



MY LIFE AS A BIOLOGIST¹

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Early Beginnings

My father was born in Kansas where he became a farmer. He later took out homesteads. First in North Dakota, and then in Alberta, Canada, becoming a Canadian citizen. My mother was born in British Columbia; her father had migrated to Canada from Tennessee, her mother from Wisconsin. They were married in Spokane and lived in British Columbia. Later they moved to Alberta and Mom attended Normal School (later the University of Alberta) and trained as a teacher. She started teaching in a one room school house at the age of 17, while Canada was in World War One. She then moved to a school near my father's homestead. They met and married, and my

brother Gene was soon born in Medicine Hat. (I used to tease my brother that he could never become President of US, but I could!).

After many years of good wheat harvests, there were three successive years of crop failure. The family left the farm, and that was the last farming for my Dad. They moved to Kansas USA where my Dad first worked in a store and later became a Fuller Brush salesman. But Mom couldn't stand the Kansas heat and they moved to Nelson, British Columbia (her home town). Dad sold Fuller Brushes there, too. They then moved south to Oregon USA, first to Dallas then to Corvallis. My mother said no more moving,

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we are staying here. I was born 3 months later.

Corvallis was a good place to grow up. It was a small college town, home of Oregon State University. There was an intellectual environment and the college was an influence on my life, as you will read later. My first recollections are of the summer after I received a child's hoe, shovel, and rake for my 3rd birthday. I remember digging in the dirt at the vacant lot next door. My father planted a Douglas fir in front of the house and in the passing years I would look at that tree's growth; I still do (the last time being 1996). We moved to another house which had a big yard. We lived there from my age 3 to 7. My Dad had a vegetable garden, and we also raised rabbits and chickens for food. I remember plucking the chickens and shelling peas. My brother Gene is more than 4 years older than me, and he would take me to nearby Oak Creek where we would catch crayfish and build dams (he is still building them!). We also went to the college cow barns and green houses, where I always looked for the orange and banana trees. I thought it would be great to have my very own orange tree (I now do). I started grade school and Mrs. Gowan was my teacher. She grew different plants in the classroom, including a small wheat garden and demonstrated the effect of the position of light on the growing patterns of the wheat. Years later I ran into her on a bus trip from Bend to Corvallis, Oregon. We had a great time; it turns out that she had a master's degree in botany!

We moved two more times in Corvallis, and then in 1944 to California. Things began to happen along biological lines during the summer between 3rd and 4th grade. I began a butterfly collection (not a very good one), and a teenager with a fantastic butterfly collection gave me two cecropia moth

(*Hyalophora cecropia*) caterpillars. The caterpillars ate only one particular kind of leaf, which grew on only one tree in town. Each day I would ride my tricycle (I got my first bicycle at age 12) to collect the leaves. One day the moths built their cocoons. Unfortunately, I did not put a stick in the jar for them to climb on when they hatched. That was my last effort to raise caterpillars as a boy; but I did it later when my children were growing up. That summer also saw my introduction to annelids. My brother was a natural salesman, and still is. He went to the campus at night and collected night crawlers (earthworms) which he then sold as fish bait. I do not know how successful he was, but he figured that cutting the worms in two would get him twice as many. Much later, as a graduate student at the University of Southern California, I gave a seminar on Libbie Hyman's doctoral dissertation which involved regeneration among oligochaetes. She found that there was a critical segment number required to successfully attain regeneration. Hyman did not work with the species my brother collected.

I Become an Editor!

Many things happened during the summer of my ninth year besides chasing butterflies. I received a baseball bat, a soft ball, a mitt, and a baseball cap for my birthday. I also had a birthday party and invited three boys and one girl. Two of us liked the same girl. The baseball equipment became the start of the neighborhood Saturday morning baseball games which extended into high school. We would go over to the lower campus of Oregon State University (one block away) and start the game with three or four of us. We gradually ended up with about 10-20 kids. The lower campus was our playground. It was 10 blocks of trees and grass. We also played football, but more about that later.

With the butterfly collection increasing in size, my companion and I started going to the Oregon State Museum of Natural History. It was a hodge-podge of a museum with no real organization. It had an extensive butterfly collection, which helped us learn the names of what we collected. However, I did not have any concept of a scientific name. There was also a human skeleton in one corner which I avoided looking at! The two of us went to the museum two or three times a week, where we would concentrate on a particular section during each visit.

We then had the idea of putting out a newspaper. I was the editor and he was the reporter. Additional people got involved, including my brother, who did a comic strip (he is very artistic). We published one issue. My mother kept a copy of the paper; hopefully it is still among the papers and pictures my Mother left my brother and me. I then continued to put out a paper by myself, which was hand printed. I made one copy and a person had to pay 2 cents to read it. My interest in being an editor continued into the fourth grade. We had a class newspaper and I was the poetry editor; however, I had difficulty in distinguishing between being a poetry and a poultry editor! My teacher was also correcting me.

More happenings that summer included a trip to Nelson, British Columbia, my Mother's hometown (Steve Martin's movie, *Roxanne*, was filmed there). My grandfather was still alive, and this was the last time I saw him. I mostly played with my cousins, whose parents had a cottage on the lake. This was my first real trip; I had been as far as Portland, Oregon, up to that point (85 miles). The change in the ecological environments (wet to dry to mountains) did not make any real impression on me. The day before we took the train to Canada, I rode my first bicycle. My mother dashed out the house to

get me off the bike; she was afraid I might get hurt and we could not go on the trip.

As I mentioned earlier, Corvallis, Oregon, was a good place to grow up, even during the Great Depression, when there was 25% unemployment. My father had work during the summer at the local cannery, but not always work in the winter. My mother taught piano lessons in the schools and at home. She also played in physical education classes at the college. The presence of Oregon State University undoubtedly influenced my life in other ways as well.

The physics department of the university set up their telescopes during the summer, and I can remember looking at Venus, Mars, and the Moon. Their telescopes were not strong enough to see the rings of Saturn; I have yet to see them through a telescope. I used to wander through the engineering buildings and shops. We played hide and seek in the mechanical engineering labs for years and we never were kicked out.

I took swimming lessons at the university during the summer and on Saturdays in the winter. As I mentioned earlier, I would walk through the agricultural barns. We would sneak into the football games (it was easy), and in the spring we would watch track meets, then turn around and see a baseball game.

Getting into the basketball games was more difficult; we would go to the men's gym one or two hours before the game and sit on the stairs. No one ever asked us to leave. It would have been a mess if the gym ever caught on fire. My friend and I would go to the homecoming events and listen to Oregon State University football games on the radio. I liked the campus environment, and it definitely played a role in my wanting to become a professor. I think the main influences of the university on me were its

intellectually stimulating atmosphere, the parades during graduation commencements, and the fact that most of the faculty lived in the nicest houses.

I Begin to Look to the Future

Let me introduce three life-long friends. First there was Miles and shortly afterwards there was John. We had moved to 12th Street when I was seven, and both Miles and John lived nearby. We walked together to Franklin Grade School. We wore black leather jackets and black leather helmets which was the fashion of the time due to the influence of the Charles Lindberg solo flight to Paris in 1927. Miles later became an auto engineer who specialized in auto exhaust; in fact, he gave a seminar where I later worked, at California State University, Long Beach (he now lives in Hawaii). John took over his family's furniture business, and then became a salmon fisherman, near Depot Bay, Oregon. He was the first of our group to die, in 1996. John and I met Bob the summer before Bob and I entered 7th grade. He did not join our group until the 9th grade, when we both delivered for the same newspaper, the *Oregon Journal*. Bob earned a Ph.D. in mathematics and after teaching at Arizona State University, Iowa State University, and Fresno State University, he returned to Corvallis and taught at Oregon State. He always wanted to get back to Corvallis.

Going to junior high school brought some changes; for one thing I got my first pair of long pants! Yes, in those days getting the first pair of long pants (corduroy) was an event. Grade school boys wore knickers. A couple of days before the end of my seventh grade year, my pants wore out and my mother bought me a pair of denim jeans. I was embarrassed; wearing jeans in those days was a sign of your family being poor. I didn't wear my next pair until college, but I spilled acid on those pants in organic chemistry lab.

I didn't get my 3rd pair of jeans until after I retired. Times do change.

During my earlier years, I never really gave much (or any) thought of what I was going to do when I grew up. I suppose I went through the common thought of being a physician, but, if I did, it was just in passing. In the ninth grade, I took a math class and one of the assignments was to keep a personal expense record

for one month. This fascinated me and I continued it for six months; a sufficient length of time to earn my business merit badge (I was a Boy Scout). We were required to write a major report in social science class on what we want to do when we grow up. I wrote on accounting and becoming a certified public accountant (CPA). In fact, two of us wrote on this subject; the other boy actually did become a CPA. In the 10th grade, I took accounting and didn't like it. That was the end of my accounting career. After keeping personal records for six months, I have never done it since.

I also took general science in the 9th grade from my math teacher. (I ran into him some 50 years later at an anniversary reception for my friend, Bob. We didn't talk about my 9th grade classes but instead about our experiences as Boy Scout masters.) I only remember one part of this science class. It was my initial introduction to the scientific method. I was given the assignment to prove or disprove that air occurs in soil. I had to devise an experiment to test this hypothesis. I put some soil in a glass jar of water and let it set for an hour or so. Air bubbles gathered on the side of the glass. I had boiled the water to remove dissolved air, showing that the bubbles had come from the soil and not the water. However, I do not recall if I had set aside a jar of boiled water for a control. I'm not sure if I knew what a control was. I had

to give an oral presentation in front of the class.

The newspaper business reentered my life in the 9th grade. As I mentioned, Bob and I delivered the *Oregon Journal* newspaper (which is now defunct). As a publicity campaign, one of my newspaper customers and I were featured in the paper. It was the first time my picture was published in a newspaper. I even received fan mail from a 14-year old girl; I never met her, but I remember her name! I also worked on the junior high newspaper, but not as a writer, but on the business side. I went out during school time to get advertisements for the school paper.

High School and New Environments

I entered Corvallis High School in my sophomore year. The two subjects I remember the most were accounting and biology. I have already commented on my initial interest in accounting, but after I took the course, I knew it was not for me as a profession. I remember a great deal about my high school biology class, taught by Miss Patton. She was a very dynamic teacher, excellent in leading and redirecting discussions, and strong on class discipline. Both direction and discipline (which I learned from her example) were important tools when I taught high school biology. I ran into Miss Patton when I was working on my master's degree. She remembered me, but she no longer taught biology; she had transferred to teaching math. I thought it was a shame at the time. Her class was a traditional biology class with many drawings required. I never managed to get an A in the class; it was always B+. I continued to deliver newspapers through the year. Bob and I became close friends.

We lived in a duplex, and the man who lived in the other side taught me how to play chess

and cribbage. I then taught Miles, John and Bob to play chess, and we had a chess conference. I don't recall who won, but it probably was not me. They did not care for cribbage. We started our football games in the ninth grade. John and I always played against Bob and Miles, and always used the pass-run option play. I do not recall who won these games. Our last game was after World War II, when Bob and I were working on our master's degrees, and the other two were undergraduates (they had been in the Navy during the war; Bob and I were classified 4F). A couple of years before John died, he wrote me that his wife threw out the old football.

The summer after my sophomore year my mother wanted to take additional music training in Portland, Oregon. She taught piano lessons both in public school and privately at home. (Neither my brother nor I can play the piano; we heard too much of it at home to want to learn.) My brother was working at the cannery at the time, and my father was in Bend, Oregon, selling Fuller Brushes. My mother rented our home for the summer and went to Portland for lessons. I went to Bend for the summer to live with my Dad.

The bus trip to Bend passed through extensive lava beds as it crossed the Cascade Mountains. The bus driver stopped at the peak of the mountains and gave us a lecture on the lava formations, etc. This odd geology fascinated me. My Dad lived in a men's boarding house, a common practice all over the United States in those days. You can see the remains of such large buildings in any industrial city. It was my first experience of seeing drunken men and 'morning after' effects. I sold some Fuller Brushes during the summer, but I did not really care for it. I made about one dollar per hour for it, which was good money in those days (Cokes were only a nickel; movies \$0.50). The best part of my

summer in Bend was field trips with a church youth group. The minister liked geology, and we visited lava caves, lava cast forests, and obsidian fields.

The country surrounding Bend was dominated by Ponderosa pines and junipers. What a contrast to the Douglas fir forests of western Oregon! The pine forests had much less underbrush, and more open space than the dense fir forests. I used to spend time watching the red ants work in these open spaces. Spending summer in a very different environment served to open my eyes to my surroundings. Over the years, I made many trips between Corvallis and Bend, and never got tired of looking at the change from one type of forest to the other.

I returned to Corvallis to enter my junior year in high school. The two subjects I remember most were typing and journalism. I think everyone should acquire a typing skill, especially today with computers. I wasn't fast at typing but I was quite accurate (not needing the current blessings of the computer backspace key). I liked working on the school newspaper. I was being groomed to be the sports editor the next year. I helped write headlines and proof copy. It was an exciting time.

An event occurred one week before Christmas vacation which affected my life. Corvallis experienced a bad flu epidemic, and the high school was closed. At the same time my mother was considering having the daughter of a deceased friend live with us since she and her stepmother did not get along. She was my age and not very attractive. I decided to go visit my Dad until school started in January. I visited Bend High School and decided to attend the second half of my junior year there. I would still go back to Corvallis High to be the newspaper sports editor.

Bend High School and Beginning of WWII

Bend, Oregon, was much different than Corvallis. In those days Bend was a saw mill town and very few of the high school graduates went to college. It was not an intellectual community. I think I liked Bend High School because it was so different from Corvallis High. I did not take any science courses there, but those classes I remember most was journalism, speech, and choir. I had an outstanding speech teacher; in fact, I spent an afternoon with her about 30 years later when I was visiting my Dad just before he died. She remembers my playing the role of a 'doctor' in the senior class play. The choir was very active and we sang in many local events (I sang 1st tenor). Journalism was my main interest and I was active on the school newspaper as a junior, and they too wanted me to stay and be sports editor. I decided to take my senior year at Bend rather than return to Corvallis High; however, I spent the summer with my mother and did various jobs during the summer.

One Sunday during my senior year my Dad and I came home from church; he turned on the radio and we heard that Pearl Harbor was bombed. The next day we crowded into a classroom and heard the President ask Congress to declare war on Japan. Many people thought that we would defeat Japan in six weeks! There was much confusion, but the impact of the war was to come later. My Dad decided to visit his mother in Florida since he had not seen her for years and we didn't know what the future was going to bring. On Christmas Day in 1941 we boarded a bus and headed for Chicago. My Dad's sister lived there and we spent a couple of days there, including a tour of the Chicago Tribune newspaper. Now, I had been around the newspaper offices in Oregon, but it was nothing like the Tribune. We then headed to New York City and spend New Year's Eve in

Times Square, where we read where Oregon State University had defeated Duke University in the transplanted Rose Bowl. We saw many of the sights of New York including television (TV). My Dad went in one room and I saw him on TV; we then reversed positions for my first appearance on live TV. We then went to Washington, DC. Security was tight at this time since we learned later that Winston Churchill was visiting the president, Franklin D. Roosevelt. We stopped at western Virginia where my uncle and family lived. This was my first and only visit with him. He was a minister in the Brethren Church. We then headed for Florida to near Sebring. It was my uncle's farm where my grandmother and my older cousin lived. My cousins were going to take me out to collect baby alligators on their property, but the weather was too cold. I picked oranges from a tree for the first time. My Dad's sister was visiting there and we drove down to Key West. I really didn't react to the coral reefs or the mangroves, but it was a beautiful drive. My Dad got chiggers – little did I know that one of my best friends on the biology faculty at California State University, Long Beach, would work on this animal group. The situation in the south USA was a shock. Seeing separate drinking fountains, separate bathrooms, and segregated sections on buses. Fortunately, times have changed.

We then headed across the country to Los Angeles. We stopped in El Paso, Texas, and took the street car into Mexico for a couple of hours (My father was still a Canadian citizen and when he applied for US citizenship they learned that he had gone into Mexico and reentered US 'illegally.' They started to deport him because of his illegal entrance back into the US from, guess where? Mexico. Fortunately, he was not deported). We spent a couple of days in the Pomona, California area. Orange groves all around; the smell of the blooms and blue, blue skies with

temperatures in the mid-70's. I was impressed. On our way back to Bend, the bus driver gave us a lecture of where the Shasta Dam was being built in northern California, and what areas would be covered with water (including the highway we were on). I always look with interest at Shasta Lake when we drive to Oregon and Washington. Back to Bend 3–4 weeks later, and back to school. A trip like that was a rare experience in those days. I did get some appreciation for the vastness of the USA as well as how climate and vegetation vary from region to region. Graduation was in May. So far the war had not really affected us.

I spent the summer with my mother. My brother, Gene, was taking civilian pilot's training; he later became a civilian instructor in the air corps. He had some time off and we worked at a saw mill where we stacked lumber (15 feet high) for drying. Gene had to leave and I changed jobs – building wooden man hole sewer covers for an army base outside of Corvallis. Years later, I saw those man hole covers. I then became a hotel bellhop. I entered Oregon State University as a journalism major. I worked on the university newspaper as an assistant night editor. We put the paper to bed one night a week (until 4 am). I took geology from the father of one of my good friends, who was my baseball buddy. We went to the softball games together and we went to Salem, Oregon, to see our first professional baseball game. We ran into a Salem boy who we met at summer camp. Later we became laboratory partners in graduate school. I didn't care for physical geology but I liked paleontology. In fact, I wrote my English term paper on paleontology. I dropped out of journalism after the first semester; it was not like high school journalism.

The Army Draft and University of Oregon

After I decided not to enter journalism as a career, I went to the campus counselor for assistance. I was assigned to a senior who gave me different tests. He determined that the best bet for me was to become a high school social science teacher! It turns out he was training to be a high school social science teacher! So much for counseling. Things began to change on campus during the spring (1943). Male students were being drafted out of the classroom. I took two math classes during summer school to improve my chances of entering officer candidate school. I worked at the army PX (shopping center) and sold beer to the soldiers (at age 19). I went to Portland on Bastille Day (14 July) for my army physical. I had memorized the eye chart, but failed the eye test anyway. On the bus back to Corvallis, my friends felt so sorry for me. I cried when I got home, but my mother said, "There will be a need for educated people after the war."

As a freshman at Oregon State, I had taken psychology. I considered this field and I went to see my psychology professor (she lived about five blocks from me). She said that if I wanted to become a psychologist I had to go to the University of Oregon since Oregon State did not offer a degree in psychology. That was my primary reason for going to University of Oregon. Just before heading for Eugene, Oregon, Bob, John, and I took a bicycle camping trip to Florence, Oregon. That was my first visit to Lake Cleawox. We try to go there every time we are in Oregon. It is one of my favorite spots in Oregon. I have a series of photos taken over the past 45 years which shows the development of a forest on the enormous expanses of sand dunes.

Just before heading off to the University of Oregon in the fall of 1943, Bob and I were looking at the Oregon State College catalog.

Bob had also become 4F (unacceptable for military service) because of eyes. We looked at the requirements for the Ph.D. program; we thought it would be neat to get a Ph.D., and maybe sometime we could become a college president! (Fortunately, neither of us did.)

The dormitory was a fraternity taken over by the university. Most of the male student body was made up of freshman who were waiting to be drafted. They didn't study much. Since I could not take any upper division psychology courses, I enrolled in general biology. I had an outstanding professor and I really enjoyed the course. I decided then and there that I wanted to get a Ph.D. in biology. The next semester we studied invertebrate zoology. I made a large chart which compared how the different phyla lived, reproduced, etc. I must have included polychaetes, but I do not recall them. I spent much time dissecting the earthworm, including a detailed study of the circulatory system. I then explained the system to some of my fellow students. I continued taking math and in my fall semester of my junior year, I took chemistry, physics, and calculus. I got so tired of working math problems in the three classes. Where's biology? I lived in a private home and was a house boy for a sorority. I washed pots and pans, and received my meals and \$5.00 a month (my room was \$12.00 per month).

The effect of the war was really evident my first two years at University of Oregon. There was about 4 to 5 women for each male. (Prior to the war men outnumbered women 2 to 1). Gasoline was rationed as were meat, canned goods, sugar, and shoes to mention a few. Actually, I really wasn't affected too much by the war. I had to take ROTC my sophomore year; I couldn't march and I ended up with the only D grade in my life. My mother moved to Los Angeles. My dad could not get gas to make the trip so he went to work in the box

factory. I joined him at the end of the school year. We didn't make boxes, but the factory cut pine wood which was shipped to Washington, California, or elsewhere. We cut the wood for orange crates. Years later, Dr. Olga Hartman gave me 2 orange crates for my book case when I went to the University of Southern California (I still use them!). I often wondered if I had stacked the pieces of wood for those boxes years previously.

I only took a semester of calculus and substituted botany in its place. Mr. Sipe was the professor and he played an important role in my life as will be seen later. I spent the summer with my mother and worked on an experimental farm in North Hollywood. It wasn't very scientific; I mainly watered and pulled weeds, but it was fun working outside (no smog those days). At this stage of my life my mother thought that I would become a botanist. I had planted a vegetable garden at our home in Corvallis. In my senior year I began to think about grad school. For the most part the professors at University of Oregon were not much help since the department emphasized pre-med. I did take an undergraduate research problem, and I chose to work with termites. Research wise, I didn't accomplish much except I was able to get a stack permit in the library (there was no such thing as open stacks in those days) and I learned about scientific literature. I did benefit from it; I gave two seminars on termites and I have advised people about termites over the years.

Oregon Institute of Marine Biology and High School Teacher

As I entered the spring of my senior year, I knew what I wanted to do but I did not know where. As I mentioned before, my botany professor, Mr. Sipe, was a person who I would talk with. He made two suggestions which turned out to be extremely important.

He knew that I was interested in invertebrate zoology (the invertebrate professor had a heart attack and was on leave). Mr. Sipe had heard that Oregon State University was going to reopen the Oregon Institute of Biology (OIMB) (the land was owned by University of Oregon, but nobody there was interested). He suggested that I go to Corvallis and talk with Dr. Ivan Pratt, who was going to be the director. My meeting with Dr. Pratt was very friendly, and I decided to go to the OIMB right after graduation. The other suggestion from Mr. Sipe was that there was a shortage of high school teachers and that I could get an emergency credential. I applied and accepted a position at Baker High School in Oregon, to begin in the fall of 1946.

The six-week session at OIMB was an exciting time for me. I took invertebrate zoology from Dr. Pratt and marine algae from a paleobotanist. I had always been interested in the ocean, and the opportunity at OIMB put things into focus for me. The course emphasized identification of marine invertebrates, more or less like my course in invertebrate systematics at California State University, Long Beach. The weather was horrible that summer. The dormitories were barracks which were built by the Army Corps of Engineers in the 1920's, when they built the breakwaters at the entrance of Coos Bay. The barracks were then used by the Civilian Conservation Corps during the 1930's. At high tide the water was below my bunk! They moved the building during the winter of 1946-7. The area was filled in by natural deposition during the 1950's. Chuck Cutress was my best friend. The first animal I keyed out was an isopod; the first polychaete was *Halosydna brevisetosa*. On the mud flat trip, we saw a *Neanthes brandti* that measured over 1 meter in length crawling over the surface. Dr. Pratt took movies of it. My term project was making a quantitative comparison of the mid-tide animals of a

protected rocky beach with an unprotected one. Dr. Pratt wanted me to break my contract and start working on my master's degree at Oregon State University. I said I did not want to break my contract, but that I would come back to OIMB the summer of 1947 and start my master's with him that fall.

I took three education courses in August 1946 at Oregon State. I didn't study very hard. My childhood friend John was back from the war and we did things together. I took my finals on a Friday and the following Monday (Labor Day) I attended my first teacher's meeting in Baker, Oregon (400 miles away); the next day I began teaching 6 periods of high school biology!

I really enjoyed teaching biology. Conditions were primitive compared to what I had at Corvallis High School. The laboratory consisted of moveable tables and chairs and nine old compound microscopes. Sometimes I was able to get two or three days ahead of the students on preparation. Baker was a town of 9000 people. I ran into a least one of my students whenever I left the house where I had a room. Another teacher lived there also and we took our meals with the Episcopalian minister. He became a good friend. We played cribbage and golf. He and his wife visited us about 25 years ago. The low point was that I caught mumps from my students and I missed 3 weeks of school. I wasn't really sick; I prepared lesson plans for the substitute teacher who had never taken biology. I also lost \$100 salary out of my contract of \$2100. The minister offered me a 4-year scholarship to become an Episcopal minister. It was flattering, but I was not in the least bit interested. The year at Baker was fun. During Thanksgiving break, I went skiing at Mt Hood. I later got John, Bob, and Miles skiing. Miles still skis. I am hoping to go to Baker (now Baker City) next year (1999) to attend the 50th anniversary of my

students. I was offered another contract, but I thought it best if I start working on my master's. I left Baker the day after school was out. I hitch hiked to Chicago to see my brother who was now an airline pilot and my grandmother, Shatto (her husband died when my Dad was 4 years old). I had always wanted to take a long hitch hiking trip – this was it. I had hitch hiked in Oregon during WWII. I then headed back to Oregon to attend my second summer at OIMB.

I Become a Polychaetologist!

When I arrived at OIMB, one of the first things Dr. Pratt asked me was what group of animals was I going work on. I do not know to this day why I said polychaetes. Maybe it was seeing that large *Neanthes brandti* the year before; maybe I liked the challenge. I really do not know. People have asked how it happened, and of course, my decision to follow polychaetes has since influenced many others.

I signed up for research, seminar, and invertebrate embryology. I gave the first seminar which was on the polychaetes of Coos Bay. There wasn't much of a library: a few papers by J. Percy Moore, none by Olga Hartman. I used Light's Manual to identify the worms (which included a key written by Hartman). This was the first published manual which was mimeographed and had a green soft cover. The invertebrate embryology course was exciting. It was strictly a lecture course; we did have a lab, but nobody really accomplished anything. We discovered a large burrowing animal inhabiting the beach flats by the barracks. We didn't know what phylum it belonged to. I went to work on it and determined that it was an apodous holothurian – then called *Caudina chilensis*. I chose this animal to do my lab work for invertebrate embryology. I didn't really see any development – it was too late in the season. However, I decided to

do my master's on the seasonal reproduction of this holothurian. I started sending off for reprints; I received my last batch during my early years at CSULB. I made my collections, and a September collection, and started making slides. I had become frustrated with identifications of polychaetes.

Coos Bay is about 125–150 miles from Corvallis and I didn't have a car. At the same time, Dr. Pratt learned that S.F. Light had died, and there was no attempt to reissue the book (this was before Ralph Smith's time). Dr. Pratt had a meeting with his graduate students (six or seven) and informed us that we were going to write a manual for Oregon. He told me "Don, you are going to do the polychaetes since you know more about them than any of us." I then made a decision: since I did not have a car and since I had been assigned to write a key to the polychaetes of Coos Bay, I might as well do it for my master's degree research. Thus, you can see how seemingly unrelated events come together – had I had a car or had Light not died, or had Dr. Pratt not decided for us to write a manual, I may never have taken up polychaetes. Who knows? We completed the manual and mimeographed it. Dr. Pratt used it in his class, but it was never published nor revised. I still have a copy. Dr. Pratt later went back to parasitology, his chosen field.

I started on my master's the fall of 1947 at Oregon State. I started working up the literature on polychaetes and I wrote my first letter to Dr. Olga Hartman. She wrote a very encouraging letter back. I also wrote the Berkeleys, and Moore; Treadwell had just died. I also wrote Fauvel; he replied in English on a postcard. I returned to OIMB the summer of 1948. I was a teaching assistant in invertebrate zoology for Dr. Pratt; in fact, I had my friend John as a student. The rest of the time I was busy collecting polychaetes. I did most of my identifications that fall at

Oregon State. I had difficulty identifying one particular worm. Finally, in desperation, I turned the worm around and considered the posterior end as the head. No problem, it was a maldanid! Later I was reading one of Fauvel's papers and he commented that many people had problems identifying this worm because its tail looks so much like a head. I did not feel so foolish then. It was exciting to write to people far way and find them to be very encouraging to me in my studies on polychaetes.

The fall semester of 1948 I was again a teaching assistant in invertebrate zoology, and in the spring I was a teaching assistant in parasitology. I visited Dr. Hartman, but I will write about that later. I took advanced parasitology and as my project, I studied the parasites of sea gulls. As I went to check the gall bladder, my partner said no parasite would live there. I found several. They were new host and distributional records, and formed the subject of my first publication. I also found an acanthocephalan which was identified by Van Cleve, the authority on this group. That summer I found larval stages in a sand crab. I fed them to mice. My friend said how stupid; trying to implant a marine parasite in a terrestrial mammal. It developed into the acanthocephalan. I repeated the experiment and it also was successful. This was another paper for me.

I learned from these two events not to accept preconceived ideas or thoughts. Both of these fellow students said that I was 'lucky.' I do not think so; I tried doing things rather than accepting the conventional wisdom that 'it won't work.' This experience lead me to develop my own trial and error science. I would rather attempt to try something than think about it. If I had not pursued this idea of trial and error science, I never would have had much luck in culturing polychaetes. I

found that students like to get their hands wet and dirty and do things.

The Hartman Years, Part 1

I remember three things about my final master's oral exam. I had many questions about the honey bee and the lymphatic system – both of which I knew only slightly. Dr. Pratt told me later that they terminated my oral exam early because I drank so much of the water that they provided, they were afraid that I would have an 'accident!' I taught one-half of the invertebrate zoology course and Dr. Pratt taught the other half during the fourth summer at OIMB. I expanded my efforts at collecting syllids from the Oregon coast during the last summer.

I had written Dr. Hartman while in Oregon. Her replies were encouraging. She wrote that the Allan Hancock Foundation offered fellowships. I applied for and received one of them. I also had teaching assistant offers at Northwestern and Hawaii Universities. At the end of the summer of 1948 I spent a couple of weeks with my mother who had moved to Los Angeles during WWII. I went to the University of Southern California (USC) campus and to the Hancock Foundation to meet Dr. Hartman. I learned that she only came to the campus on Saturday, and I would be back in Oregon by then. The receptionist at the desk called Dr. Hartman at home, and I talked with her briefly; I heard a baby crying in the background. I was amazed when I saw the worm stacks in Room 30. Back in Oregon I made arrangements to meet Dr. Hartman in January 1949. We spent about two hours talking polychaetes. She gave me copies of her reprints. I decided then that I wanted to work on my Ph.D. at USC. I also applied at the other two universities mentioned above plus others because I needed financial support.

On the way back to Oregon I stopped at Stanford University to visit my friends Bob and Paul (more about Paul in a later chapter) who were working on advanced degrees. I went to the Biology Department, but I really didn't talk to anyone; I did see G.M. Smith working in his office; he wrote many botany texts. I stopped at Berkeley, where I met Cadet Hand and Donald Abbott, both of whom were working on their Ph.D.'s.

On the return trip to southern California I stopped at Hopkins Marine Station at the invitation of W.K. Fisher, who had retired as director. I had sent him sipunculids and echiuroids from Oregon which he used in his monographs of these two groups. I slept in the lab and was awakened by the seals in the morning. I met Ralph Smith who was working on the life history of *Neanthes lighti*. Pete Riser and Don Abbott were also there. I collected more syllids.

I arrived in Los Angeles in August 1949. I lived with my mother; she put me up in her garage (no car) since her main source of income was renting out rooms. At my first meeting with Dr. Hartman I told her that I wanted to work on the syllids of the Pacific Coast for my Ph.D. She said no. She didn't think it was appropriate for a dissertation. I then decided to work on the life history of *Typosyllis*. Since Dr. Hartman did not have an academic appointment, she could not be on my committee; however, unofficially she was my chair. I'm sure that she could have flunked me out if she thought that I was unworthy. My committee consisted of Martin, Moore, Dawson (he never had a graduate student; I was the closest to one), Sheldon, and Buchanan. The latter two died and were replaced by Garth and Mayer. I will discuss my doctoral research later.

What was it like to be the first student to work with Dr. Hartman? The environment was formal. She always called me Mr. Reish and I always called her Dr. Hartman. After completion of my doctorate, she called me Don but never Dr. Reish. She was, and always will be, Dr. Hartman to me. My first job as a Hancock Fellow was to type the list of polychaete genera which was used in her catalog of the polychaetes. I then checked the alcohol in all the vials in the stacks. These two jobs took me two years. I then started sorting samples for her. As a Hancock Fellow, I had to work 12 hours a week. They paid \$100.00 per month plus tuition.

I was amazed by Dr. Hartman's library and especially her catalog to all the references to polychaetes. I copied the syllid catalog (which I later gave to Fred Piltz) and later the nereid catalog. You could set your clock by her work schedule. She came in at 7 am and left at 11:30 am. I then had Room 30 to myself until 9:45 pm, when the doors of the Hancock Foundation were locked. Hancock was open until noon on Saturdays and never on Sundays. Dr. Hartman never took a break. She looked at worms for about 2–3 hours and typed (very fast) for the rest of the time. She never told me what she was doing or whenever she completed a manuscript. She was very receptive of my questions and the discussion usually ended up with me carrying a pile of references to my desk. However, sometimes she either did not hear my question or was thinking about something else; her reply was unrelated to my question. I could not get her back on track so I walked away and asked the question later. One day she showed me a fancy slide rule that she had bought. She asked me to teach her how to use it. I had taught others in years past how to use a slide rule. She wanted to solve her long problem then and there. She could not wait to learn the basics. She never learned how to use it.

The Hartman Years, Part 2

Fred Ziesenhenné had collected invertebrates before WWII. Hartman and I combined his collection of polychaetes with my master's thesis to produce the *Marine Annelids of Oregon*, which was published by the Oregon State College Press. It was the only paper we did together.

Dr. Hartman did not go to scientific meetings. She said that if people wanted to see her they could come to the Hancock Foundation; many did over the years, and even after I left USC she often called to tell me that so and so was going to be in town. I was grateful to her for these chances to meet many polychaete workers over the years. She did, however, attend two meetings at Berkeley; one was an AAAS meeting and the other a pollution meeting. I also heard her give a lecture when USC honored her as Researcher of the Year. She often showed me letters or manuscripts submitted to her for evaluation. I can't remember of her ever approving a manuscript as submitted.

I do not think Dr. Hartman liked Dr. Marion Pettibone. However, they had some things in common. They both taught in a private high school before starting on their doctoral work after the age of 30. Dr. Pettibone didn't want her doctoral thesis published by the University of Washington Press.

We never discussed personal matters. People asked soon after I arrived at Hancock if Hartman was married. I didn't know; it was two years or more after my arrival before I learned she was married to Anker Petersen, her artist. I admit I was puzzled since Petersen didn't come to work until 12 noon. Obviously they took turns babysitting their daughter (remember, I heard a baby crying when I talked to Dr. Hartman on the phone in 1948.). After Petersen quit his job at

Hancock, how she got her drawings made became even more puzzling (she had a microscope at home.). One day after Petersen had quit, I noticed that Dr. Hartman was wearing a wedding ring, but it was on her right hand ring finger which is customary among Danish people. Her husband was from Denmark. In later years she mentioned that the three of them had gone to Denmark. This was the only comment she ever made to me about her husband and daughter.

A few days before I married Janice, both Dr. Hartman and I went on a two-day trip on the *Velero IV* (her first trip aboard the vessel). She never mentioned my up-coming wedding during the cruise, but the crew made the usual joking comments. I had sent her a wedding invitation, but she did not attend. She did send a gift (a cook book). You can, therefore, sense that our relationship was very formal. It remained that way during the four years I had space in room 30 and for the five more years while I was in a lab nearby. According to Kristian Fauchald, Hartman became less formal in later years. She appreciated my dedicating 'Marine Life of Southern California' to her, Dr. Ivan Pratt, and Dr. John Mohr.

We had many disagreements, which were polite, but never really resolved. Most of them centered around my findings when I analyzed offspring of worms I had cultured. I published on the systematics of *Nereis grubei*, and placed 3 of her species (along with others) into synonymy. Offspring from one mating contained three of her species. She never accepted my findings. She was also unhappy when I published on the life history of *Nereis grubei* in the Hancock series. She said that I should not have published this material until I could go to Peru (the type locality) and work with the worms there. I have yet to go to Peru. After receiving my Ph. D., I cultured many

polychaetes, including *Capitella capitata*. I found offspring which matched her *C. ovincola*. I also found hermaphroditic *C. capitata* as well as the traditional form. All of these from a single female! Hartman said that it was impossible. What would she say about Judith Grassle's work with *Capitella*? I was timid about pursuing the speciation problem with *Capitella* in view of the 'Queen's' comments. Don't get the wrong impression. I deeply respected Dr. Hartman, and I am grateful for the many things she did for me. I am attempting to capture the mood of the time.

One day in August 1951 I walked into room 30 and there was a pallet containing books. It was Dr. Hartman's 'Literature of the Polychaetous Annelids,' which she published privately. I purchased the first copy, which I still have. This book saved me countless hours of typing reference cards. I could now just make notations in the book. I think the availability of this book was one of the main reasons why Keith Woodwick chose to work on polychaetes. He arrived at USC a couple of weeks after it was published. He had to choose between polychaetes and crustaceans (his only other choice). Again, little details played a major role. Keith got his students, Jim Blake and Todd Bridges (and others, I think), to work on polychaetes.

Hartman Years (Conclusion) and My Dissertation

As I was completing my dissertation, Dr. Hartman tried to obtain a position for me. She wrote to many people, including Waldo L. Schmidt, who was then Director of Zoology at the United States National Museum of Natural History. Nobody had an opening. While I was completing my dissertation, I went to work for the California Department of Fish & Game on funds obtained from the newly formed Water Pollution Control Boards (name changed later). It was a

seasonal aid position which was good for only nine months (more later). I was newly married, had my Ph.D., and was unemployed (despite my temporary job, I had no prospects for a permanent position). Dr. Hartman obtained a temporary position for me as her assistant. My duties were to take benthic samples from the harbor to Catalina Island and sort the samples into animal groups. This was Dr. Hartman's last 'official' dealings with me. I held this position for a few months until I went to Alaska (more later).

As I indicated earlier, I had wanted to study the life history of *Typosyllis*, but since I could never get adults to eat or reproduce, I switched to *Nereis mediator* (as it was known then). It occurred in the same niche, and would eat and reproduce in the lab. I had two frustrations with this species: (1) it is only sexually mature for 12 hours and (2) I couldn't get the trochophores to eat. The first problem was solved by having 50–75 worms in separate petri dishes. I examined them daily and watched for early signs of sexual metamorphosis. That problem was solved. The latter problem took me one year to solve. Trial and error research played a role. I tried many different foods to no avail. I tried to construct a plunger jar system like the one devised by D.P. Wilson. I didn't have the physical set-up to build this system, but I used his concept. I placed 5-gallon jars on the window sill (north facing) with sea water and connected to a compressed air system. Since the trochophores of this species swim for several days and do not feed, by the time they were old enough to feed, there was a growth of phytoplankton on the side of the jar. The larvae fed and I had the basis for my dissertation. I used this technique with *Nereis grubei*, as it is now called, in the 1960s and again in 1998. As I indicated earlier, I was able to correct many taxonomic problems with southern California species of *Nereis* by studying the variability of offspring. It also

pointed out the usefulness of life history studies in solving taxonomic problems – a subject of my paper in the Hartman memorial volume. There is continual need for this type of study. The year I spent trying to get larvae to eat proved to be well spent; not only for completion of my dissertation, but also for culturing additional species by me and my graduate students over the years.

1951 was an important year in my life, and also a sad one. I will present events in a more-or-less chronological order. I mentioned my friend, Paul. He had lived a block from me in Corvallis, but we were not close friends then. He moved to San Diego late in 1950 and we did many things together. I bought his 1935 Buick sedan from him, which was my first car. One Saturday in February we decided to go skiing. We didn't know where to go so we went up Highway 39 which ended at Crystal Lake, but there is no skiing there. Then we went to Hollywood, rented skis and learned that there was skiing at Mt. Waterman. On Sunday, 25 February 1951, we headed for Mt. Waterman. Ski conditions were poor, and Paul met a young lady. She learned that we were from Oregon. She called her friend over who had graduated from University of Oregon. That was Janice, my future wife, and the rest is history. That night she told her cousin that she had met the man she was going to marry!

I had already passed my French exam at Oregon State and I took the German exam in January, but failed. I began studying German night and day and I passed the exam in May. I was then able to take my preliminary exams in July. Hyman's Volumes 2 and 3 came out in June and I spent much time studying her three volumes of *The Invertebrates*: Vol. 1 - Protozoa to Ctenophora, Vol. 2 - Platyhelminthes and Rhynchocoela, Vol. 3 - Acanthocephala, Aschelminthes, and Entoprocta. Hyman coined the term

Aschelminthes to include six phyla but she never discussed their relationships. I figured that Dr. Mohr would ask a question about the relationships between them. I guessed correctly. Written exams were four hours each for four days, followed by an oral exam. During the orals, Dr. Buchanan stayed for a few minutes then signed his ok. Dr. Martin asked me lots of questions about bioluminescence, of which I knew little.

1951 Concluded and Beyond

The Pacific Division of the AAAS met on the USC campus in June 1951, while I was busy studying for my preliminary exams. This meeting was an important influence on my life, which, in turn, affected the lives of many of my students. Curtis Newcombe arranged a symposium on water pollution at the meeting. Dr. Mohr spoke on protozoans as indicators of pollution. I just recently learned he filled in for the originally scheduled speaker. The State of California had just established the State Water Pollution Control Board (as it was known then), and California Department of Fish and Game had the responsibility of conducting biological studies. CDF&G approached Dr. Mohr to head up the studies in southern California. He, in turn, selected Bob Menzies, Howard Winter, and Chuck Horvath to conduct surveys in Los Angeles/Long Beach Harbors, San Gabriel River, Newport Bay, San Diego Bay, and Avalon Bay. They were hired as seasonal aids which limited their employment to nine months. Horvath obtained a small orange peel bucket from Scripps. These studies were the start of benthic studies at USC. I identified the polychaetes from these surveys, and later became a seasonal aid after their nine-month period was completed. My first study was to survey Alamitos Bay, the beginning of my long interest in that body of water. I was responsible for writing up many of the reports, two of which were published in the CDF&G quarterly. I later surveyed the

biota of the San Gabriel River after it was dredged (there was no benthic life in 1952); this was my first pollution abatement publication.

Continuing the happenings of 1951; I began to write up my dissertation. In August my friend (since childhood) Paul was drowned in Baja California; a rip-tide claimed him. He was an only child and I drove his parents back to Corvallis. It was a sad trip. His father had initiated the Microbiology Department at Oregon State.

Early in 1952 I proposed to Janice and we set a June wedding date. Jerry Barnard went with me when I selected the ring for her. I thought that I would have my dissertation completed before the wedding. Ha! (I finished in December). Curtis Newcombe, who worked with the U.S. Public Health Service, suggested that I apply for a grant to study the relationship of polychaetes to pollution. Dr. Mohr served as the principle investigator as required by USC. It wasn't funded until mid-1953.

I had applied for a faculty position at Cal Poly San Luis Obispo, but I later withdrew my name because I did not qualify to teach microbiology. Mid-September of 1952 I was offered a faculty position at the new campus of Cal Poly Pomona. Janice had just accepted a position at Los Angeles City Schools. I turned the job down because I didn't think my 1935 Buick could make the daily 60-mile round trip. Jerry Barnard took the job and stayed there one year. My nine month CF&G seasonal position terminated in early 1953. Dr. Hartman obtained a temporary appointment for me with the Hancock Foundation to collect and sort benthic samples as I had described earlier. She was impressed with what we had done with benthic work in the bays and harbors.

There was considerable interest in the Arctic Ocean in the early 1950's, probably due to the cold war with Russia. Personnel at USC were actively involved with biological studies at Point Barrow, Alaska. John Mohr had spent the summer of 1952 at Point Barrow and in 1953 he took Chuck Horvath, Jerry Barnard, and me with him to the Arctic. It was my first real experience in an entirely different zoogeographical area. It became clear to me how important it is for a biologist (at least one interested in whole organisms) to visit and study diverse biological regions. I flew by myself to Point Barrow on military planes. At Fairbanks I got on the wrong plane and flew to Barter Island, which is on the Alaska-Canadian-Arctic border. We then flew to Point Barrow at 1500 feet along the Alaska Arctic coastline. It was quite a trip, especially since I had to wear a parachute (with no instructions). The others were envious of me since none of them had flown that route. Jerry and I worked together, especially with the biota of Nuwuk Pond. A pond which was freshwater for the top 6-7 feet with fresh water fauna, and the remaining 10 feet to the bottom was saline, with marine fauna. Water froze to 6-7 feet each winter, hence the fresh water floated on top of sea water. We did a paper on this, and I did two others based on my Arctic experience. We returned to the warmth of southern California and I began to work on the U.S. Public Health Service grant (EPA didn't exist until 1970).

Conclusion of My USC and Hancock Days

I had a pollution grant from the US Public Health Service with a 3-year initial period and a 2-year continuation. The total for the five years was \$35,000 which included USC's overhead. My salary was \$5000 a year, which ended up as \$6500 at the beginning of my 5th year. I conducted three benthic surveys in the Los Angeles-Long Beach Harbors in 1954 and one in 1955. Many people helped me take the samples,

including Jerry Barnard, Keith Woodwick, and my brother. They helped without pay. Results of the 1954 surveys were published in the Hancock Occasional Paper series in 1959. I divided the harbors into five ecological zones based on species composition. These divisions were based on my understanding of the animals and their environments, and not on any statistical analysis. The uniqueness of the publication is that I was able to include all the raw data. Years later, Don Boesch used my data for his EPA-US Army Corps publication on cluster analysis. My results and his cluster analysis agreed almost 100%. I have often expressed my opinion that a person knowledgeable of the environment does not really need statistics. I do, however, realize that statistics have become a necessity today. I think back to my dissertation days when I had to use a slide rule to do chi-square analyses.

Bob Menzies had gone to Scripps on soft money after completion of his doctorate. He told me that Bob Parker was conducting a survey around the mouth of the Mississippi River and needed help in identifying polychaetes. It was my first consulting job; I was paid \$3.00 per hour. I was still in Hartman's lab and I didn't tell her what I was doing since she did not believe in getting paid to identify worms. You should do it for the love of the worms! In that collection I found a new species, *Cossura delta*, which I described. Hartman never commented about the species nor questioned the source of the material. It was the first new species that I had described on my own. Chuck Horvath had a consulting job in the West Basin of Los Angeles Harbor, where a creosote company stored its pilings. He turned the job over to me when he went to the Arctic. Wooden blocks were suspended in the water and checked monthly for wood borers. He had used weights, but when I needed additional ones, I used a gallon jar. In late 1953 I looked

at the mud which had accumulated in the jar and discovered *Neanthes caudata* which later became known as *N. arenaceodentata*. Herpin had published on the earlier stages of the worm and I completed the life cycle and published the study in Pacific Science. I then placed gallon jars all around the harbor and studied polychaete settlement and related it to areas of pollution. I published the findings in California Fish & Game. Many of my ideas for the use of gallon jars came from the monograph of Gunnar Thorson. I met him on three or four occasions; he was a very dynamic person and one who would talk with any and all.

During one of these talks he wondered why polychaetes were so numerous in the benthos of southern California but not in the Danish seas. I had read some of Petersen's benthic studies of the early 1900's, and, believe it or not, one morning while taking a shower, the answer came to me. The Danes used a large screen to wash their sediments and we used a finer mesh. I then loaded up my car with gallon jars, formalin, and an orange peel bucket, and headed down to Alamitos Bay (Basin 1 had just been dredged from land a few months earlier). I washed the sediment through a series of Tyler sieves, and identified and counted the animals retained on each sieve. I published the results in Ecology and this paper is still being cited. No one had ever thought of the importance of the mesh size before. Maybe I should take showers more often!!!

The five years I was on the US Public Health Service grant afforded me many opportunities to do exploratory research. I cultured many different polychaetes including *Capitella*, *Ophryotrocha*, and *Ctenodrilus*. As I had written earlier, I realized something was strange about *Capitella*, but I really didn't grasp the picture. I was also able to publish many papers during

this time, and I believe that it gave me the opportunity to develop a 'publication' habit which has resulted in my publishing at least one paper per year since my first one in 1950.

Jerry Barnard and I ate lunch together for several years including our post-doctoral period. We talked about many things and thought it would be great to do a 'Light's Manual' for southern California. We discussed how we would do this; of course we never did, but it was always on my mind and was the seed for my 'Marine Life of Southern California.' More on this later.

I Accept a Position at Long Beach State

In the mid-1950's, University of Hawaii received a grant from AEC to determine what plants and animals were present on Eniwetok Atoll, Marshall Islands, at the site of the atomic and hydrogen bomb tests. Dr. Robert Hiatt was the director of the project and he had offered me a graduate assistantship for my doctorate years earlier. I was asked to collect and identify the polychaetes and leave a voucher specimen of each species for the museum at Eniwetok (years later, Alan Miller used these species to verify his species in connection with his ecological studies). Yale Dawson and John Garth also participated in the study. I made two trips of one month each in 1956 and 1957. This opportunity introduced me to another strikingly different environment much like the Arctic experience had done earlier. I also collected polychaetes at Bikini and Majuro Atolls. The beauty of the coral reefs is unsurpassed, especially in the lagoon. You can see so much more there and you do not have to worry about the surf. I have since seen coral reefs from other areas but none of them compare with the lagoons in the Marshall Islands. My study resulted in two papers in Pacific Science including five new species (two nereids, two sabellids, one serpulid).

On my first day at Eniwetok they flew me around the atoll. The first bomb crater I saw was caused by an atomic bomb; the second one by a hydrogen bomb, which completely destroyed a coral rock island of about one square mile in size. It wasn't until the second year that I saw a conventional bomb crater from WWII; I then could appreciate the magnitude of the impact of atomic bombs [I have since visited the two bomb sites in Japan].

Here is another example of a seemingly small thing becoming important in determining my future direction. In April 1956, shortly after my daughter Lisa was born, John Mohr and I went to Cincinnati to attend a week long fresh water pollution meeting. They found time for me to speak, giving the last paper on the last day. I met, among others, C.M. Tarzwell, who arranged the meeting, and B. Anderson who did the first toxicity studies with *Daphnia* in the 1920's. Tarzy worked for the U.S. Public Health Service (there was no EPA then). He later came to visit me while I was still at USC. I introduced him to Los Angeles-Long Beach Harbors and discussed the possibility of setting up a marine pollution lab in southern California. A few years later he became the Director of the EPA lab in Rhode Island and he offered me a job. My roots were now deep in southern California, and I didn't take it. Still later, he funded a grant with me to culture polychaetes. Many of my students benefitted from this grant. He also had me write the annelid section of Standard Methods. It pays to go to scientific meetings.

After my return from the second trip to the Marshall Islands, I decided not to apply for an additional US Public Health Grant. After nine years of research at USC, I wanted to teach. Positions in academic institutions were not abundant in 1958. I applied at UCLA, Cal State Northridge (just starting), and San Jose

State without success. In January 1958, Jerry Barnard was on the Velero IV and Dale Arvey, an ornithologist from Long Beach State University, was also aboard. He told Jerry that a position was opening up at Long Beach State. Jerry had applied there a year earlier, but President Pete (Peterson) informed him that they did not welcome anyone who wished to do research. However, another thing occurred which affected my life in addition to Jerry's trip on the Velero IV that January 1958; the Russians had successfully launched Sputnik. Research was no longer a dirty word at the State Colleges of California.

I applied for a position at Long Beach State in February 1958. I went to the campus for an interview and that day they happened to be laying the concrete in the basement of PH2 (the science building) where my worm culture lab exists today. I had not received any news by June, and finally I called Dale Arvey (the Chair) as to my status. They had not granted tenure to a person in science education because he did not get his doctorate; Dale asked if I would like to teach science to elementary teachers and supervise student teachers in science (remember I had taught high school biology). Since I didn't have a position for the fall, I said that I would be glad to! I was given one-year credit as a high school teacher but none for six years of post-doctoral research. My first semester in the fall of 1958 I taught science to elementary teachers at night and three General Biology labs during the day. I ran into Dick Lincoln and after looking at each other for some time, we realized that we were at Oregon State at the same time. I taught science to elementary teachers for several years. I continued my student teaching supervision until five years after my retirement.

I Establish a Research Program at Long Beach State

After a few years of teaching science to elementary teachers, the curriculum was changed in biology. Art Lockley had taught invertebrate zoology as a two semester, 3–unit course. We changed it to a one semester, 4–unit class, and I took over the teaching. It was a pleasure to be teaching it, and I made an effort to have a living animal in the lab each time. The source of most of my animals was Alamitos Bay, which is located nearby. In a couple of years, the enrollment became too large for one lab so we had two concurrent labs. Jack Anderson was my first teaching assistant for invertebrate zoology. With the interest in invertebrate zoology, we added invertebrate systematics which I taught every other spring. At the same time, we added algae, which I alternated with invertebrate systematics in the spring semester. Lloyd Finley was the only person to take all three of these classes from me and receive an A in all three. Years later, I added polychaete systematics which I taught four times. Still later I initiated the intern program in science and Ken Schiff was my teaching assistant for this. One final course that I taught was oceanographic techniques, through the Ocean Studies Consortium. Students from Fullerton, Northridge, and Costa Mesa also joined Long Beach State students. The class met all day on Fridays and we spent a great deal of time on the Nautilus.

As I described earlier, I had conducted a survey of Alamitos Bay for California Fish and Game, and had published a paper on this study. While still at USC, I used Alamitos Bay as a more or less clean area for comparison with conditions at Los Angeles–Long Beach Harbors. A fortunate thing happened in that the City of Long Beach began to dredge and build the Alamitos Bay Marina. Again, I took

advantage of the opportunity and while still at USC, and saw it as an opportunity to study succession in the subtidal environment. My first year at Long Beach State, I applied for an NSF grant to study succession not only in Alamitos Bay but also in the marinas being constructed in Marina del Rey, Oxnard, and Ventura. I obtained the grant – a sum total of \$21,000, the largest single grant to Long Beach State at the time. Al Stone became my first graduate student. I published several papers as a result of this grant. Studies included community development on the subtidal benthos, settlement on boat floats, and settlement on rocky jetties in Marina del Rey and Oxnard. I had just completed my study of the subtidal benthos in Alamitos Bay when the bay was hit by a severe red tide. The dissolved oxygen dropped to near zero. Again, I took advantage of the situation and studied the effect of the red tide on boat floats and subtidal benthos. California Fish and Game Quarterly published the paper.

In the early 1960's I received a grant from the National Institutes of Health (NIH) to study the effects of environmental variables on the species that I had used as indicators of pollution, namely *Capitella capitata*, *Dorvillea articulata*, *Neanthes arenaceodentata*, and *Nereis grubei*. My first attempt was unsuccessful, but in the meantime I had gone to New York to attend the First International Oceanographic Congress; I used the opportunity to stop at Washington, DC, and meet the people at NIH and explain what I wanted to do. The second attempt was successful. One of my friends at USC (now at Cal Tech) had worked out a method of controlling the concentration of dissolved oxygen (DO) in an Erlenmeyer flask. I used over 1000 Erlenmeyer flasks, many of which I still have. Anybody want any of them? I established the culture of *Neanthes* from six worms collected in Los Angeles Inner Harbor in 1964. This was the

beginning of my lab culture of this worm which has been in continuous culture since that time with no new bloodlines or genes added. Tom Richards and Jack Anderson helped me on this NIH grant. The technique of controlling the DO in the flask was used by many of my graduate students. Not only did we measure survival, but sublethal effects, burrowing in *Limnoria*, amino acid compensation, hemoglobin compensation, and others. The use of this method was the beginning of our studies of sublethal effects of environmental variables which has continued to this day as growth rate of juvenile *Neanthes*.

I go to Europe

I made the first of many trips to Europe in 1962. I was asked to discuss a polychaete toxicological test at the First International Water Pollution Conference in London. I also presented a paper on the offshore State of California pollution study of 1955-59. The authors, Tibby and Barnard, could not make the trip. I took a 707 to Copenhagen with a midnight stop in Greenland. Wheeler North introduced me to the underground subway system in London. In those days you had to spend two weeks overseas otherwise the air fare was much higher. I made a trip to Plymouth and renewed by acquaintance with D.P. Wilson. I spent the weekend with Robert and Mary Clark in Bristol. I also went to Gothenburg to visit some American friends. The conference was next to Westminster Abbey and I walked through it each day on the way to the conference. I was startled to see the grave site of Sir Isaac Newton. I flew back to Copenhagen and went to the marine lab where I spent some time with Gunnar Thorson.

My second trip was four years later. I was asked to present my DO studies with polychaetes that I had used as pollution indicators to the 3rd International Water

Pollution Conference in Munich. I flew to Paris, saw some of the sights before flying to Marseille, where I met Gerard Bellan and his wife Denise Bellan-Santini. I went on to Monte Carlo and lost a few francs at the casino. Gerard Bellan was in Munich for the conference, and he discussed my presentation with me. After the conference I went to Amsterdam. As strange as it seems, the air fare to Europe today is about the same as it was then.

Trip number three was my first of several associations with FAO, the Food and Agriculture Organization of the United Nations. They sponsored an international pollution conference in Rome. I was also involved in a work shop associated with the conference. I presented a paper there on the use of polychaetes as indicators of marine pollution. The Bellans were there also. They had spent the summer before in Long Beach with me. I made my first of three runs at the Circus Maximus, the chariot track of Roman Days. I never managed to complete a lap in any try.

My 4th trip to Europe in 1973 was a very busy one. I attended an invertebrate development conference in the former Yugoslavia organized by John Coslow (Duke University). I took living *Neanthes*, *Capitella*, and *Ctenodrilus* with me to demonstrate the larvae. I then went to another Yugoslavian city to present a paper and chair a session at the Medical Oceanographic Conference. I was elected Vice President of the group. Next stop was Paris, where I participated in a work shop in preparation for a meeting the next year. I had flown to Marseille where I met the Bellans. We then traveled to Cherbourg where Gerard's parents lived. I saw the door to the lab where Herpin studied the early development of *Neanthes* and other polychaetes, but didn't go inside.

There were many more trips to Europe in the 1970s, mostly to France, Italy, and Yugoslavia. In 1975, Janice, the boys, and my mother went with me to Rome where I had another workshop. We then drove through northern Italy, France, Belgium, the Netherlands, and then to England before coming home. The longest trip was the one with my family; four weeks. The shortest was to Gothenburg, Sweden, to participate in an FAO workshop (it lasted only two days). Nearly all my trips to Europe have been paid by some organization and I think the primary reason was related to my studies with polychaetes and pollution. Gerard Bellan was about the only other person in the world who realized the importance of polychaetes in environmental studies at that time.

Research Grants and Major Contracts

I have already written about my first research grant from the US Public Health Service, studying the relationship between polychaetes and pollution. This grant had a significant influence on my professional life. I have no idea how many publications, invited presentations both domestic and foreign, and consulting jobs resulted from my five-year study at SC.

Just before joining the faculty at CSULB, the City of Long Beach began to develop the Alamitos Bay Marina by dredging (interestingly, if the City had attempted to start this development today, it would not have come to pass since they dredged wetlands). I saw this development as a golden opportunity to study succession in the subtidal environment as well as on jetties and boat floats. I submitted a proposal to NSF my first semester at CSULB which was funded (three years, \$21,000 total). Al Stone, my first graduate student, helped me on this project plus some undergraduate students. I also included Marina del Rey, and the two

marinas in Ventura County. I found that there was really no succession in the subtidal environment. Settlement depended upon what animals were reproducing at the time of dredging (in some instances, we sampled two weeks after the area was dredged). There was a slight indication of succession on boat floats; however, again it depended on what organisms were reproducing at the time. Certainly, it was not the elaborate succession scheme that Scheer had published for Newport Bay. I was visiting Ventura City Marina and I met the contractor and he asked me what to do about the extensive bloom of algae (*Ulva* and *Enteromorpha*). I said nothing; it would be gone in two months. I saw him two months later and the green algae was gone. He said he thought that I had been crazy to suggest nothing! Many publications resulted from this three-year study, plus consulting jobs at Ventura City Marina and Marina del Rey.

The next grant was from NIH to study the effect of environmental variables on polychaetes used as indicators of pollution. This was the study that resulted in my purchasing 1000 Erlenmeyer flasks. Jack Anderson and Tom Richards, plus some undergraduate students, helped me on this three-year study (\$35,000). The technique of controlling the dissolved oxygen in the flasks was the key to this research. I had gotten the idea from Bill Hildemann from USC days (actually he did the work as a graduate student at Cal Tech). Many graduate students used this technique including Tom Richards who used it in his Ph.D. work at Maine.

Following the appearance of Rachael Carson's book 'Silent Spring,' in the early 1960s, there was considerable concern about the effects of DDT on organisms, including those in the marine environment. US Fish and Wildlife Service requested Ken Maxwell and I to submit a proposal. We did; we studied the

movement of DDT through a laboratory food chain – *Enteromorpha* to *Neanthes* to a fish (first an opaleye later a mollie). We had uptake in the algae which we fed the worm, which also took up DDT, but we had difficulty with the fish. It was difficult to feed the fish a sufficient number of worms. Jack Word and Wayne Davis helped me on the biological phase of this research. Very little of this work resulted in publication, but I learned the difficulty of conducting a laboratory food chain. I have never attempted it again, but Joe LeMay did go from worm to fish using radioactive tracers. He was successful.

I had written earlier about the influence of C.M. Tarzwell had on my life. He became the first director of the newly established EPA lab in Rhode Island (he had offered me a job there, but I never applied.). Tarzy asked me to write a proposal to develop culture techniques for many species of polychaetes which could be used in marine toxicity tests. He funded me for 3.5 years and this was one of my largest grants (\$250,000). Many graduate students worked on this grant. I assigned each student to a polychaete, and it was their responsibility to learn how to culture them and the results constituted their master's degree research. Kathy King, *Boccardia proboscidea*; Stan Rice, *Polydora ligni*; Mark Rossi, *Halosydna johnsoni*; John Shisko, *Dexiospira brasiliensis*; Doug Morgan, *Cirriformia spirobrancha* and *C. luxuriosa*; Scott Carr came a little later in the study and he conducted toxicity tests with *Ctenodrilus serratus* and *Dinophilus gyrociliatus*. Mike Martin coordinated the efforts and conducted many toxicity tests with many of these species of polychaetes. EPA published the results of this study in their publication series. The results were also presented orally by these students at the Hartman memorial symposium.

Environment Canada contacted me to study the long term effects of mine tailings on survival and reproduction on four species of polychaetes (*Capitella*, *Neanthes*, *Ctenodrilus*, *Ophryotrocha*) at 15–20 C. Canada was considering discharging mine wastes into an Arctic Sea body of water. With my previous Arctic experience, I suggested that they drain one of the thousands of Arctic slope lakes and empty the wastes into the dry bed. The permafrost would prevent the movement of the wastes. The Canadians ultimately did this but not before the study was done. Tom Gerlinger helped me on this project and some of the data were the basis for his master's thesis. We published some of the work.

Dorothy Soule established Harbor Projects at USC and she contacted me for assistance in the biological aspects of Los Angeles–Long Beach Harbor. They made benthic and monthly test panel collections. Most of the sorting and identification was done in my lab. I do not know how many people worked on this project over the next five years. I will not attempt to name them. My daughter, Lisa, began while still in high school and she later became the coordinator of the personnel. She trained the people in the identification of the invertebrates. I do not know the total amount of funds involved, but it must have been between \$100,000 and \$200,000.

Dredging and disposal of marine sediments became an important environmental issue in the early 1970's. The Los Angeles district Army Corps (via Russ Bellmer) wrote several contracts with me, including the preparation of a toxicity manual for them. This was a major effort (over \$300,000) and Joe LeMay was my right hand man over the 3- to 4-year study. We conducted toxicity tests (metals, DDT, PCBs, hydrocarbons) on 20 species of animals (seven crustaceans, five polychaetes, four bivalves, four fish). In

general, crustaceans were usually the most sensitive, with fish and pelecypods being the most tolerant. A manual was prepared and the results of some of this work was published.

Jack Anderson, Murray Dailey, and I received a contract from MMS to write a book summarizing the ecology of the Southern California Bight. Fred Piltz, a former student of mine, was the project manager for MMS. The three of us served as editors and we wrote a 1000-page book published by the University of California Press with the help of over 20 contributors. Not many books of this type have been written and I think that it will be a source of valuable information for many years or decades.

The last major contract I had was with Jack Anderson (then at SCCWRP) to study the effects of produced water from offshore platforms on five species (echinoderm fertilization test, *Mysidopsis*, *Neanthes*, Microtox® and fish). Ken Schiff, Steve Bay, and Andrew Jirik worked on the project. It is well to remember that one species is not the most sensitive to all toxicants; *Neanthes* was the most tolerant to produced water but was the most sensitive to the reference toxicant. Ken Schiff was the first author of the publication.

The foregoing account describes the major grants and contracts that I have received over the years. I have also received many consulting contracts too numerous to mention.

Some Interesting Consulting Contracts

There will be some duplication of information from what has been written earlier. Disneyland contacted Ken Maxwell and me to investigate the problem they were having with leeches in their waterways. The divers, who enter the water every day to

monitor tracks and pipes, would have leeches attached to them whenever they went in the water. They wanted extra hazard pay because of the leeches. Ken and I would go to Disneyland when it was not in operation and look for leeches. We never found any living leeches because they would poison the water just before we arrived. The director of maintenance claimed that the leeches came from the 1900 drug store on main street (no longer there). They had medicinal leeches on display which the director claimed were emptied into the water. I found three cocoons in a far corner of the river which had developing leeches. I brought them to the lab and raised them by feeding them sludge worms. A person in Maine identified the leech as one that feeds on worms and snails and is not a blood sucker. Once again it proves the importance of correct species identification. The officials at Disneyland were happy!

I do not know how it started but the radiation branch of EPA contacted me to determine if the drums containing low level radioactive wastes had any effect on the marine benthic environment. Thousands of these drums were dumped in the Atlantic and Pacific Oceans in deep water in the 1950's. I went on the first trip in 1976 off Delaware, about 125 miles. They brought a drum up from 10,000 feet. I noted serpulid tubes on the surface which I later studied and believe to be a new species (I do not know if I still have the specimens; maybe my gradual cleanup of my stuff will encounter them). Steve Bay went on the second trip off Delaware; Randy McGlade went off San Francisco (Gulf of the Farallones Deep Water Dumpsite) twice and Joe LeMay once. There did not seem to be any effect of the drums on the benthic fauna. Joe collected fish on his trip and preserved the stomachs and intestines. I was able to demonstrate that invertebrates can be identified from intestinal contents;

furthermore, I was able to demonstrate that so-called planktonic feeders also feed on benthic animals. EPA published some of my reports in the radiation series.

I was one of three (Herb Ward, editor of SETAC journal was another) asked to review data collected years earlier on the long term effects of oil well production on the ecology of the Gulf of Mexico. For two years I went to Houston about every eight weeks to evaluate the data. None of the original data collected, which showed no effect, had been published. The oil companies who sponsored the original study wanted us to reevaluate the data, draw our own conclusions, and publish our findings. We found no effect; in fact, we found that more oil enters the Gulf of Mexico each day via the Mississippi River than has been spilled in 30–40 years of drilling. Only recently have we been focusing on the effect of storm water runoff on the environment. Our reevaluation was published in the Rice University Monograph Series.

One day, Herb Ward called me and asked for me to come to the Kennedy Space Center to advise them on the effect of space shuttle takeoffs on the marine environment. NASA had not considered that cooling water from the lift-off flows into the estuary adjacent to the pad. I trained their benthic staff with the help of Tony Phillips and Tom Gerlinger. There was some effect but this did not stop NASA.

I became associated with Atlantic Scientific (no longer in existence, the owner died many years ago). Russ Bellmer had worked for them also. He specialized in the smaller contracts. Navy homeporting was one area and I did work in Los Angeles–Long Beach Harbors, San Francisco Bay, and Newport, Rhode Island (I borrowed a bottom sampler from Wayne Davis). He was on the short list, unsuccessfully, to do work in Israel and Fiji.

Marine Borers have been an area which has been a consulting field for me from beginning to recent times. I had already written about monitoring the logs stored in the West Basin of Los Angeles Harbor which led to my finding and culturing *Neanthes*. I advised Southern California Edison Company about *Teredo*-infested pilings. They wanted to use the pilings in the construction of the Redondo Beach electrical generating station. We thought (Denis Fox and I) it was not wise. There was an explosion of the intake pipe (4 feet in diameter) for Standard Oil in El Segundo. They called me in to examine the pipe (I got the job through a father in my son's Indian Guide Troup!). I crawled in the pipe a ways and found the inside of the pipe to be riddled by thousands of pholads of the same age. There apparently had been a big reproductive bloom and the larvae settled on the inside of the pipe and eventually burrowed through the wall, causing the explosion. I suggested that Standard Oil abandon use of the pipe. Unfortunately, as is often the case, I never learned what finally was decided. Rick Ware called me a couple of years ago. Huntington Harbor had a wood boring problem. It was potentially threatening their walkways.

I do not remember how many Los Angeles–Long Beach Harbor consulting jobs I have had. There were those through Dorothy Soule and Harbor Projects which I discussed earlier. I had several through Atlantis Scientific, ecology of the Navy Base through Tom McDonnell and Brown & Caldwell, ecology of the harbors based on 50 years of study and observations. This came through Karen Green and MEC Analytical.

Over the years, my former students have been good to me via consulting jobs. Included in the list is Jack Anderson, Tom Gerlinger,

Rick Ware, Tom McDonnell, and Karen Green. Needless to say, I wish to thank them.

The Graduate Students

During my academic career I had 57 students complete their master's degrees under my direction. Most of the students came up the ranks by way of taking invertebrate zoology or invertebrate systematics. Some came from other universities after completing their bachelor's degrees. Kathy King, Scott Carr, and Fred Piltz came from University of California, Irvine; Bob Galbraith from University of California, Santa Barbara; Phil Oshida and Tom Kauwling from University of California, Berkeley; Wayne Davis from Cal Poly San Luis Obispo; Ken Schiff from San Diego State University; Dave Russell from Pomona College; Tom Gerlinger from Ohio State University; Tom Biksey from University of Pennsylvania; Tom McDonnell and Terry McCoppin from Loyola-Marymount College; Eric Gonzalez from Panama University; Joe Tarfaro from Louisiana State University; I don't remember where Rivian Lande, Don Moore, Wayne Brannon, Doug Morgan, or Al Stone went as undergraduates.

What were my criteria for the selection of a potential graduate student? For those who did well in my classes, the choice was easy. I also considered such things as their curiosity, their ability to think creatively, and finally my gut feeling. I had 15 graduate students working under my direction at the same time. People would ask me how I handled this number of students. Basically, I would spend some time with them at the beginning and again at the end of their research and thesis writing. Grad students further along would help the beginners. During the time of my EPA grant, Mike Martin worked for me full time and helped a lot. Joe LeMay did the same when I had the large Army Corps grant. While I didn't spend much time with them during the

'middle' period, I would talk with them. I think that a student just starting on a graduate program needs some attention. The slow part always seemed to be writing up their research. Some were good writers and others had to go through several drafts. How did we select a topic for their research? Partly it depended upon their interest and partly on my grants and contracts at the time. For example, there were several studies on the effect of reduced DO on an organism; we then went through a period of culturing polychaetes, and lastly toxicity studies. I think the one thing I miss most since retiring is the contact with graduate students. I see many each year, but I have lost contact with others. Now for the roster (student, date of their Master's completion, thesis subject, and post-student life, if known):

1. Al Stone, 1960. Effect of rain runoff on Estuarine polychaetes. High school teacher, then manager of a flower shop.
2. Bob Galbraith, 1961. Homing in limpets. Crofton Community College.
3. Dr. Dean Bok, 1965. *Limnoria* cytology. Faculty at UCLA in anatomy.
4. Dr. Jack Anderson, 1966. Three species of *Limnoria* - temperature effects, published. Consulting firm.
5. Dr. Tom Richards, 1966. Life history of *Stauronereis*, published. Faculty at Cal Poly San Luis Obispo.
6. Rivian Lande, 1966. Movement of *Ophiodromus* on starfish, published. Retired from faculty at Long Beach City College.
7. Don Moore, 1967. Seasonal reproduction in *Mytilus edulis*, published.
8. Dr. Alan Mearns, 1967. Amino acids in *Neanthes succinea*, published. With National Oceanographic and Atmospheric Administration.

9. Joe Tarfaro, 1967. Polychaetes of Palos Verdes Peninsula. With New Orleans Police Department.
10. Dr. Don Perkins, 1968. Protozoa. Faculty at Oklahoma State University.
11. Dr. Robert Crippen. 1968. Polychaetes on boat floats in LA Harbor, published. Consulting firm in Canada.
12. Ken Hilger, 1968. *Halosydna* setal regeneration, published. High school teacher.
13. Dr. Kevin Eckelbarger, 1969. Life history of *Lyrodus*, published. Director of Marine Program, University of Maine.
14. Dr. Wayne Davis, 1969. Effect of low DO on *Neanthes*, published. At Environmental Protection Agency in Rhode Island.
15. Dr. Marty Raps, 1969. Low DO and hemoglobin compensation, published.
16. Dr. Norman Shields, 1971. Effect of pressure on protozoa.
17. Dr. Raymond Cripps, 1971. *Neanthes* physiology. In Australia.
18. John Abati, 1971. Free amino acid in stressed *Neanthes*, published. Died 1974.
19. Robin Finley, 1971. Environmental effects on *Hydroides* life history. Died 1994.
20. Tom Kauwling, 1972. Benthic polychaetes in Huntington Harbor, published. High school teacher in Long Beach.
21. Wayne Brannon, 1973. *Limnoria* and effect of low DO.
22. Dr. Steve Rossi, 1973. *Mytilus* physiology, published. Researcher at University of California, San Diego Medical School.
23. Dr. Fred Piltz, 1974. Effect of copper on *Neanthes*. With Minerals Management Service in Ventura, California.
24. Tray Schreiber, 1974. Benthic polychaetes in Huntington Harbor.
25. Lee Hill, 1974. Polychaetes in Long Beach Naval Station.
26. Mike Martin, 1974. *Neanthes succinea* population comparisons.
27. Glenn Reilly, 1974. *Neanthes succinea* population comparisons. High school teacher.
28. Dr. Jack Word, 1974. Effect of Zn on *Neanthes*. Consulting firm.
29. Rick Rowe, 1974. Polychaetes at San Clemente Island, published. City of San Diego Metropolitan Wastewater Department
30. Dr. John Dorsey, 1974. Polychaetes at San Clemente Island, published. City of Los Angeles.
31. Dr. Stan Rice, 1975. Life history of *Polydora ligni*, published. Faculty at University of Tampa.
32. John Shisko, 1975. Life history of *Dexiospira brasiliensis*. City of Los Angeles' Environmental Monitoring Division.
33. Doug Morgan, 1975. Life history of *Cirriiformia luxuriosa* & *C. spirobrancha*. Consulting firm.
34. Dr. Phil Oshida, 1975. Temperature effects on *Neanthes*, published. Environmental Protection Agency, Washington DC.
35. Kathy King, 1976. Life history of *Boccardia proboscidea*. Consulting firm.
36. Dr. Scott Carr, 1976. Effect of oil on small polychaetes. With United States Geological Survey in Corpus Christi, Texas.
37. Mark Rossi, 1976. Life history of *Halosydna johnsoni*. High school teacher.
38. Randy McGlade, 1977. Effect of metals on *Neanthes*. Consulting firm; first in biology then in business after a master's degree in business.
39. Steve Petrich, 1978. Effect of aluminum and nickel on polychaetes, published.

- City of Los Angeles' Environmental Monitoring Division.
40. Rick Ware, 1979. Food habits of white croaker, published. Has his own consulting firm.
 41. Tom Gerlinger, 1979. Effect of mine tailings on polychaetes, published. Has his own consulting firm.
 42. Sue Williams, 1979. Systematics of terebellids, published. Teacher and consultant in Ventura, California.
 43. Dr. David Russell, 1980. *Polydora nuchalis* digestive tract. Environmental Protection Agency in Maryland.
 44. Ricky James, 1981. Seasonal benthic population in LA River, published.
 45. Steve Bay, 1982. Chromium uptake in *Neanthes*. Southern California Coastal Water Research Project.
 46. Tom McDonnell, 1982. Effect of metals on amphipods. Consulting firm.
 47. Ann Martin (now Dalkey), 1982. Effect of sewage on *Neanthes*. City of Los Angeles' Environmental Monitoring Division.
 48. Joe LeMay, 1983. Cadmium from *Neanthes* to arrow goby, published. Has his own consulting firm.
 49. Terry McCoppin-Frohoff, 1983. Metals and reproduction in *Neanthes*. Teaching in community colleges.
 50. Karen Green. 1984. Systematics of malidanids, published. Consulting firm.
 51. Jim Shubsda, 1984. Cadmium effects on *Neanthes*.
 52. Cathy Crouch, 1984. Seasonal study of polychaetes in sea grass roots, published. Working on Ph.D. at University of California, Los Angeles.
 53. Tom Biksey, 1987. Benthic study in outer Los Angeles Harbor, published. Consulting firm in Philadelphia.
 54. Peter Striplin, 1987. Food habits of *Astropecten*. Consulting firm in Washington State.

55. Ken Schiff, 1988. Effect of DDT on *Capitella capitata*. Southern California Coastal Water Research Project.
56. Stan Asato, 1988. Effect of metals on mysids, published. City of Los Angeles' Environmental Monitoring Division.
57. Eric Gonzalez, 1988. Intertidal polychaetes in Panama. Los Angeles City Health Department.

As you will note, I have lost track of several of my graduate students. If any of you know the whereabouts of any of them, I would appreciate learning their address.

The Worm Farm

How did the worm farm come about? Like so many things, some incidental events led to the establishment of the worm farm. As I had discussed earlier, I established the culture of *Neanthes* in 1964. My students and I used the worms for a variety of experiments. In 1975 I was invited to present a paper at American Society for Testing and Materials (pre-E-47 times) sponsored symposium at Virginia Polytechnic University in Blacksburg. I presented a talk on the culture techniques with polychaetes and discussed their potential as toxicological test animals. After my presentation, two men talked with me about the possibility of shipping worms to them. One was Jim Horne (I see him every year at Society of Environmental Toxicology and Chemistry) and one from the US Navy. They both purchased worms from me – adult size – and thus the worm farm began. I also sold *Capitella* and collected *Dexiospira* for settlement studies which resulted in 2 papers with people from Harvard.

As I left Blacksburg, I noticed a sign giving directions to Christiansburg, 3 miles. I had spent a weekend there in 1942 with my Dad and we stayed at his brother's home. On my next visit to Blacksburg for a workshop, I met

my cousin. Another cousin had gone to Virginia Polytech. After completing his Ph.D., Mike Johns did a post-doc at the Environmental Protection Agency lab in Rhode Island. He worked with Carol Pesch. Carol had spent three weeks in my lab learning how to culture *Neanthes*. She had established a culture in Rhode Island and Mike had helped her. Mike then took a position with a consulting firm in Seattle. An area in Puget Sound was declared a superfund site and Mike, together with Tom Ginn and others, developed the 20-day juvenile growth test for studying effects of contaminated sediment. I was involved in some of the planning and development of this test. The test became standard and was picked up by other agencies and many consulting firms. At the present time, cultures of this *Neanthes* species are maintained by the Army Corps in Mississippi, Hong Kong, Korea, and the United Kingdom.

Why use this animal? It lives in sediment, the worms are isogenetic, readily available, and have excellent control survival. While it is not the most sensitive test animal, the use of growth as a measure is a good sublethal test. How many labs have used this animal? Presently about 80 firms. At last count the total sold over the past 25 years is nearly 400,000. This figure does not include those used by my students and me, or the tests I conducted at Orange County Sanitation District. I have prepared a bibliography of the published papers, abstracts, and some reports – it totals about 200 citations. This figure does not include the reports issued by consulting firms, since I do not have access to them.

Jim Weinberg and his wife followed up on earlier studies by Gerald Pesch and found that the Connecticut population (known as *Neanthes acuminata*) has a chromosome number of $2N=22$; my lab culture has $2N=18$;

the local natural populations has $2N=18$, but the centromere is located at a different spot in one chromosome pair. A Hawaiian population has $2N=28$. The Southern California population should not be referred to as *N. acuminata* (as indicated in the Southern California Association of Marine Invertebrate Taxonomists [SCAMIT] species list). It is a new species! No one has looked at the chromosome number of *N. caudata* (the European species which is the same general type as the others). I also found it at Eniwetok Atoll, Marshall Islands. How many similar species problems like this occur in polychaetes? I think it is widespread as shown with the *Capitella* complex.

What do I do with the money from the sales of worms? Many things. It has paid salaries to many students (and still does) and my son Jim; I started taking salary after I reached the age of 70. It has paid my way to scientific meetings, helped with publication costs for the polychaete proceedings, helped pay travel expenses for students going to meetings, helped pay for sea water, and even purchased air conditioning units for biology department offices.

What has the worm meant to me? I think that it has helped to emphasize the importance of polychaetes in marine environmental studies. I think that I have been one of the principal players, if not the principal one, in championing the cause of the importance of polychaetes in marine pollution studies (50 years). This worm has provided me the opportunity to attend meetings and given me a forum to stress the importance of polychaetes. Polychaetes have played an important role in my scientific life and *Neanthes* has been at the forefront.

Retirement Years

In the mid-1980's, I began to think about retiring. I was almost eligible for another

sabbatical. I decided to take the year sabbatical (my 3rd) and then put in the minimum time afterwards and retire. My last class was in marine natural history in May 1988. I had planned a little speech outlining my 40 years of teaching, from high school to CSULB. Suddenly, the door burst opened and about 15 of my former graduate students ran in with a banner (which I still have) congratulating me. It was quite a pleasant surprise. Marion Nipper, who was working at SCCWRP, also came and she met Scott Carr there – the rest of their meeting is history (Marion works with Scott in Texas). Two years earlier at the Southern California Academy of Sciences May meeting, they conducted a roast for me. I think the best part of the roast was the presentation of three volumes of collected reprints by my former students. Jerry Barnard was also there; it may have been the last time that I saw him.

People view retirement in many different ways. Some faculty members leave the area, some stay put but not at CSULB – I do not know what they do. There is no one answer to how one should spend his/her time in retirement. But one thing is certain – you must plan for it. Not only what you do but also what you and your spouse plan to do. Planning should also include financial planning, which should begin years (decades) before retirement. My interests, in not any particular order, include polychaetes, gardening, travel, wife and family. I chose to remain at CSULB and conduct some research, writing, taking care of the worm farm. After formal retirement, I was hired for several years to supervise student teachers. I didn't mind this since my schedule was flexible. Believe it or not, CSULB has just rehired me (recycle me?) in fall 2000 to reinstate the intern program. I had initiated this program before I retired. Again, scheduling is flexible.

Just before retiring, I became active in American Society for Testing and Materials. I co-authored the west coast mysid toxicity test protocol followed by aquatic and sediment test protocols with polychaetes. I also co-authored the section on selecting test animals for toxicity testing. I had earlier written the polychaete aquatic test protocol for Standard Methods. I was invited to conduct a polychaete workshop at the Water Environment Federation meeting in Toronto. Janice went with me, but first we attended Stan and Kelli Asato's wedding before grabbing the plane. At the work shop I met Lenore Clesceri, who later talked me into taking over the toxicity section of Standard Methods (SM). Since nothing new had been done in SM for the past few editions, there was a lot of leg work to do. I called upon former students to help me – Jack Anderson, Scott Carr, Joe Gully, Steve Bay, Joe Greene came through for me. The part coordinators (I am one of 10, plus three editors) meet each January to deal with the problems. We have met in Arizona (3X), Florida (soon to be 2X), Louisiana, Hawaii, and San Diego. I guess this is our fringe benefit. Lisa has been a paid artist for them.

Other scientific activities include attending Society of Environmental Toxicology and Chemistry meetings each year, and most of the two meetings per year of American Society for Testing and Materials. I wrote a new edition of Marine Life of Southern California (1995) and I am thinking of doing another edition (3rd) in the next couple of years. Hopefully, it will have lots of colored photos – time will tell. It is easy to think of projects. Tom Gerlinger got me a contract with Orange County Sanitation Districts to conduct sediment tests with *Neanthes*. We published two papers plus a few posters at Society of Environmental Toxicology and Chemistry. Tom McDonnell hired me on a consulting job in Long Beach Harbor, and

Karen Green is going to hire me to write about pollution history in Los Angeles–Long Beach Harbors.

I served as the editor for the 3rd, 4th, 5th, and 6th International Polychaete Conferences and will serve as editor for the 7th, to be held in Iceland in 2001. If the 8th conference is held in a non-English speaking country, I will offer my services to check the English after the paper has been accepted. Years earlier I was the editor of the Bulletin of the Southern California Academy of Science. Remember, I was an editor at the age of 10, a sports editor in high school, and was initially a journalism major in college. I guess printer's ink is in my blood!

I Look Back – I Look Forward

Obviously at the age of 76 at the time of this writing, there are more years behind me than ahead of me. How does one evaluate one's contributions to biology and science? I am no different from anyone else – I think of myself as being more important than I actually am. I think that I made important contributions in two areas: teaching and research.

As far as teaching is concerned, let us first consider the numbers. I taught 175 students in high school, 325 as teaching assistants at Oregon State University in general zoology, invertebrate zoology, and parasitology. I had 30 students as teaching assistants and instructors at Oregon Institute of Biology, 40 students as instructors at University of Southern California in general biology, and approximately 2800 at CSULB where I taught general biology (107), general zoology (86), nature study (361), marine natural history (846), invertebrate zoology (686), algae (68), invertebrate systematics (226), polychaetes (46), oceanographic techniques (35), interns (64), seminars (90) and an estimated 200 student teachers. There were 57 who completed their master's

degrees under my direction as I had named earlier.

What was the impact of my teaching on these students? As one of my high school students said one time to me, "Mr. Reish, is this your first year of teaching?" I said yes. She replied, "I thought so." I saw two of my high school students at my wife's sorority reunion in 2000. I had not seen them for 53 years. They remembered me and we had a pleasant time talking about our times 53 years ago.

At Oregon State I got at least two interested in marine biology, both of whom made a name for themselves – John McGowan at Scripps and Bill Burns, who was head of the Zoology Department at Wisconsin.

CSULB is where I believe I had the greatest impact. Over the years of sitting through probably at least 1000 classes in junior and senior high school science classes, I wondered what makes a good teacher. I came to realize that teaching is an art – it is a product of one's personality, his interest in people, his love of the subject matter, and desire to do something truly worthwhile. He must exhibit enthusiasm and sometimes be a bit of a ham. A few of the techniques I employed were singing marine biology songs, running over the rock jetties, having snail races in invertebrate zoology classes. These devices were done with a purpose in mind – another way of showing my enthusiasm for biology and teaching. As I discovered in supervising student teachers, not all people can become good teachers. You must work at it. In labs, I tried to spend a little time with each student on a one-to-one basis so that I got to know them better. I wasn't 100% successful – no one is – but I believe that I had some lasting effects on many as indicated by their interest in my story.

What was my impact on science, in biology, in invertebrate zoology, polychaetology? I think that I am perhaps the primary person to scientifically popularize polychaetes. My work on polychaetes as indicators of pollution lead to the necessity of including polychaete identification on all marine monitoring programs. The impact of the importance of sieve size in screening benthic samples facilitated this. I think that speaking up for polychaetes also helped to make people polychaete conscious. The earlier benthic studies look primitive by today's standards, but the principles were established by then. As I demonstrated with my students, you do not have to attend a marine biology research station to be able to culture marine animals. Polychaete cultures are now being established in marine countries. I get reprint requests and advice for culturing polychaetes even today. In fact, some colleagues have suggested that I go back to it. I probably will not.

I was trained as a classical zoologist, and I have described some 20 new species of polychaetes, some of which are not considered valid today. I think that my impact on polychaete systematics is minor. However, I believe that polychaete systematics is important and I will continue to support it.

Jerry Barnard and I did the first toxicity tests with polychaetes using *Capitella* in the harbor. It was nearly 10 years later before I did my second one. This lead to the EPA grant that I described earlier and lead to the establishment of the many cultures, including *Neanthes arenaceodentata*.

I began writing toxicity test procedures for Standard Methods, American Society for Testing and Materials, EPA, US Army Corps of Engineers, and the Food and Agriculture Organization of the UN. I plan to continue

this activity as long as I can make worthwhile contributions.

What does the future hold for me? What would I like to do? What would I like to accomplish? At age 76, health is of prime importance in dictating what one can and cannot do. I attempt to keep my weight steady; Janice and I try to walk two miles a day (running is a past effort for me – my joints lack sufficient cartilage). One fortunate thing is that we have enough savings and earnings so that we can do many things. We'd like to do more traveling to foreign places as long as we can. We would like to visit the Amazon, New Zealand, and Australia again, we always enjoy Europe, we would like to do a land/garden tour of Japan. Hawaii is always calling; I cannot remember how many times we have been there, but it is always great when we do. US and Canada have lots of great places to see again or for the first time. I will continue to garden and Janice will continue to knit. What about Science? My contributions to original research will be minimal if at all. I will continue to help others. My role as editor of the polychaete proceedings has been valuable to others. I am looking forward to editing the proceedings of the 7th polychaete conference to be held in Iceland this July. I will continue working with Standard Methods at least through the 2002 edition and maybe the 2006 edition. I will continue with the help of 4 others to do the annual marine pollution review for the Water Environment Research journal. I am assisting a colleague in Japan and England which may or may not result in any publication. Janice and I plan to continue to live where we do until we move to our final residence overlooking the 605 freeway.