my share of the reports of Leg 10 on the Glomar Challenger. By August, Doc, Jim, Gary and Tosi completed their moves. Doc, Jim and Gary were working on seismograms from the moon. Tosi was working on seismic data from Middle America.

In October I went to the Society of Exploration of Geophysicists meeting at Disneyland in California. My purpose was to let various oil explorartionists know that we were now in Galveston and to hope that I could drum up some support. Nothing special had materialized, but I had my first helicopter flight from the Los Angeles airport to Disneyland. It was the noisiest vehicle that I ever had traveled in, and I had been in several quite large and noisy land vehicles.

Things were looking a little bleak, with very little to work with, no ship to get to sea, and very little money.

I was able to persuade Joel Watkins to join us in Galveston. Soon after, we arranged with Dick Geyer of Texas A. &M to use their research vessel Aliminos for a short cruise. It was equipped with a sounder, a single channel streamer, an air gun source, and a magnetometer. Joel and I decided to locate the Sigsbee Scarp. It had been mentioned in the literature, but no one seemed to know where it was located. We went south from Galveston until we crossed the scarp and followed it westward by zig zagging across it at about 10 mile intervals. It finally terminated at a submarine canyon about southeast of Corpus Christi. This was a canyon which had not been previously reported in the literature, so we called it Perdido Canyon. We made a sufficient survey beyond the Canyon to make sure the Sigsbee Scarp did not continue beyond it. We then returned to our starting point on the Sigsbee Scarp and zigzagged across it to the east and north.. Shortly after resuming our observations we discovered a knoll which we later named Green Knoll after Cecil Green. This was located just beyond the end of Green Canyon, which we also named. The scarp continued to a region near the mouth of the Mississippi Delta where it was buried, but continued beneath the delta sediments. Shortly it bifurcated with the northerly branch dying out in sixty miles beneath the delta. The southerly branch continued to the north and east until it died out at the DeSoto Canyon which lies at the north end of the West Florida Escarpment. There was abundant evidence of slumps, turbidity currents and salt tectonics associated with the Sigsbee Scarp. I made a bad gaffe when I presented a paper about this at a scientific meeting. I failed to credit Texas A&M for the use of their ship. This was called to my attention by Dick Geyer who was in the audience. I apologized as well as I could.

The Marine Biomedical Institute held a meeting of its advisory council in early 1973. The committee consisted of six members. The only ones that I can remember were Cecil Green, retired and a major stockholder in Texas Instruments, and Jimmy Storm, the owner of Storm Drilling Co. During this meeting Doc made a strong plea for the purchase a a former drilling supply vessel of 125 feet that was available at a modest price, because the supply vessels were being replaced by faster and larger vessels. After the meeting Cecil Green agreed to provide money to the MBI so that the vessel could be purchased for our use. We bought the ship and named it Ida Green in honor of Cecil's wife. While it was not comparable to Vema, it at least would let us get to sea in the Gulf of Mexico and the Caribbean. With funds from the University we were able to equip it with a 12 khz. sounder, and a satellite navigator, then available commercially. With this vessel in hand, I hired Arch Roberts, who had extensive experience in ship operations, and Ken Griffiths to run an electronics shop for us. These additions paid off great dividends to the institute, the seismic projects as well as the sea going projects.

With this rudimentary equipment, I went to the Office of Naval Research and persuaded them to give us a small contract to investigate the Campeche Scarp. In my work on Vema on the Sigsbee Knolls, I had obtained strong indications that sizable canyons existed there. I argued that such canyons would be good hiding places for submarines that could quickly cross the Gulf and attack the numerous oil drilling and production platforms off the coasts of Louisiana and Texas, and then return to the canyons again to hide. Detailed knowledge of the topography of the canyons would make them less desirable hiding places. We got the contract and Joel and I and several students, that had joined us at Galveston, spent about a month making detailed soundings of the several major canyons incised in the Campeche Scarp..

We started to assemble a single channel streamer and purchased an air gun from Bolt Associates to increase the capabilities of Ida Green. We also had a former technician that I had known at Lamont,to apply for a job with us and I hired him . He assembled a nuclear resonance magnetometer for us. Joel Watkins was amazed that I knew how to weld together a davit and attach it to the ship so that we could handle the air gun.

About this time Joel and I had gone to a shipyard at Freeport for something about our ship. There was a group from Mobil that were unloading a multi-channel streamer from their ship, because they had a newer and better one for their work. We talked to them and asked what they were going to do with the off loaded streamer. They said they were going to scrap it. We asked if we could have it. They told us that they could not give it to us, but gave us the name of a man at Mobil to call who could give it to us. We quickly returned to Galveston and made the call and were granted our request. We arranged for a crew of workmen from the Medical Branch to go to the shipyard, load the streamer on their stake truck, bring it back and off load it in the ground floor of the building where our offices were located. Doc asked what I was going to do with it, and I replied it would provide us adequate single channel streamers for at least fifty years.

Soon after Texaco called us and told us that they had some equipment that they no longer needed and would we like to have it. I said by all means. They delivered this equipment to our ground floor. It included a Texas Instruments DFS 10000, an obsolete equipment for multi-channel seismic recording with its associated tape recorders.

At about that time there was another meeting of the advisory group. During a lull in the proceedings, Jim Storm told us that he had recently had a blowout at one of his drilling rigs, and the drilling rig had sunk. They had found the mat drifting nearby, a day or so after the blowout. He had spent a good deal of money trying to locate the rig, which he believed to be somewhere in the blowout crater, with no success. Doc told him that he and I had had considerable experience locating shipwrecks in WWII and we could find it for him. He said he would give us the funds equivalent to the cost Of Ida Green if we could find it for him.

The Storm drilling rig was known as a jack-up rig. It consisted of a mat, a steel box in the shape of an equilateral triangle about 200 feet on a side and about 20 feet tall. This mat was attached to three columns of steel, one at each vertex, about ten feet in diameter and about 250 feet tall. The drilling platform was similar in shape to the mat and had three large holes through which the columns penetrated and were attached. The drilling platform could be jacked up on the columns so that it was supported above the water. On the drill platform a drilling derrick about 100 feet tall was mounted on railroad tracks welded to the platform. There was living quarters for the drilling crew, space for drill pipe and other supplies, and machinery to control the drilling, surrounding the rails. The drill platform was essentially a barge. The rig was towed to the desired location, the mat was jacked down until it was on bottom. Then the drill platform was jacked up until it was about 50 feet above the water surface, above the expected wave action. The drill tower was them moved out on the railroad tracks until the drill pipe extended over the side. From this position, the drill pipe was lowered to bottom and drilling was begun.This type of drilling equipment was used in water depths of up to 300 feet.

While drilling, once in a while, the drill would penetrate a large pocket of high pressure gas. The gas would make its way to the surface and would undermine the support of the drill rig and and blow a crater in the bottom. Needless to say drillers do what they can to avoid this, but occasionally it happens anyway. This is what had happened to the Storm platform. The whole rig overturned, the mat separated from the drill platform, the drill derrick rolled off the platform and the platform sank in the crater in the ocean floor. Fortunately the drill crew had been evacuated when tilting of the rig had been detected. The mat was found a few days later floating a number of miles east and had been towed until it could be grounded and anchored in shallow water. Two buoys had been anchored near different edges of the crater for easy reference by searchers.

We had been planning to do some single channel reflection work on the Mississippi Delta, so we added a side trip to see if we could find the lost drill platform.

We arrived in the area during the night and steamed towards one of the buoys, using the satellite navigation to guide us. We were using the radar to try to find the buoys as it was an exceptionally dark night. We saw nothing on the radar. Finally I turned to our captain, Otis Murray, and said "we should be getting very close to one of the buoys now". Just then we heard something hit the hull and roll along the side of the ship We rushed to look and it was indeed one of the buoys. This made a strong believer of Otis in satellite navigation. We made several passes across the crater and then decided that since we would need to navigate relative to the buoys, which did not show up on our radar, we had better wait for daylight. I tuned over the watch to one of our technicians to follow one of the sounding lines we wanted to the south on the delta, planning to return on a parallel course a mile to the east so that we would be back at the buoy at daylight. When I awoke, it was well past daylight and I rushed to the bridge to see what had happened. It turned out that there was a current of about two knots to the south, and it had carried us too far and had taken us extra time to return up-current. We finally reached the buoy about ten o'clock.

The crater was about 1600 feet in diameter and about 40 feet deeper than the bottom depth of 230 feet in the area. The current of about 2 knots running about south, due to one mouth of the River entering the Gulf just to the north, made traversing the crater difficult. We made a number of traverses towing our magnetometer. We observed three targets. One turned out to be piping of another drill hole in the vicinity. We decided to pass over the largest anomaly at slow speed to try to get a recording of it on our sounder. We had great difficulty maintaining our course at slow speed, but we did obtain a target resembling the size of the drill platform and nearby a tall structure with reflectors at 14 foot intervals which we believed to be the drill tower. About 3 P.M. Dr Ewing and Jim Storm came on board from a motor launch, which had brought them out from shore. As they came aboard I asked Jim what on his equipment had several layers about 14 feet apart. He said "How did you know that?" I told him that our sounder showed echoes from such intervals.

We showed the newcomers our records. They asked to see records made on additional passes. We complied and it was decided we had found the drill platform and that we should try to hang up a grapnel in the wreck to mark its location as well as observing bearings from the buoys. We did not have one on board so I welded some bars together after shaping them like a grapnel and we attached this to a rope and started trying to tow it across the wreck. The rope created so much drag that it did not follow the vessel path very well, and was hard to keep the grapnel near bottom or on bottom. We also thought our makeshift grapnel might not snag the wreck very well, so we sent the launch ashore to get us a proper grapnel. Supper was announced as we worked and our visitors and most of the staff went to supper. I told them I would stay and try once more. Right after they left, the grapnel caught the platform and the line was snatched over the side and the 50 gallon drum we had attached to the end of the line nearly took one of the students with it when it went over.

I went down to the supper table and asked them what else they wanted us to do. They couldn't believe we had finally snagged the wreck. We all had our suppers and then returned to the bridge. It was decided to tow one of the marker buoys over adjacent to the wreck as an additional marker of the wreck, which we did.

We had recalled the motor launch when we had hung up the grapnel in the wreck. With it located and marked, it was decided that we should return to our work and Doc and Jim Storm returned to the beach on the motor launch.

We were later told that Storm Drilling had returned to the spot to find the buoy we had moved, and our grapnel marker had disappeared. Using the remaining buoy as guide, they attempted to raise the drill platform. The divers got on the platform, with great difficulty because of the current, and filled it with ping pong balls for buoyancy. Then they tried to wrench it loose from the bottom with a line from a barge mounted derrick. Apparently the suction of the mud in which it had settled was too great and they were unsuccessful. After spending considerable money on the effort, they abandoned it, and so far as I know it is still there.

They did recover the mat returning it to a shipyard after much difficulty. I think they used it for a mat for another drill rig they then had built.

We studied the bottom structure beneath the Mississippi Delta locating a former buried channel which apparently connected to the Athchafalaya river. This had been, in earlier geological time, the mouth of the river.

Having a streamer and a mutichannel recording system in hand, I could not resist the desire to use them. Since none of us knew how to operate the equipment I got Doc to ask Cecil Green to get Texas Instruments, which had made the equipment, to take our recorder and put it in good shape. After a little while, Texas Instruments advised us that it would be easier for them to furnish us a recorder that they had already put in operating shape than to bring the one we had into good condition. We readily agreed and asked how we could learn how to operate it. They said that there was no operating manual but that they would give two of us a weeks direction to learn its operation. We made a date and Arch Roberts and I went to Texas Instruments for indoctrination.

Our instructor at TI told us that we had learned the operation of the gear faster than anyone that he had previously instructed. At the end of the week. he helped us load the equipment on our truck and we brought it to Galveston.

Among the equipment we had received from Texaco was a streamer winch they had discarded when they had moved to a longer streamer with a larger number of channels., and a streamer winch. We had this winch mounted on the stern of the Ida Green and and Arch and I loaded our streamer on it. Meanwhile I had purchased another air gun from Bolt. We had to mount a second davit over the stern that could lower the gun into the water behind the vessel.

Having had no experience with a long streamer and having been told that it was difficult to adjust it to tow at a proper depth, I was worried about the next step. Charlie Kershaw, the technician I had hired to get our magnetometer working told us that he had experience with streamers with a geophysical prospecting group that he had once worked for, so we got him to tell us what we needed to get to make our streamer work. He advised that we get some of the oil that was used in the streamer since there was always spillage when working with the streamer. He also advised us to buy sheet lead, as it would be needed to adjust the streamer to neutral density, and to get lots of friction tape, as it was needed to streamline the lead sheet when wrapped around the sections of streamer.

The streamer was made in sections. Each section was one receiving unit, and was made up of about twenty individual hydrophores, the hydrophone were mounted in series and connected to connectors at each end of the 100 meter long section. There were also a number of wire pairs in each section connected to the connectors at each end. These provided for connections to sections farther along the streamer cable. At the front end we had a leader about 500 meters long that allowed the streamer to tow far enough astern to avoid most of the ship noise. Each section was meant to be able to be used at any position along the streamer cable. At the tail end a 200 meter rope connected to a stern buoy was meant to keep the streamer stretched out straight behind the ship. Of course, since the ship would be moving only at about two knots, to minimize self noise and towing noise, the streamer was normally at a small angle with the axis of the ship depending on the effects on the ship of wind and current. We also had been given about twenty so-called "birds", which were devices streamlined to attach to the cable, with pressure units controlling fins, which would control the depth of a portion of the streamer.These had been given to us by another oil company. They had upgraded their birds so that the depth control could be changed from shipboard.

Finally we were ready, and we put to sea with high hopes. We headed south from Galveston until we reached the 20 fathom curve. We spent about four days of putting the streamer out and recovering it, adding weights, or removing them until we got the streamer to tow reasonably at a depth of about 10 meters. Finally we got under way shooting the air guns at about 10 second intervals. We recorded the 12 channels of our streamer on magnetic tape. We made a single channel record immediately visible to us on the ship as we progressed. We traveled on a course of about 200 deg. until we had passed the Sigsbee Scarp by about 30 miles. We then turned east about 40 miles and headed back toward our point of departure on a course of about 340 degrees. Thus we had a v showed track from about the 20 fathom curve to a depth of about 1500 fathoms. We returned to port, knowing that our single channel record looked very good and hoping our multi-channel results would be a great improvement. We did not have the means to translate the 12 channel tapes into visible records, so we arranged for a commercial company to make profiles of our v shaped transects. When these returned we were very pleased with the results and I rushed to show Doc the transects. He, too, was very happy with the results. Now we could plan a program to determine the structure of the Gulf of Mexico Basin!!

A few nights later, at about 2 a.m. after I had gone to bed, I heard the doorbell wring. I answered it. Harriet Ewing, Doc's wife was there and said Doc had had a stroke, and that it would take too long for an ambulance to come to get him, would I help her get him in the car and take him to the hospital. Doc had bought the house across the street from me. This was not planned, it just happened. I threw some clothes on and went across the street. Doc was in a coma , laying on the floor of the bathroom. Harriet and I had trouble getting him out of the bath room because he was so big and heavy. We finally got him on a blanket which we dragged out to my car where we loaded him into the back seat. Harriet got in back cradling his head in her lap. I drove the five miles to the hospital at a speed of 70 mph. Harriet had called ahead and the emergency room people were waiting for our arrival at the Emergency Entrance. They did what they could for him and installed him in the intensive care section. He never regained consciousness, and after five days died. Frank Mongelli, executive Manager at PGI , where Doc was President and I was Treasurer at the time, came down and stayed at our house until after the funeral.

There was a funeral at a large church in Galveston. It was planned to have a major service in the cometary in Sparkill where Doc was to be buried. I expected to be asked to give the obituary since I had been working with Doc longer than anyone else. But, I wasn't. Frank Press was asked instead. At the time I felt bad about this, but Frank gave such an exceptionally fine obituary I was glad it had worked out that way. There were about a hundred people in attendance.

About a week later a grave side service was held in the cometary at Sparkill.It was attended by about a hundred people, mostly colleagues, many of whom had come from Washington for it. Doc was buried at the top of the hill where there was an excellent view of the Hudson River to the northeast. At least he had had a chance to see the first multi channel seismic section of the deep Golf of Mexico.

Just before his death. Doc had been engaged in negotiations for the establishment of an Institute of Marine Sciences at the University in Austin. The Marine Biological Station at Port Aransas was to be one branch, and the Institute of Geophysics in Galveston was to be the other, with the headquarters at Austin. After the funeral, Lorene Rogers, President of the University of Texas at Austin, asked me to show her the new building that was just nearing completion alongside the wharf where Ida Green was docked. When we had finished the tour, she asked me if I would carry on the work as the Acting Head. I agreed.

While, Frank Mongelli was still in Galveston, I placed a call to Bob Ewing who I knew was retiring from the Navy with the rank of Captain. He had taken a graduate course in business administration while in the Navy. I asked him to come to Galveston to serve as our business manager. At about this time I also recruited Dick Buffler and Jean Schaub to help with the multi channel work. I knew it would be too much for Joel and I to do by ourselves.

About this time NASA canceled the communication link with the moon. This shut down the data coming in from the moon seismometers. As a result, Gary Latham started to design an ocean bottom seismometer. Jim established a number of unattended seismic stations, powered by solar cells, at various locations in Texas.

In our early days in Galveston, Doc, JIm, Gary and I made a pilgrimage to west Texas where the university operated an astronomical observatory. Doc noticed that the large telescope was merely setting on concrete pedestals, and asked why they didn't have any fastenings. Thai responded that it weighed so much there was no chance that it would move. Doc then asked if they knew that early in the century there had been an earthquake in the Rio Grande valley. He told them a similar earthquake could make significant horizontal motions of the telescope base. On a later visit, Jim noticed they had added sizable clamps connecting the telescope base to the pedestals.

With the first multi channel reflections in hand we started to get visits from geophysicists from the various oil companies. After talking to a number of them, I decided to start an Industrial Associates program. We had charged each associate $50,000 a year at Lamont, but since we had much less data to offer I decided that we could only ask for $20,000 per year. We soon had signed up four, and I hired a person to provide the Associates with the services they requested. This was often copies of the processed multi channel reflection profiles, navigation data, soundings, 3.5 khz soundings, and magnetic data. We had acquired a 35 mm film processing equipment from the gifts of discarded equipment from one of the oil companies. This could process 100 feet of film at a time. I learned how to use the equipment and trained our Associates Server in its use. He would photocopy all the data except the multi channel, for a cruise, and develop them. When an oil company wonted them he would have a copy made for them. The multi channel data we provided in profiles on 18 inch wide paper, which would cover a single profile. Sometimes these would be 15 or twenty feet long. We had them reproduced commercially. A geoscientist of an Associate would telephone our server and indicate what data he wanted, and our server would make the appropriate copies and send them. Sometimes they would call and want to visit to look over some of our data , before requesting copies. Our server made the appointment and made sure the data was handy when their geoscientist arrived. We had two meetings a year with the Associates at which we described the various research going on in the whole group, and asked for any advice they had that would improve our operations. We were surprised at the other data that we were acquiring for which the Associates wanted copies. They were especially interested in the ocean bottom seismometer that Gary was developing as well as the multichannel data.

I went to Washington and tried to get a contract from ONR or a grant from NSF for multichannel work covering the whole Gulf of Mexicco Basin. While they were interested, since we were the only lab in the country equipped to get multi-channel data, they would not finance our work. I later found out this was because they did not want to add the support of Ida Green to the oceanographic ships they were already supporting, and they felt that supporting thus program would commit them to that.

Joel and I decided to start the program anyway, with some funds we received from the University. We made a number of profiles in the Basin west of Galveston. These were sections from Campeche Peninsula to the mainland of Mexico to the west, along the axis of the Gulf of Campeche, and a couple between the Campeche Peninsula of Mexico and Texas . We had these processed commercially. When the profiles became available, our Industrial Associates rapidly grew and we soon had twenty members. The multi-channel sections excited them because they could see that there was up to 5 km of sediments in the deep basin where we had worked.

At an Associates meeting I told them that we wanted to chart the rest of the basin, but did not have any money, and requested that they provide funds so that we could proceed. They agreed, and we soon had acquired a number of sections from Campeche to Texas and the Gulf coast to Florida, and a couple of sections east-west parallel to the axis of the main basin. At the next meeting we suggested that they provide the funds so that we could have the data converted into geological sections. It made no sense for them to do the reductions twenty times, when it could be done once. None of them were willing to have one of them do the data reductions, as it would give that company an advantage over the rest, so they gave us the funds and we had them processed and displayed the sections at the next meeting. One representative told me that these sections were the best he had ever seen produced from the ancient equipment that we were using.

Meanwhile, the University had hired Creighton Burke to head the Marine Science Institute at Austin, with the Biological Institute near Corpus Christi and the Geophysical Laboratory at Galveston as two divisions. I had known Creighton for a number of years. He had been a head geologist for Mobil and for a long time he had been Mobil's representative at Lamont. He and Hollis Hedberg of Gulf Oil, had persuaded their companies were the first to sign on to the Industrial Associates at Lamont. The other's soon followed, as oil companies are always worried that other companies are getting an advantage over them.

Creighton was very enthusiastic about our multi-channel work. He also was impressed with Gary's OBS's, because they were less than half as expensive to build as those built by WHOI, LDEO and Scripps. He thought Jim's work in Texas with remote stations reporting in by telephone lines, was very exciting and Tosi's work with Honduras to set up a seismic monitoring and alert system for earthquake surveillance, in that earthquake prone area, was also important.

Tosi had had to set up a system very economically in a country without any experience and very little infrastructure. In order to do that he had to acquire all of the equipment and get it all working at Galveston, and then ship it to Honduras and reassemble it without spending much on travel, or in Honduras. I had suggested to him that he use my 15 foot Bee travel trailer to set up the equipment and get it in working order at Galveston, and then shipping the system, still in working order, to Honduras . There it could be set up in an area near a telephone line, so the data could be monitored at Tegucigalpa, the capitol city. I had been thinking of getting another trailer anyway, so I donated it to Galveston Geophysical Laboratory, taking a deduction from my taxes for the donation, which was almost as large as I could have received by its sale. This worked well and Tosi got them set up with a single trip there. He trained them in its operation and interpretation. For quite a while he supervised their operation and answered questions and gave advice until they felt comfortable. A couple of other Central American countries then approached him for similar help.

Soon after Doc had died, the new building was completed. The Vetlesen foundation had arranged for a noted artist to paint a montage of scenes of Doc's research life on two large murals. They were painted on 4 by 8 foot sheets of plywood. Each mural was 12 feet wide, and 24 feet high. The Atrium of the new building had been designed so that each mural was mounted on two facing Atrium walls. When these had been mounted I set up a dedication of the building, which included naming it Maurice Ewing Hall. I got Hollis Hedberg to be the featured speaker, President Rogers spoke, and I Identified each of the pictures on the montage with the operation that they memorialized. (Since the Institute of Geophysics, the present name of the successor group, abandoned the building about 1983, and moved to Austin, the murals have been dismounted and put in storage, as the Institute is now in a rented building).

After the dedication, we all moved into the new building. We had requested the telephone company to install the telephones before we moved. We completed the move, and the phones still hadn't been installed. Each day for about a week I called the Telephone Company and asked when the phones would be installed. They kept answering "soon". Finally I got mad and went to the town library and looked up the name of the president of Southwestern Bell. I called his office, and his secretary answered. I told her I wanted to talk to the President, whose name I then knew. She insisted on knowing the subject, and I kept insisting I wanted to talk to him. Finally I told her that we were getting the runaround on getting our telephones installed, and that they were an important part of our business which was stalled by their lack. The next day, our telephones were installed.

Creighton was on board at the next advisory committee meeting of the Marine Biomedical Group, since the new structure of the Marine Science Institute hadn't yet been set up. After the meeting, he and I invited Jimmie Storm to visit our new building and then to have lunch with us. Over lunch we asked him if he would fund us for establishment of our own computer room, primarily for the reduction of the multi-channel seismic data. It would cost about $10,000 to buy the computers, have them installed and have adequate air conditioning installed to keep them at the proper temperature. Since he had not fulfilled his pledge of "enough money to buy the Ida Green, if we found his sunken drill platform" he agreed providing that we did not publicize his support. We soon had our computer department running . This was not only important for our multi-channel work, but Jim, Gary, Tosi and the rest of our staff made good use of it.

At the next Industrial Associates meeting we proposed that we make grids across the Sigsbee Escarpemt, the Campache Escarpment,

the Florida Escapement, and the ridges off the west coast of Mexico, which we soon called the Mexican Ridges, and connecting traverses between these grids to complete a framework of the Gulf Of Mexico.
These grids were to be made up of five lines 70 miles long, about 5 miles apart, perpendicular to the feature, followed by five lines 5 miles apart parallel to the feature across the previous lines. This would provide a three dimensional view of these complex features. I explained our plan to the Associates representatives and asked for their funding of the work telling them that we were now set up to reduce the data ourselves. I asked for their support and any ideas that would improve our plan. After a couple minutes of silence, Creighton got up and supported my plan and and called on particular Associates representatives to comment on it. A lively discussion ensued. Several good suggestions which we incorporated in our plan were made, and the result was that they would go back to their companies and urge their support. Within a few days we had three companies that had subscribed their support, and the rest soon followed. I believe that Creighton supplied the spark that got this plan accepted. We went to work and gathered our supplies and spent a couple months completing the sea observations.

Joel Watkins and Mark Houston, who had recently joined our staff spent the next couple of months getting the computer reductions going and computing the seismic sections. Dick Buffler and Jean Schaub joined in with interpretation of the data. If I remember correctly a series of papers resulted from this work. The Associates were excited when we showed them the sections at the next IA meeting, while we were working on the publications.

When the Marine Science Institute was set up in Austin, with Creighton as the head, I was appointed the Director of the Galveston Geophysics Laboratory at Galveston.

In our work with the multi-channel work, the ship maintaining q speed of only two knots often rolled excessively since our course had to be maintained despite the direction of the wind, currents and waves to obtain the data we wanted. I couldn't see how to include anti-rolling tanks on R.V.Ida Green, so I searched around for another solution. Along the Gulf Coast, shrimp boats had adapted a system of "flopper stoppers" to reduce their rolling, so I decided to try them. I got the Marine Architect, that we had used on Vema, to design "flopper stoppers" of the correct size for our vessel, which was larger than most of the shrimpers. This involved mounting a mast near the rail on each side near the midpoint of the vessel. About twenty foot long booms were attached above rail height that could be lowered to a horizontal position. Beneath the end of each boom a wire rope suspended a "fish" into the water about ten feet deep when the ship was on an even keel. The fish were attached by a bridal to the wire ropes, had a heavy weight at the bow, a large horizontal fin at the back, and were shaped so that they towed with little resistance in the forward direction. When the ship rolled they were shaped to provide great resistance to rising through the water. This resistance would reduce the rolling by about 50%. These only had to be used on certain courses when the ship was somewhat broadside to the waves. After using them awhile we found the masts were causing some deck warping and we had to have heavy doublers installed beneath the masts.

Bill Behrens, a geologist had been on the staff of the Marine Biological Laboratory at Port Aransas our sister institution in the Marine Science Institute under the direction of Creighton Burke. Creighton arranged for Bill to join us at Galveston, since we had similar interests. Bill was interested in obtaining core samples from the bottom. Archie and I started to try to accommodate his needs. Several copies of the coring winch we had made for Vema had been made for the Navy for a special project. The winches had been installed on several charter vessels. When the project was over, the winches had been sold to second hand equipment dealers. We tried to find one of them without success. Archie, who had worked for one of the groups that had acquired one of the winches knew where a set of plans was located, so we sent and obtained a copy. Archie and I were going over the plans to make sure that they were complete, when I got a call from our marine architect that he had discovered one of the missing winches in a scrap yard in Baltimore. We acquired the winch at a price that was only about 10 % of the price we would have had to pay to have one fabricated. I had our architect design an A frame for the midships area and plan the deck stiffening beneath the winch mounting area, and the A-frame. These were soon added to the ship and we were equipped to do bottom work in deep water. We also added a bin, about 10 ft. by 10 ft. by 20 ft inside, to act as storage area for the bottom cores. This was mounted so that it could be removed if it were not going to be used on a cruise. This never happened, though, as we found the storage area useful in all our work.

We had great difficulty since the University would not let us have a petty cash account for small purchases needed to get the ship to sea. They insisted that everything be bought by purchase order through Austin. A ship cannot be run that way, but I could not convince them of that. There are often last minute small items that must be bought, or last minute small repairs to be made, before the start of a cruise.

On one leg of our multi-channel survey in the Gulf, one of the Associates asked us to take four of their new staff with us to see how multi-channel work was accomplished. They spent two weeks with us, helped us, and caused us no bother. We had a severe storm that came up and caught us with the streamer out. It was too rough to recover the streamer, so we had to ride the storm out with the streamer deployed. We had to go where the storm took us, as we could not do anything but run before it maintaining steerage way. Since the streamer was out, I kept the air gun working and surprisingly got data only disturbed by noise about 10 % of the time. At the end of the cruise the four men came to me to pay for their meals. I told them that was not necessary, as they had helped us in our work. Nevertheless they insisted on giving me travelers checks of $100 apiece, made out to the Lab. I got together with Bob Ewing and we decided that we would establish a petty cash fund of $400. This was to be used for small urgent purchases. When a few had accumulated the receipts were sent to Austin for reimbursement, and the petty cash fund was renewed. Probably Austin would not have approved, but we didn't burden them with that part of our operation.

Another place where we had trouble with the University not understanding the complications of ship operations, was with scientists and technicians traveling to other ports to join the ship. Austin would not provide travel advances. This was not too hard for the scientists, who could usually buy their passage on credit cards, but often caused them difficulties since the travel voucher could not be sent in for payment until their return and often payment was delayed beyond the date they had to pay their credit card bills. The technicians in general could not get credit cards, so this was especially hard for them. I persuaded a donor to provide us a fund of $5000 specifically for travel advances for our staff. This solved that problem for us as the refund was repaid after the travel voucher was finally paid, and the money could be recycled. Other parts of the University were not happy that our staff could get travel advances and they could not. We also made this option available to the non-seagoing members of our group.

We knew when we first got the ship that we would have trouble with the University since they had no experience in ship operations. They would find working with ship chandlers, port officials and other necessary ship operations would not fit their purchasing system. So we hired a Texas Corporation to handle ship operations, billing the University once a month instead of a myriad of small and not so small bills, with no opportunity to get several bids. This did not work out either, because when for instance a cook quit as we were just about to sail from an outport on a Saturday, we could not get anyone at the corporation, so that they could get us a replacement immediately. We had to wait running up ship costs to noon Monday to replace him. As a result I got Palisades Geophysical Institute to handle ship operations. This non-profit corporation, of which I was President served us well, and was available to me, as President, every day.

With the installation of the trawl winch for Bill Behrens work, we were now able to make all the observations we had been able to make on Vema except gravity measurements, which I had initiated. At a Society of Exploration Geophysicists meeting I had heard a paper by Walter Munk which involved the use of an accelerometer developed for him by a consultant in Silicon Valley. I felt sure that this same technique could be used to make gravity measurements on Ida Green. At Lamont, we had to develop an apparatus to make corrections for cross coupling. Cross coupling are vertical effects on the measurement system caused by horizontal accelerations because of details in the construction of the instrument. It was well known that by summing two identical instruments oriented at 180 degrees to each other, the cross coupling could be eliminated. This had never been done, because noone believed they could make two identical instruments, so that some cross coupling effects would still exist. I believed that the instrument Munk had described was so simple, that identical copies could be made. I talked to the man who had developed the instrument and he convinced me that it could be done for less than $10,000. By this time sea gravimeters were costing about $200,000 when produced commercially, I had no idea where we could get that amount of money so that we could add gravity to our sea measurements.

I decided that I would have to find about $25,000 somehow and give it a try. I applied a number of places without much success. Finally I asked Cecil Green for help. After much dithering, he made $25000 available to me. I got Austin to make a contract with the accelerometer designer to develop the gravimeter for $10,000. Meanwhile, I bought a gyro-stabilized platform, by then available commercially for $10,000. After about a year, the designer said he needed some money for supplies, and I persuaded the University to make an advance payment of $3000 . After about another six months he called and implied he had the instrument nearly completed. I took Ken Griffiths and we went to Silicon Valley to pick up the instrument and to learn about its operation. When we got there we found out that there was no instrument ready and that he wanted more money. I assured him that we could not advance him anymore until we could get delivery of the instrument. He was reluctant but finally assured me that thus would happen soon. After another six months, I called him and asked what was happening. He said he could not complete the instrument without more money. When I reported to The University that he would never complete the instrument. They had their Lawyer try to recover the advance payment. After some time the matter was dropped and we were out the $3000 and I had nothing to show for it.

I had read about another accelerometer commercially available for about $1000. I obtained all the information I could and became convinced that if two of these instruments could be placed back to back and enclosed in a temperature controlled box, they could become a suitable gravity meter, so I decided to try to make one myself. I went to the company in California and obtained two well matched accelerometers. I mounted them back to back and decided to test them at sea before, worrying about enclosing them in a temperature controlled box. John Kuo had come to Galveston on his sabbatical leave and he went with us on a cruise in the Gilf primarily for multi channel measurements. John and I took the accelerometers on the gyro stabilized platform and made observations during the cruise. The results looked hopeful, but there was a small residual cross coupling, which we attributed to an imperfect alignment of the two units.

I became too busy with the multi channel work and the lab Directors duties, so that I never had time to work further on it before I left Galveston. This is the closest to a failure that I experienced in my career, and I believe had I been able to spend more time at it I could have succeeded.

Keeping Ida Green working at sea was taking most of my time in this period. I managed to get arrangements with an oil company to make multi channel measurements in the vicinity of the Virgin Isalnds. They wanted to see if there were significant accumulations of sediments there and if there were any structures conducive to oil accumulation before starting an exploration program offshore there. We agreed to a month long program for that purpose so long as we could publish our results. This was arranged and Joel went as chief scientist, since I had matters at

Galveston that could not wait. After they had been working about two weeks, the streamer got tangled in a coral head and broke off, near the ship. I got the radio message about this and that their efforts to recover the streamer had not succeeded thus far.

We had another streamer in the yard, which was not as good as the lost one, so I radioed them to come in to Fort Lauderdale, and I would load the streamer in a truck and meet them there on their arrival. We could load the streamer on board and they could return and complete the program.

I hired one of the largest U-haul trucks and loaded the streamer in it. It made a pile about three feet deep. but it was obvious that the truck was badly overloaded. I hadn't;t realized how much all that oil, copper wire and multiple hydrophones weighed until I sat down and calculated the total weight. We had to hire a second truck, back it up to the first to transfer half the streamer into the second truck and break the streamer at a joint near the middle. This meant that I had to get Archie to drive the second truck. We had a couple of summer students at the lab and I had one accompany each of us on the trucks, so that in case of a breakdown one could stay with the truck while the second could go for help. I was afraid we would get separated en route.

We arrived at Fort Lauderdale and the four of us unloaded the two trucks and flaked the streamer down on the ramp at the end of the dock where Ida Green was to moor. We reconnected the two halves, just in time for the arrival of Ida Green about 1 P.M. I had expected to have the scientific crew on the ship to load the streamer on Ida Green but they refused. They had been at sea about three and a half weeks, and were determined to get some shore leave. Joel convinced me not to make an issue of it. So Arch and I and the two students loaded the streamer on the winch and made the necessary connections, finishing about 8 P.M. It was fortunate that we had the two students with us, as Arch and I never could have done the job without help. I had expected the ship to leave immediately, but there was no crew nor any scientists to man the ship, so we had to wait till the next day for them to sail.

They returned to the Virgin Islands and concluded the planned work. About a week later they came into our dock at Galveston with the lost streamer flaked in a temporary bin on the deck. Joel had returned to where they had lost the streamer, fortunately in shallow water, and had dredged the streamer up and everybody helped bring it aboard hand over hand and flake it down. We were glad to get the streamer back but we then had to remove the spare from Ida Green and return it to our yard . We had to repair the damaged leader and replace the lost streamer on the stern winch. That too was al lot of work.

I went to a meeting in Tegucigalpa and presented some of our multi-channel results in the Gulf of Mexico . Most of the participants spoke Spanish at the meeting, which I could not. One of the scientists there, was from Guatemela who spoke good English and whom I had met at several international scientific meetings. I asked him if he would tell the other participants at the meeting that we would be glad to make multi-channel observations off their coasts if they could fund it. They would not have to fund us for the transit from Texas, if we could make observations for several countries on a single trip. He explained my offer in Spanish but we got no response. Guatemela was having internal troubles at that time and obviously could not participate. I wondered whether he had presented my offer in such a way as to discourage the others so they would not get some advantage over Guatemela. I will never know. I went home disappointed.

Since I could not get any funding from NSF or ONR for our ship operations even though we were the only research Institution in the U.S. able to make multi-channel observations, I was desperately looking elsewhere for funding. At one meeting at Lamont I offered to make our ship and equipment available to the other Institutions to make multi-channel observations. I requested only that one of our staff participate in the observations (I wanted to make sure that noone made any electronic changes to our equipment), that the data reduction be done cooperatively at our computer lab (since we had the necessary programs to handle data in the format that our data was taken) and that our representative be included as an an author in the publications that resulted from the program (since our representative would need to get something for his participation). I got no takers.

Much later I was approached by scientists from the Mexican and the Costa Rican governments to make some observations off their Pacific Coast in hopes that the results would get oil companies interested in prospecting there. I was glad to agree, and we set up a program of about six profiles, four off Mexico, and two off Guatemela , from as near shore as we could safely navigate offshore as far as the Mid-America Trench axis. A Mexican scientist would accompany us off their coast, and a Guatemela scientist for the two profiles off their coast. Both would be able to speak English.

We made a quick passage to Mazatlan where we picked up the Mexican scientist. We spent a couple of days refueling and revictualing, and the crew and scientists quickly sought out many of the tourist things. We had been tied up only about two hours when one of the scientific party, Tom Shipley, at that time a student, returned to the ship reporting that his pocket had been picked on his first bus ride, and he had lost his wallet and all his money. We loaned him some money and he went out on the town again, perhaps a little wiser. In the bay on which Mazatlan was located there were a number of motorboats which towed large kites which would lift a moan to a height of about 200 feet. When they slowed down the kite descended and the man was returned to the water. A ride cost about $50 as I remember it. Our scientific crew decided they would raise the necessary fee if I would go for such a ride. When I agreed that it was a good idea, they quickly lost their enthusiasm and the idea was dropped.

About 3 a.m. the next morning, I awoke in my cabin on the upper deck of R.V.Ida Green to see a hand and arm extend from the door towards my pants hanging on a hook. only half awake, I hollered and jumped out of bed. The thief ran down the companionway with me following about 30 feet behind. In the main cabin he hurdled the mate, who was supposed to be on watch but was asleep on the deck. I lost sight of the thief when he reached the open deck and I presumed he had disappeared onto the dock among the numerous large crates waiting there for shipment overseas. I waked Captain Otis Murray and told him what had happened. We then searched the whole ship in case the thief had merely hidden on board, but we did not find him. I guess it was lucky that I lost the twenty dollars in my pants pocket though, as when we searched the engine room we found water over the floor boards, an obvious leak. Otis and I searched and found that the outboard valve of the waste heat evaporator, that had been recently installed, had not been shut off when the engines were stopped as it should have been. If I had not been awakened, it is probable the ship would have sunk at the dock. We secured the valve and pumped the ship dry. Otis tried to wake up the mate and found that he was passed out drunk. He sent him home and obtained a replacement.

Finally we were ready and started work offshore . We had only made a little progress on Friday afternoon, when a Mexican Gunboat arrived where we were working , with armed men on deck and their deck gun manned and ordered us to stop. Our Mexican representative informed them that we would stop making observations, but that if we stopped the ship the long cable astern would sink and be lost. They allowed us to continue at our

2 knots while they boarded us. An officer with an armed guard came on board and our Mexican representative told them that we were working with a Mexican Government Agency and gave them the name of the Government Agency, and the man in charge of our program. They radioed their headquarters this information. When their headquarters tried to confirm it, they found that the man in the agency who knew about the operation had left for the weekend. They told us we would have to stop and accompany them back into Mazatlan, until they could contact the appropriate individual. We told them it would take us several hours to recover our towed cable. They said they would wait. They did and kept armed guards watching our every move. When we returned to Mazatlan, they stationed an armed guard on the vessel and noone was allowed on shore. It took all of Monday for them to satisfy themselves that we were working with the Mexican Government. They removed the armed guard late in the day, and gave us permission to continue our work. We sailed in the early morning and resumed our observations, having lost over four days with a boring dock side stay of three days. We had no further trouble and completed our observations.

We returned our Mexican observer to Mazatlan and picked up an observer from Costa Rica. We sailed to the lines in Costa Rica and quickly completed our planned observations. When we were finished we returned their observer to San Jose in Guatemela and started our return to Texas.

As we made our way south towards the Panama Canal we encountered numerous sea turtles about 3 feet in diameter all swimming northward, toward the shore.. We must have seen at least a hundred. This was in the spring of the year. None of us had ever seen anything like that before.

All hands were glad to get home to Texas, after our voyage of a little more than two months.

Shortly after our return to Texas, Creighton convinced me that we should be teaching a course in Austin. I decided the best strategy would be to give the first course myself so that it would be hard for the other professors to refuse to do what I had already done. So I planned a course in geophysics that would be given for two hours once a week. It turned out that the only time a two hour course could be given that would not interfere with other geology courses was late in the afternoon. I soon settled on leaving Galveston early in the morning on Tuesdays (I think) arriving in Austin just before lunch. From 1 to 3 i would make finale preparations for the days lesson, present the course from 3 to 5, Start for Galveston, stopping for some fast food on the way, and reaching

Galveston about eleven. I found it was hard for me to take time to prepare my lesson beforehand, so I started driving to Austin Monday night. The next morning I would prepare my lesson, have lunch locally with Creighton, confer with him about our various programs until time for my course, give the lesson and then return to Galveston. The students seemed to like this arrangement and did quite well, but I found it rather exhausting with all the other things I was doing. I did this for the whole of a fall term, then requested Jim or Gary to do something for the spring term. It turned out that they had commitments that would not permit them to do it, but they promised that one of them would do it in following fall term. The following events resulted in no further courses.

Early in the spring term, tragedy struck. Creighton was working in his office after the others had left for the day, when he had an aneurysm of the brain. He was found collapsed on the floor of his office,and was rushed to the hospital. The doctors told his wife they would have to operate to remove a section of the bad vessel and reattach the vessel. However it was likely that much brain damage had been done and they did not know what he would be like when he recovered. If not operated on quickly, he would certainly die. She decided to have the operation done and it took most of the spring term for him to recover his health. It turned out that his memory of things in the past had not been damaged much, but he had no short term memory and did not seem to be aware of what was going on around him, so that he could not continue in his job. Pete Flawn, a senior geologist had been installed as interim director while Creighton was recovering and continued in that capacity while a search was made for a new director, after it became obvious that Creighton could not continue as director.

We decided to make a multi-channel transect from Florida across the Blake Plateau to the deep Atlantic Basin that spring. Since I had to remain at Galveston, Joel Watkins planned the operation, directed the sea work and supervised all the data reduction and the publication of the results. This turned out to be a very important project and provided a very interesting section. Joel convinced the AAPG to publish the paper and to provide a separate accompanying multi-channel section and a geological interpretation. This section was about eighteen inches wide and about 10 feet long. In all, it was a masterful piece of work. I don't remember how it was financed. Even the oil companies in our Industrial Associates were greatly impressed.

An independent oil company came to visit me and asked if we could do a reconnaissance survey near St Lucia, Barbados , Granada and Trinidad in the Caribbean windward islands. They wanted to see if there were good oil prospects in those areas. I agreed that we could do that if they would finance the trip from Galveston to the area and return as well as in the working area. I told them it normally took us about 2 to 3 months after completing the work and returning the data to Galveston to produce thee reduced multi-channel sections, when we could give them copies, and that it normally took us about another two years to publish the results. We could not undertake any work that included any prohibition on publication. They agreed to this and funded the project. They wanted two of their people to accompany us on the ship while we were taking the data to which I agreed. Their two representatives and I flew to Barbados in their Lear Jet to join the ship at the start of the profiling.

Gary had worked on the Ocean Bottom Seismometers (OBS) at Lamont, and had decided that by starting over he could build some at a much reduced cost, which he did. On the ship's return from the windward islands we sent the ship into the middle of the Gulf of Mexico to shoot refraction profiles from OBS's and accompanying multi-channel profiles for comparison. An OBS was located in the western Gulf, and another OBS was located about 60 miles east of it. The ship started making multi channel profiles about another 30 miles east back toward the line of the OBS's firing explosive charges at intervals to supplement the sounds from the four air guns which we were now using for the MC work. The profile was continued to about thirty miles beyond the westerly OBS. Then the ship returned and recovered the OBS"s by signaling them to release their bottom anchors. This was a great success and produced a good reversed refraction section accompanied by the mutichannel section along the same transect. When reported to the Industrial Associates, it excited them and most of them were soon including OBS 's into their programs. Gary's OBS's were about a third of the cost of those being built by the other oceanographic institutions.

A group of institutions got together and planned a program of multi-channel and OBS research in the Pacific to the west of Mexico. I don't remember all of them, but I do remember Scripps, LDGO, WHOI and we were part of it. Together we would be able to deploy about 100 OBS's and we could supplement the work with multi-channel profiles.. Cores and dredging were also to be included. A program of explosive shots were needed for recoding at all the bottom locations. I figured that we could turn the van we had placed on Ida Green's deck to store bottom cores into a magazine that could carry the explosives, so we volunteered to carry the explosives and be the firing vessel for the project, as well as deploying about 20 OBS's that we had by then, to make multi-channel profiles and to take cores and dredges. The project never got funded, and all our extensive planning went for nought

At the meeting when we learned that the above project would not be funded, Joel and I offered to let other institutions use our ship and equipment to make multi-channel sections since we were the only ones with that capability at that time. We only requested that one of our staff accompany the ship operations (we had concern that others might make changes in our equipment without our knowledge) that the data be reduced to final profiles on our computers (our computers were programmed to read the recorded tapes) and that our representative would be included as one author in the resulting publications (payback for his participation).We had no takers. This would have been the prototype multi-institution project which soon after became common in the community, strongly fostered by the funding agencies.

At about this time we were approached by the Dominican Republic to make multichannel sections south of their country. Their purpose was the hope that offshore sections of thick sediments would interest the oil companies to try to develop oil prospects for them. I was glad to find a source for continuing multi-channel work and for comparisons with sections south of Texas and Louisiana. We successfully obtained a number of transects imaging a thick section of sediments filling a trench , which was a continuation of the filled trench south of Puerto Rico we had earlier noted while I was still at LDGO. Joel called it the Muertos Trench or "dead trench" claiming it was a former trench, no longer active, now masked by total filling by sediments. We presented the data in a number of papers. We never heard that any oil companies introduced prospecting there.

The deep sea drilling project was planning a campaign to drill in the Mid-America Trench and asked us to make multichannel sections of the areas of interest. In addition they were interested in dredging to see if they could obtain rocks to aid in the later interpretations. We were glad to get the project and I added a bottom dredge and other equipment so that I could implement the dredging technique that I had developed for earlier work on the Mid-Atlantic Ridge. This work put a lot of strain on us as the time was very limited until the drill ship would arrive. We were able to obtain the desired multichannel transects and produce the desired

computed sections in time, as well as collecting many rocks near the bottom of the landward wall of the trench.
Our sections included BSR's (bottom simulated reflections) which had been shown previously at LDGO were caused by gas hydrates. These made our papers, published later, of great interest to the geological community. The drilling project used our transects to locate strategic locations for the drilling work.

On my return from this cruise I was approached by a Navy group at Panama City, Florida, about some shallow water work near Panama City that they wanted to use for some underwater sound projects. They contacted me because of my part in the work on dispersion in shallow water published in Geological Society Memoir 27 a a result of some of our wartime work at WHOI. After some lengthy discussion with them we agreed to make two fifty mile long multichannel lines covering water depths of 10 to 20 fathoms, to determine the layering along transects they wished to use in their work. In addition we agreed to take cores at about 40 (as I remember it now) locations along the same lines. Since the two programs could not be done at the same time, we had to make two passes over the same lines to accomplish the desired work. These cores were later used to determine the acoustic properties of the sediments along the survey lines.

The multi channel data were carried out without a hitch. The coring operation was a little more difficult. It was necessary to set up the coring apparatus along the rail of the ship, put it over the side, arm it, take the core and return the corer to the racks along the outside of the rail, remove the pipes, remove the core and return the core equipment to operating form and take another core. In our normal deep sea operations we seldom had taken more than one core a day, leaving much time, when other operations were going on, to remove the pipes, remove the core, clean the pipes, and reassemble the coring equipment in preparation for another coring station. In deep water the trip to bottom and back took about two hours. In this shallow water, it only took about thirty minutes to set up the corer along the rail, launch the corer, take a core, and recover the gear in the overboard rack. It often took longer, to remove the core pipes and extrude the sediments since often the pipes were somewhat bent making them hard to separate. Nevertheless we took all the cores in about two days, taking 25 in one day, a coring record. Anyone who has had coring experience will realize what a monumental task we accomplished. The only problems, were one of my smashed finger which got caught when we separated a bent core pipe from its attachment, and the badly sunburned lips of Tom Shipley who was helping on deck all day in the near tropical sun.

We observed gas bubbles rising to the surface at several locations along the tracks.

While we were reducing the data of the multichannel equipment, as a part of the project, we used the data for repeated seismic refraction measurement to determine the sound velocity along the transect, as well as the reflection sections. As this work progressed I realized that by collecting the sane data in a different way on the computers, we could also obtain reversed seismic profiles at each shot location. I persuaded Mark Houston to include this additional modification in the data reduction and we were able to provide additional information to the sponsors. Our Industrial Associates were also very interested when we presented this work to them in one of our meetings

By piecing projects together this way, I was able to keep the ship operating and to observe many multichannel profiles, even though we had been denied any federal support for our programs. Of course the work for the drilling project was pass through federal money.

Creighton Burke, who had worked previously for Mobil, found out that they were going to enlarge their sea operations, outfit a new and larger ship, and update their equipment. He approached them to see if they would be willing to give the Fred Moore, the ship they would no longer need, to us. Part of the deal was that they would have all of the main ship operating gear in good operational shape, leave the air compressor that could provide sufficient air for four air guns instead of the two, that we had been using, and that they would leave all of the handling gear for the four guns , and the winch capable of handling a 48 channel streamer. This would be a great advantage for us since we would have a great increase of initial signal strength and the greater power of a 48 channel streamer instead of the 24 channel one that we had been using.

With our streamer we had been using about 6 “birds” spaced along the streamer to maintain the proper operating depth. These were streamlined bodies about three feet long which could be attached to the streamer anywhere except at a sound receiver (hydrophone). They could swivel around the streamer so that they did not prevent the streamer from rotating about its axis as towing might require. They also had fins that were controlled by a depth device to seek the desired towing depth. The ones we had, had been given to us by one of the oil companies and were approaching the end of their usable life. Along with the Fred Moore would come about a dozen "birds", as they were called in the industry, which were in good condition. Mobil was changing to ones whose depth could be changed from the towing ship, rather than at each bird.. With the longer streamer these would be a necessary addition to our equipment. We had acquired numerous streamer sections as gifts from various members of our Industrial associates as they upgraded or replaced their own equipment. We could thus make up a 48 channel streamer, although a number of the segments would require some maintenance work. We looked forward to having this larger and better equipped ship to work with.

Before the Fred Moore became available, Creighton collapsed while working in his office after the rest of the office staff had left. He was found by a maintenance worker and was soon in the hospital. It turned out that he had an aneurysm in the brain and that a portion of his brain was damaged. His wife, Yvonne, had to make a quick decision for the doctors of whether to operate to remove a section of the damaged blood vessel reattaching it. The doctors did not know whether this would save his life or what if any functions of his brain would be saved. Yvonne had to make this decision quickly as the option for the operation would remain open for a very short time. She chose to have them operate.

Peter Flawn, a geologist, was installed to act temporarily in Creighton's place, until it could be determined whether Creighton would be able to return. This was a serious blow to us as Creighton had given us superior support with all our projects, and had so many great contacts in the geophysics and oil business. Pete did not have any experience in ship operations, as Creighton had, nor did he have such good contacts in the geophysics and oil business. Nevertheless we anticipated that with Pete's help, we could progress in the optimistic way we had been.

Before Creighton was stricken, he had discussed with me the problem of our staff location in Galveston. The Geology Department and their students had little opportunity to influence or be influenced by us. We decided that the professors on our staff would give one course a semester about geophysics, that geology students could attend. This would be a two hour presentation once a week in Austin so that the travel would not be too burdensome for us. I offered to start, by giving the first course. This would establish the precedent. Thus I would travel to Austin late Monday, spend Tuesday visiting with Creighton and any other business I had in Austin, finish preparation of my lecture, and meet with the class from 2 to 4. Then I would return to Galveston in the late afternoon or evening. I had been doing this for a month before Creighton's collapse. I continued, but soon found I had little contact with Pete as he was usually too busy to see me. Nevertheless I enjoyed the time with the students and thought the course was going quite well. The travel was a little burdensome, but not too bad. I decided that I would not have too much trouble convincing Jim and Gary to continue the program. I felt that I could make out well having to do it about once every three years.

Soon after Pete's taking office, he called a faculty meeting in Austin. Joel, Jim, Gary and I attended. In the course of the meeting a discussion about Master's and Ph D degrees arose. I spoke up and said that the facilities for the geophysical work at sea was so expensive that we should try to only attract Ph D candidates, giving Master's degrees to those who could not make it. Pete then said that Master's degrees were as important as Ph d"s and that this attitude is what had decimated the Geology Department at Columbia. I was completely taken aback, expecting Pete to have a friendly and helpful attitude instead of a caustic one.

Soon after that meeting I received a request from JOIDES to compete for a contract for multi channel and other geophysical observations of a South Atlantic area about 15 degrees south latitude, for the Deep Sea Drilling Program.. After our several year head start, WHOI and LDGO had finally begun to make multi channel observations, and would also compete. One of the requirements was that Joides wanted the data to be used, not only for selecting drilling sited for the Deep Sea Drilling Project, but also for additional scientific studies.

I called together Joel Watkins, Dick Buffler, Mark Houston, Jean Schaub and stated that this would be a good project to use the Fred Moore, that we expected to have by then, and that the 48 channel streamer and the computerized navigation, which would be left on Fred Moore, controlled by satellite navigational fixes, should give us an advantage in the competition. We would all have to put our heads together and decide what additional scientific studies we could generate with the data, besides picking out the drilling sites, which was the basic purpose for the study.

In about a month we put together a proposal that we thought would be a winner. The additional work that we proposed would cost so much that we decided to provide three possible budgets. One to just make available the information required for choosing drilling sites, a second for additional work that would use the data for other purposes that would cost about 10 % more e than the basic work, and a third that would require additional funds, of about 40% more for a more sophisticated study.

The meeting with the JOIDES group was to be held in Washington. I made a date with Frank Press, now Jimmy Carter's Presidential Science Advisor, to have lunch together the day of the meeting I expected the meeting to be over by then. The meeting was called for 8 A.M., so Dick Buffer and I went to Washington the night before, spending the night at the Cosmos Club. LDGO made their presentation to the committee first, we came second and WHOI came third. The committee quizzed us, with everyone present, on various facets of our proposals and about our equipment and experience. Of course the panel had had our proposals to study in advance of the meeting. About 11:30 they asked us to get lunch and return to the meeting room about 1:30 when they expected to return to give us their decision. I told them that I had a date to lunch with Frank Press, and that it was possible that I would be a little delayed for the later session. They did not see any problem with this.

My luncheon with Frank was very pleasant. He showed me around the White House Office Building, including the empty cabinet meeting room. Over lunch , we discussed a number of science questions, and he told me that he admired what I had done in Texas in getting most of our support from industrial sources. I explained that I had tried hard both at ONR and NSF to get grants or contracts but had had little success, which baffled me. He told me, that he had heard that it was the government agencies reluctance to enter into obligations for additional ship support, which they believed would be the result of giving us funding.

I returned to the JOIDES meeting in time to find that the committee had not yet returned. After about an hours wait, the committee returned and told us that they felt that Galveston had responded best to their request, and that we would get the grant. They asked the others to leave and requested us to remain to confer on the details. After the others had left, and a little discussion they advised us that the funds available would be what we requested for the basic work. I pointed out that there was no way we could undertake the additional work that we had outlined without additional funds. They responded that those were the only funds available. I never could understand the inclusion of the additional work expected in view of the funds available. However we were glad to have won the competition. Now it all depended on our getting the Fred Moore ready in time.

About this time it was decided that the damage to Creighton's brain was so extensive that he would never be able to return as the head of the Marine Institute. A search committee was appointed and several candidates were considered. One of these was Robbie Moore, a geophysicist at the

University of Alaska. Due to some kind of misunderstanding Robbie believed he had been selected by a talk he had with one of the University Vice Presidents, so he presented his resignation to the University of Alaska and made preparations to live in Austin.. This had not been the intention of the selection committee, but they felt that they would have to accommodate to the change. To do this, they needed the concurrence of the two divisions of the Marine Institute. I was called and asked to call together the senior members of the staff and explain what had happened and see if they could possibly provide concurrence. Our decision was that while we would not have chosen Robbie as our first choice, that he was acceptable. I suppose that the biological group at Aransas must also have done the same. Anyway, arrangements were made that Robbie was to succeed Pete Flawn at the end of the semester .

As Pete's last major act, at least with us, was to call me to Austin to discuss the use of Palisades Geophysical Institute as our ship operators. I asked to bring Frank Mongelli but was refused. The meeting consisted of Pete, Ross Shipman, formerly Creighton's assistant and now Pete's assistant, and me. Pete opened the meeting with a bomb shell stating that we would have to quit using Palisades Geophysical Institute as our ship operators. His reasoning was that since I was President of PGI that there was "the appearance of a conflict of interest". I asked how there could be any conflict of interest of a non-profit research corporation providing a service for a non-profit University. The only response was "the appearance of a conflict of interest". I received a small salary from PGI, on the insistence of the board of directors, because of projects that PGI was carrying out for the Navy. The board had insisted that the Navy could never take seriously a corporation with an unpaid President. I had been endorsing my pay checks as a contribution to the University of Texas. I explained to Pete that we had tried initially to use a Texas commercial corporation to operate the ship, but that early on when our cook had quit on a Saturday morning as the ship was due to sail from an outport, we could not get anyone at the corporation over the weekend and had had to wait until the following Tuesday to get a new cook so that we could sail. This resulted in the ship costs increased by three days. These were not negligible. My response was to get PGI to provide the service. Since I was President I could get them to act whenever we needed them, and they did. I further started that I had served as ship operator at Lamont, and that I could not do that with all the duties as the Director of the lab. Pete was adamant that there would be a change.

Pete had rebuffed me at every turn during the time he remained as acting head. I had tried to approach him with positive results both for myself and the lab, but he always returned a negative response. For instance, when I told him what Frank Press had said about getting support from industry, he replied that was nothing new and had been done often. It had certainly never had been done with any Oceanographic Institution which had very expensive equipment and ship operations. I can not understand his negative attitude for me and the Marine Geophysical Institute unless it was his reaction as a geologist to the fact that Geophysics was flourishing while Geology appeared to be languishing. I took no action, waiting to see what Pete would do. Anyway, I had little time to worry about it.

Immediately after this, the Fred Moore was made available to us, after several delays, about mid November. We had to sail for the South Atlantic in the earliest part of January to carry out the South Atlantic Survey in time for the drill ship's arrival. This left little time to get ready. To our dismay we found out that all the ship operating gear was not ready to go. In fact the two diesel main engines, two of the three diesel engines of the generators, and the diesel engine for the large air compressor all needed overhauling. Besides, Mobil had removed all the gun handling gear that they had said would be left, so that we would have to fabricate replacements. There were many other maintenance items that had not been maintained at the end of Mobil's tenure, that we had expected to be ready. Of course all of this would cost large expenditures if we were not to fail in our survey contract. There was no time to argue about money, so I used the approximately 1 million dollars that had accumulated in our Industrial Associates fund. I had carefully refrained from using it although there was much pressure form the staff to do so. Now my frugality saved us from having to renege on our survey contract. We hired a commercial company to overhaul the diesel engines at our dock in Galveston. After a quick survey they told us that they thought one of the main engine's overhaul could wait until the end of the cruise and that they would just be able to complete the rest of the engine overhauls with the use of lots of overtime. I had no recourse than to agree.

Meanwhile Arch and I with the help of local welders went to work to fabricate handling gear for the four air guns. I had already ordered four new guns, which were delivered in ample time. Some of the old streamer units needed some maintenance and and additional units required to make up the 48 channel streamer needed much attention. I arranged for one of the geophysical survey companies to take the 48 units and the leader to their shops and to bring them up to acceptable use standards. We had obtained three bids for this service and chose the intermediate one since they stated that they could adjust the flotation so that the streamer would tow properly. The other two did not offer this service. Since we would have little time to accomplish this after we sailed, this appeared to be a necessary factor for us. Meanwhile,Joel and other staff members set about ordering the large quantities of supplies that were needed to keep the streamer operational, the recording tapes etc. for the recording channels, and the supplies for the towed magnetometer and the sounders. We also had to acquire a satellite navigator which were now available commercially.

A little before Christmas I received a telephone call from Ross Shipman telling me that Pete Flawn had just been named President of the University to take place about June. I responded that I guessed that would leave me with nothing to do, because of Pete"s attitude towards me, but to retire for the good of the laboratory. I had no time to dwell on this revolting development.

With all the work to accomplish, much of it unexpected, it took a herculean effort to get ready to sail in the first week of January, but we did. Joel and I both went along to see that everything was ready by the time we got to the survey area. It would be necessary to go into port for refueling and revictualing before the survey was completed. I was to leave the ship there and Joel would supervise the completion of the survey.

About a day after leaving Galveston, the Chief Engineer reported that the main engine that had not been overhauled broke down and that the engine room staff could not repair it. There was nothing to do but to return to Galveston and have the engine overhauled. Fortunately, there was just enough money left to accomplish this. In five of six days this was accomplished and we immediately set sail again.

All the way across the Caribbean we wanted to stop to make a test of the streamer and all the gear to make sure that we were ready when we got to the survey area, but we had strong head winds and heavy seas and I decided that we had better postpone those tests. Finally I could not postpone the tests any longer and as we approached the Windward islands we let out the streamer. To our dismay it was much too buoyant and floated on the surface despite the birds trying to make it tow at a usable depth. I sent a cable to the company that had overhauled the streamer and asked for advice. They responded we would just have to adjust it ourselves. This caused us a two day delay while everyone in the scientific party worked long hours with very exhausting labor to make the necessary adjustments. Finally we decided that we had succeeded well enough to proceed.

After a quick stop at Recife, Brazil to top off our fuel and victuals and the planned replacement of the captain, we finally arrived at the survey area about two days later than we had intended. After a day or two when we got acquainted with all of this new (to us) gear the survey went off well. We found that the computer control of the ship's course was a great improvement and convenience over our previous experience. At the appropriate stopping point we went into Southwest Africa for a much needed 3 day rest, revctualling and refueling After one day in port I left flying to Johannesburg to get a flight back to the states.

The ship returned to the survey area, with Joel in charge, completed the survey and returned to Galveston about mid march.

On our way into Southwest Africa the man who was supposed to keep the air guns functioning came to me and demanded an increase in his wages. I told him that I could not possibly give him a raise since he was nor even doing his job. We had bought four guns and already had one which we took along as a spare. When a gun had problems, which happened only infrequently, we would substitute the spare for it , and bring it aboard and the air gun man was supposed to repair it no matter what time if the day or night, in case another gun would fail. When the guns were working the air gun man would have no particular duties. The man in question had insisted on getting his sleep at night whether the spare gun was ready or not. Several times I had to help him when repair a gun which had failed before the spare was ready. He replied that he worked as hard as anyone on the ship except me, and that noone could work that hard. I told him that when he did his job I would consider a raise. When it was time to leave port the air gun man did not show up and Joel had to train one of the other scientific party members to keep the guns working.

On my return to Galveston, I decided that I needed to make a decision about what to do now that Pete was to be President of the University in June.

Starting with a staff of four Professors, after two years we had lost Prof. Ewing because of his untimely death, and after serving a year or so as Acting Director, I had been made Director. We now had four Professors, seven senior scientists, six other scientists, and groups of graduate students and assistants. I had added Arch Roberts who had established a small machine stop and kept the scientific equipment mechanically sound, and Ken Griffiths with three technicians who had established an electronics laboratory. They fabricated such electronics as we needed, that were not commercially available, and kept our electronic equipment operational. In addition we had established an Industrial Associates program.

In those seven years we had acquired two vessels, 125 foot Ida Green and 230 foot Fred Moore. Ida Green was capable of making 24 channel reflection recordings, with a sound source of two high pressure air guns, taking precision soundings, towed magnetometer recordings, underwater photographs up to 3000 fathom depths, a deep sea trawl winch that made it possible to take cores and dredges of the bottom. All observations and samples were located by satellite navigation..

Fred Moore was capable of taking and recording precision depth soundings, 48 channel seismic reflection recordings, operating four high pressure air guns as sound sources, a single channel seismic reflection recording giving immediate reflection sections for guidance, following courses controlled by computer and located by satellite navigation , and towed magnetometer recording .

Gary Latham and Jim Dorman had analyzed lunar seismic data until NASA had terminated the data radio reception. Jim Dorman had established three seismic unattended recording stations, solar powered, in Texas. Tosi Matumoto had established a remote seismic station reporting to a major city in real time for a Central American government. It was to be used for population safety, quick damage control purposes and research. Nearby governments were inquiring about similar systems.

We had also established a computer laboratory capable of analyzing and charting the multi-channel data, and was also available and used for all of the other laboratory geophysical programs

Gary Latham developed cheap ocean bottom seismographs which could be used for earthquake monitoring as well as refraction experiments. He had made a number of refraction profiles with them in the Gulf of Mexico. Our industrial associates were very interested in this equipment, besides the multi-channel reflection data.

Joel Watkins, Dick Buffler, Jeanne Schaub, Mark Houston and I, with our staff and students had collected multi-channel data extensively in the Gulf of Mexico, the Mid-America Trench., around the Windward Islands, across the Blake Plateau into the deep Atlantic, and three dimensional surveys of the Sigsbee Scarp, the Mexican Ridges, the Campeche Scarp,and the West Florida Scarp. In addition we had made crustal sections in the Virgin Islands. south of the Dominican Republic, and off Trinidad. Bill Behrens had established a program of coring and dredging in the Gulf of Mexico.

Our staff had published 344 papers in well known Scientific journals. The University had built a building in Galveston which housed our staff, and paid the salaries of the Professors , Senior scientists and about six other personnel. Additional staff were funded by our various research programs. Otherwise we had mainly equipped ourselves from the equipment donated to us by commercial geophysical and oil companies which were upgrading their own equipment. Most of our research had been funded by commercial companies. We had only one small government grant, although we had received several contracts from JOIDES, which was National Science Foundation money sub-contracted to us.

With this we had initiated and established ourselves as the leading oceanographic institution carrying out multi-channel work in the deep ocean, and a fairly capable oceanic geophysical laboratory.

All of the above had taken a great toll on me. In addition , I was having trouble remembering geophysical terms I had been using for 30 years. This was a source of great embarrassment to me. Because of having had rheumatic fever when I was young, I could expect a short life expectancy. I didn't see how I could carry on to handle the problems that the University President , who apparently did not like me and had little appreciation of what we had accomplished in Galveston, would initiate.

Besides, I had a lot of traveling I wished to do while i was physically able. I decided to retire, and sent in my letter of resignation to take effect July 1, 1979.

The staff gave me a "glad to see you go" party at which I was asked to make some remarks. I responded that I had had an interesting career and felt especially privileged because I had played a part in many exciting developments such as: First ocean bottom seismic refraction measurements, the first deep sea photography, producing the manual "Sound in Sea Water", the development of the underway bathythrermograph with handling winch, the underway bottom sampler, introducing and equipping the Navy with its first underwater photography, location and photography of ship wrecks,explaining the method by which the Germans were destroying whole acoustic mine fields, Sofar (sound transmission across oceans), the first seismic refraction measurements in the deep sea,the founding of Lamont Geological Observatory, long range submarine detection with low frequency narrow band passive listening, which led to the Navy Sosus

System (ocean wide surveillance of ships at sea), pendulum gravity measurements on submarines, acquiring and equipping Vema, developing a high sped deep sea winch, establishing the first Industrial Associates program in an Oceanographic Institution, the International Geophysical year, discovery and charting the Worzel Ash in the Pacific Ocean, surface ship gravity measurements, the discovery of a number of sea mounts, first passive anti-rolling tanks for oceanographic ships, established satellite navigation at sea, discovery of the Sigsbee Knolls, one of the first co-chief scientist of the Deep Sea Drilling Project, drilling Discovery Knoll in the deep Gulf of Mexico and demonstrating that it was a salt dome with cap rock and at least of show of petroleum (the first petroleum discovery in the deep ocean basin) , founding the Galveston Geophysical Laboratory, acquiring and equipping Ida Green, locating and buoying a lost sea drilling platform, initiating multi-channel seismic reflection measurements in the deep sea, the seismic stratigraphy of the Deep Gulf.lf of Mexico, equipping Fred Moore for our multi channel work. From my scientific investigations I was able to author or co-author more than 140 papers published in recognized journals.

Afterword

Between 1937, when I first started to go to sea to help doing research and 1979, when I retired, the concepts of the ocean basin had completely changed. The ocean basin was no longer thought of as a simple basin receiving sediments from land since the inception of the seas. Instead the ocean crust was found to be at least as complicated as the land crust. It had mountains (ocean ridges and Island chains) with higher peaks and longer chains than those on land. It had larger and more level plains. It had deeper and longer canyons, than any on land. It had more volcanic peaks (sea mounts, islands) than continents. Its crust was thinner 6 to 20 km compared to 30 to 60 km beneath the land. The oldest age for an area of an ocean basin that had been found was about 120 million years while for land areas it was about 4 billion years. There were more earthquakes beneath the oceans than beneath the lands, and in addition there were the great ocean deeps with no comparable features on continents

Photography had been established at nearly all depths of the ocean. Recordings of continuous soundings at all depths were common. . Seismic observations, both natural and man made had been observed. Dispersion of sound waves in shallow water had been discovered. The deep sound channel had been demonstrated and long range sound transmission (greater than 2300 miles) had been accomplished. A number of projects had contributed to Defense Department problems. The means for measuring the magnetic field on land had been adapted for use at sea. Gravity measurements on surface vessels had been started.The largest gravity anomalies and at least as large positive anomalies as on land, were discovered at sea. Heat flow through the ocean floor was being measured. Magnetic reversals had been proven to exist on the sea bottom. The concept of turbidity currents had been proven and were believed to be a principal carver of submarine canyons. Salt diapirs had been shown to exist, with the first show o petroleum in the deep ocean. The first discovery of gas hydrates had been found . The concepts of sea floor spreading and plate tectonics had been initiated.

The Deep Sea Drilling Project was the second operation of JOIDES (Joint Oceanographic Institution Deep Earth Sampling Project). JOIDES was a pioneer consortium of four Oceanographic Institutions; LDEO, Scripps, University of Miami, and the Woods Hole Oceanographic Institution (Joides now includes several additional institutions. and cooperates with a number of foreign nations scientists and their scientific institutions) This operation proved that groups of institutions could work successfully together. (Although I used to joke that JOIDES, pronounced "joy dees" stood for all of the joyful days that we spent in committee meetings). Deep sea sediment drilling had commenced.

Our researches contributed to many of the above concepts, discoveries and techniques. I felt privileged that I had a hand in many of these investigations, and contributed to others by developing the capabilities of Vema and Conrad for research at sea, that let others male their observations. Further I had played an early part in establishing the reputation of the WHOI and founding two ocean research Institutions, and JOIDES

Today there are dedicated ships with fifty or more scientists from several institutions, and sometimes other countries, making up the scientific party, like the Deep Sea Drilling Ship, or the Maurice Ewing specializing in multi-channel seismic work. Several institutions have several ships of varying sizes which make mostly single purpose individual cruises, so far as I know, and others with only a single ship or none, like the the University of Texas Institute of Geophysics (UTIG), relying on the dedicated ships, or those of other institutions to obtain their sea data. This is quite a change from the way we operated Vema, Conrad at LDEO and Ida Green and Fred Moore at Galveston Geophysical Institute, (which is now UTIG).

Our style of taking sediment cores, studying them for what individual scientists were interested in, and carefully storing them so that they were available as new studies were initiated, not only within our labs and in other labs in the U.S but also in the rest of the world. Soon there were many studies, that initially had never been even conceived, being made with these cores. By the 60's one particular core had been totally used up from the many samples that had been taken from it, even though our policy was that any sample that was not destroyed in its study, be returned to us, after its use, so that it could be available for other investigators.

This account points out the importance of basic research. Our efforts before WWII to measure the sediment thickness, gave us the background for our studies of underwater sound for the U.S.Navy. Our making magnetometer observations, when noone knew what to do with them, made an invaluable set of data available for the development of the theories of sea floor spreading, plate tectonics and mineral location. Making a world wide collection of sediment cores and carefully storing them, made many studies, which evolved after many of the cores had been taken, quickly possible. The initiation of the interpretation of gravity anomalies to develop geological structure, especially at sea, has became a useful tool. The refraction measurements of sediment thickness led to ocean crustal and sub moho studies These were important building blocks for revising the framework of Geology and all its ramifications and contributions to society

These and many other studies were undertaken as basic research , that is, undertaken for the sake of knowledge, and curiosity. Many have had significant impact on the welfare and support of society. Often, the impact is not immediately obvious and may not be evident until many years later. For example , our studies of the main sound channel in the ocean in the 1940's became one facet in the 1990's for the study of climate change.

For the welfare of all the people of the world, it is important that basic research be fostered and supported by all.

With scientific crews of 10 or so, at Galveston, we kept our ships operating at sea as much as our funding would allow, making observations for several project s on the same cruise. At Lamont, we had kept our ships at sea whenever they were not in necessary upkeep, and ran ten or more projects all the time they were at sea. On one occasion a scientist from another institution made the remark that we didn't even have proper docking facilities for Vema. Doc's response was that "the ships were only useful and taking data when they were at area".

We believed that at any one place, the observations made might be the only data available at that location for many years or even decades or centuries, so that every possible observation should be made. For example, starting in 1953 with the acquisition of Vema, we made magnetic field observations. although we did not then know how to interpret them. In the early 1960's when the seas floor spreading and plate tectonics were developing , those observations provided a good base of information because they were available.

The close association of the scientific staff on board where everyone had to help each other in handling their equipment and recording data, caused a great deal of cross fertilization of ideas, concepts and cooperation. This carried over to the lab on the return from sea to the benefit and close association of students and staff and the intercomparisons between different disciplines.

The accidental inclusion of the geochemistry group and the geophysics group at Lamont in the initial stages, because both groups needed more lab space, worked to the advantage of both groups. Soon the geophysics group were seeking help in time dating samples from the geochemists, and the geochemistry group was seeking help from sea sediments for determining radioactive geographical distributions. No doubt this would have occurred eventually, but the close association made it happen much quicker. I believe this led the Geochemists to consider other problems that could be studied at sea.

Our style of administration was to make the fewest rules and regulations, for the staff to follow, that was possible. Scientists do not stand regimentation well, and I believe it inhibits innovative thinking and initiative. We did have to form a salary committee to prevent grossly different pay scales, for essentially similar work in the laboratories, from developing. On the ship we had to have a Chief Scientist whose duty was to see that the work of all projects got a fair share of ship operations, as well as to further his own projects. This demanded the Chief Scientist get a fair working knowledge of all the observations being made on board. He also had to make sure that assistance for a project from others in the party was available when needed.

Personally, I tried to instill in everyone that I had any contact with that there was nothing that was too much trouble, too dirty, or too difficult to get good observations, and to do everything possible to increase the accuracy of observations. I also tried to set a good example by participating in all projects as much as time would allow.

Anecdotes and Observations.

I spent about two months at sea,on average, over a period of forty years and only once, near the middle of the Sargaso Sea, did I see such a dead calm that it was impossible to see any motion of the sea surface.

The necessary development of our own equipment and instruments with our own machine shop and electronic shop, developed skills that made it possible with persistence and much labor to build and keep equipment working that would have otherwise failed. This ability has saved a number of expensive cruises from being total failures. I believe this capability has now largely been lost. At sea, I often referred to an electric drill clamped in a vise as my portable lathe, and a file as my portable miller, and a hack saw as my power cutoff machine. (for those not in the know, a lathe is a machine used to shape cylindrical objects, a miller is a machine that makes flat surfaces or linear slots, and a cutoff machine reduces the length of a bar or plate).

One of the prettiest sights that I have seen occurred when we were recovering our sea bottom equipment, such as a core. The 1/2 inch wire rope was reeled in at about 500 feet a minute. Water adhered to the wire rope until it was about rail high when it was shed in large drops . Initially traveling upwards the drops were shed laterally and followed parabolic arcs as they returned to the sea. The overhead light on the A frame reflected from the drops so that they looked like large sparks repeatedly forming an umbrella-like pattern. This fireworks show never failed to intrigue me.

My crystal ball was not very good. When asked,in the 60's, I told a number of people that I did not think it was possible for another institution like Lanmont to be developed because of the increasing complication of the equipment and the changing funding situation. Then , starting in 1972 , we did just that at Galveston,

Another time, at a scientific meeting, I was asked when a surface ship gravity measurement could be made. I replied that as far as I could see, never in my lifetime. Just one year later, with Dr. Graf's instrument that I mounted on a gyro-stabilized platform I did that.

I think I got the most satisfaction in finding the Vema and upgrading its capabilities. Examples of these were the improvements in the deep sea winch, constructing a spare reel which made it safe to carry a spare spool of 30,000 feet of 1/2 inch wire rope on board, learning the tricks of using two wire ropes to the ocean bottom in deep water simultaneously without tangling, installing passive anti-rolling tanks, and the installation of satellite navigation. It was especially gratifying to see the important uses that the other Professors, Senior Scientists, and Graduate Students mad of this important tool for research.

The close association of the numerous disparate observations at sea on our ships fostered a close cooperation of different investigations on common problems. This carried over to other observations which could be better made on land e.g earthquake seismology, geochemistry. I believe this led to an esprit de coups which was hard to find in many other institutions.

J.Lamar Worzel, June 2001.