The Luck of Walter Munk

Walter H. Munk

We have been asked to talk about ourselves. For a title, I have chosen the title of a talk given by Roger Revelle in 1982*.

I was born in Austria soon after the end of World War I. My grandfather Lucian Brunner [photo 1] was a Viennese banker with political ambitions. He ran for mayor on a reform platform and was thoroughly defeated. He became a socialist and changed the name of his bank from "Lucian Brunner" to "Österreichische Volksbank" (Austrian People's Bank), but kept all the shares.

Grandfather was intrigued by high technology. He was on the board of the "Südbahn," the railroad that developed the audacious route from Vienna to Trieste, and he built funiculars in the Dolomites. This area is now in Italy, but was then part of Austria. At the time it would have made some sense for an Austrian to become an oceanographer. Austria had inherited the northern Adriatic, down to Venice, from Napoleon, had a Navy and was doing respectable ocean research. But by the time I grew up the country was landlocked, and becoming an oceanographer made no sense at all.

Lucian's daughter, my mother [photo 2], read botany at Newnham, one of the two women's colleges at Cambridge University. It was then unheard of for a girl from the continent to go to university in England. She married my father at the end of the war. They were divorced when I was very young, and father went to live and ski in Kitzbuehel. He never held a profession, and did not have to. Mother married Rudolf [photo 3].

Engelsberg who became "Generaldirector der Osterreichischem Salinen" (President of the Austrian Salt Mines). My stepfather used to take me on inspection trips deep into the mountains where the salt is mined. This government monopoly is an ancient industry, going back thousands of years. The miners have their own uniform; their traditional greeting is: "glück auf" (literally "luck up," meaning essentially "good luck").

My youth was spent skiing and playing tennis, with no sign of any intellectual curiosity [photo 4]. We spent summers and Christmas near Salzburg in a 300-year-old farm house that had been renovated by my grandfather [photo 5]. To save me from further inaction, mother sent me to New York at the age of fifteen, where, after a year at a preparatory school, I went to work in a local bank that had been associated with grandfather’s bank. I hated every minute of it.

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To keep my options open, I lived at the International House and attended night school at Columbia University. After three years in New York I drove to Pasadena, California to get admitted to the California Institute of Technology. I was unbelievably naive. When I showed up at the Dean’s office and he said “let me see your files,” I had to reply “there are no files.” In my junior year I took some courses in geology and geophysics, and at the age of 21 abruptly transformed from an acceptable student to a good student. The combination of out-of-doors fieldwork with the challenges of the fantastic geology of California was irresistible.

In summer 1939, at the end of my junior year, I found a summer job in La Jolla, a sea-side village a hundred miles south of Pasadena, where a girlfriend was spending the summer with her grandparents. The only available job was at a sleepy Marine Station with a staff of 15 people (including the gardener). The romance did not last, but today, sixty years later, I am still working at the Scripps Institution of Oceanography [photo 6].

Harald Sverdrup [photo 7], the Norwegian polar explorer, was the Director, and Roger Revelle [photo 8] (who was to succeed Sverdrup) had just been appointed Instructor. Sverdrup and Revelle became two of my three heroes (the third is G. I. Taylor), as well as my life-long friends, with a profound influence on my life and career. I asked Sverdrup then and there whether I could become his student. He thought about it for an interminable 30 seconds and then said: “I cannot think of a single job in Oceanography that will open in the next twenty years,” to which I responded instantly, “I’ll take it.” For a while I was the Scripps student body.

Later in the year Hitler invaded Austria. My family left Austria in time; my stepfather had been a senior member of the Schuschnigg government that had opposed the Austrian Nazi party, and my mother was Jewish. I completed a master's thesis on internal waves in the Gulf of California (a horrible paper) and then enlisted in the Field Artillery as a private, expecting to see early action in Europe. I served for almost two years (some of it in the ski troops [photo 9]), but no American troops were sent to Europe. By that time a civilian group under Sverdrup had been formed at the U.S. Navy Radio and Sound Laboratory in San Diego to work on problems of anti-submarine warfare. They requested my participation. I was discharged from the Army for that purpose. One week later Pearl Harbor happened, my unit was shipped to the Pacific and suffered very heavy casualties.

In 1942, I learned of Allied preparations for an amphibious winter landing on the northwest coast of Africa. The coast is subject to heavy northwesterly swell, with breakers exceeding two meters on two out of three days. Yet practice landings in North Carolina were suspended whenever the wave height reached two meters, because of broaching of the LCVP landing craft. It was a catastrophe waiting to happen. The problem, simply put, was to pick the one out of three days when the waves were low. Sverdrup and I developed a method of wave prediction, and it worked. Later we helped train Navy and Air Force meteorologists to predict amphibious landing conditions for the Pacific Theater of War. The method successfully predicted wave conditions for the landings in Europe as "unfavorable but not impossible." This was considered the lesser of two evils as compared to a postponement by a fortnightly tidal cycle.

Back at Scripps after the war I continued with the wave work and earned some consulting money by predicting wave forces on offshore structures. A drilling rig belonging to Humble Oil for which I had calculated wave forces collapsed in a storm. Fifty years later I was admitted to the Houston Hall of Fame for pioneering contributions to the off-shore oil industry.

There were so many interesting things to do, I did not get to the Ph.D. dissertation until 1947, and then only under Sverdrup's threat of dismissal. My thesis of 19 pages was written *de novo* in three weeks and is the shortest Scripps dissertation on record. As it turned out, it is basically flawed. Thirty years later I received a call from the University Alumnus Office "about my degree" and thought for a moment that they were going to withdraw my doctorate degree. Instead I was given the Distinguished Alumnus Award. Fortunately there is no mechanism for canceling a degree once granted.

I took advantage of early, and very primitive, computers to derive power spectra of ocean waves [photo 10]. Frank Snodgrass and I discovered very low-frequency (up to 30s in period) forerunners of ocean swell. From the measured dispersion one can calculate the distances to the source region, which turned out to be an incredible 15,000 to 20,000 km. The Pacific Ocean is large, but not that large. The waves had to originate in the Indian Ocean passing by New Zealand, or something was wrong with classical wave theory. We organized an expedition "Waves across the Pacific" for tracking the wave disturbances along a great circle route from New Zealand to Alaska. The theory proved correct. My wife and I occupied the station in Samoa, living with our two daughters in a native *fale* in a village without electricity and running water [photo 11].

The low frequency swell opened a challenge to look for lower frequencies still. For a while I worked on tsunamis (so well known here in Japan) and built a special instrument as an aid for tsunami warning. And this led to my interest in tides, an ancient subject considered by most oceanographers as having been closed by Victorian mathematicians. We learned how to measure open sea tides with pressure recorders freely dropped to the deep-sea bottom, to be recalled a few months later by acoustic command from a surface vessel [photo 12](the technique has since become standard). I have recently returned to the subject of tides. Carl Wunsch and I suggested that lunar tidal dissipation provides power for mixing the abyssal ocean. The proposal was first considered as lunatic, but a year later it is considered as obvious. This is not the first time something I worked on was first considered lunatic, and soon after considered obvious, even trivial; it happened at the very start of my career to the suggestion that summer swell on California beaches was generated in the southern hemisphere. I much prefer the early antagonism to the later condescension.

Going back to the early fifties, these were the great expedition years of the Scripps Institution. Roger Revelle, who had become Director, transformed the local marine station into a global oceanographic institution. A generation of terrestrial geophysicists had developed the tools of our trade: seismic exploration, magnetic surveys, gravity surveys and heat flow. These technologies were now introduced into the marine environment, but haltingly and with great trepidation. As it turned out, each and every one of these techniques ultimately worked better at sea than on land. Their application led to the understanding of sea-floor spreading, leading to the great revolution of plate tectonics, the first meaningful theory of the formation of the Earth. The secret lies in the formation of crust along the ocean ridges, and the destruction of crust in the deep ocean trenches. Land geologists could have pounded their rock hammers for eons without solving the problem.

I participated in the *Capricorn* expedition into the south Pacific [photo 13]. Revelle was expedition leader, and various Scripps scientists were doing seismic and magnetic work and measuring the heat flow through the deep sea floor [photo 14]. We were gone for nine months (nowadays the scientific parties are flown in and out every month or two). I came home and married Judith Horton.

There were other ways in which my life changed in a profound way after *Capricorn*. The great oceanographer-meteorologist C. G. Rossby had once invited me to join his department at the University of Chicago. When I declined because I was happy in La Jolla, he told me that every oceanographer worth his salt changes jobs every seven years. Since I first came, Scripps has grown from 15 people to over a thousand; I may have seen more change by staying put.

In the late fifties, I was offered a job at M.I.T. and at Harvard (the offers came on the same day). Roger Revelle asked: what is it that you can do there that you cannot do better right here? I started the La Jolla branch of the University Institute of Geophysics and Planetary Physics (IGPP, originally IGP) and served as Director from 1959 to 1982. The timing was very fortunate. Traditionally geophysics had been conducted separately from oceanographic institutes. But the plate tectonic revolution showed that a seagoing facility [photo 15] was just the place to be. I continued my work as a "wet geophysicist."

The early theme of IGPP was *measuring* the motion of the (not so) solid Earth in *real time* (previously one depended on fossil evidence for inferring this motion. We had in mind the motion along faults, the growth of mountains, the splitting of the Gulf of California. We used the words "ultralow frequency seismology," and "ultrahigh frequency geodesy"; in fact they are nearly the same. A long baseline laser strain meter was established in the Palm Desert to obtain time series of the rate of earth strain. With the support of Cecil Green, a network of low frequency seismometers was globally deployed and is active today. The IDA global network has revolutionized the knowledge of the constitution of the Earth's interior. Leadership was provided by Freeman Gilbert, George Backus, Robert Parker, John Orcutt, and many others.

Judith helped Lloyd Ruocco design the redwood building overhanging the cliff. Thirty years later she helped Fred Liebhardt build the Revelle Laboratories of IGPP across the street from the old building, and connecting the west and east Scripps campus with a handsome cable-stayed footbridge [photo 16]. It is fair to say that the IGPP laboratories have had a significant influence on the general development of the Scripps campus. We live just north of Scripps on the cliffs overlooking the ocean. *Seiche* is home-built and has been 45 years in the making. Judith built an amphitheatre in our front yard. We enjoy having students and visitors come to *Seiche*. Judith calls it “living above the store [photo 17, 18].”

Returning to my own early work, I had become curious about irregularities in the rotation of the Earth. It all started with the assertion by Victor Starr in 1948 that changes in the global angular momentum of the atmosphere between winter and summer must be accompanied by “imperceptible” changes in the spin of the Earth. How big is imperceptible? I happened to have come across an article by M. and Mme Stoyko in Paris who had measured “minute” seasonal changes in the length of day with precision pendulum clocks. Gordon MacDonald and I put the two things together.

Not only does the spin rate change (the Earth is not a good time-keeper), the Earth also “wobbles” on its axis. The distance of the point where I now stand from the north pole will vary by ten meters from one month to another. Again this is associated with things that happen in the atmosphere, the ocean and the (not so) solid Earth. To astronomers these irregularities have been a nuisance for three centuries, a “noise” in the coordinates of the platform from which they conduct their measurements. But this noise can be exploited to learn about the integral distribution of mass and motion on the planet Earth. It is now a flourishing subject, an essential ingredient in the precision time-keeping required to make global positioning (GPS) work.

The first draft of *Rotation of the Earth* was written on a sabbatical in Cambridge. We returned there on two subsequent sabbaticals; I became a fellow of Churchill College (it means a lot that mother played hockey for one of the Cambridge colleges half a century earlier), and worked at what are today the Bullard Laboratories (Sir Edward Bullard was a close friend for many years). I eventually received a Cambridge degree, which entitles me to walk on their manicured lawns. In 1971, we spent a sabbatical in Trieste, where the Brunner family have been prominent citizens for generations. Judith and I would commute weekly to Roberto Frasetto's Laboratorio Grandi Masse in Venice. We then thought that the proposed construction of the three gates to protect the city from occasional flooding was imminent [photo 19]. After thirty years there are still no gates (the heroic efforts of M.I.T./Venice oceanographer Paola Malanotte and many others notwithstanding), and the floods are now much more frequent.

I designed a system for opening and closing gates with the tides to induce a one-way flow which would sweep much of the pollution out of the lagoon. Judith (who is a sculptor) generated the support of the Istituto di Restauro to try a new system of cleaning statues. It was found that millisecond laser pulses at a flux level of 10 Joules/cm^2 would remove the black encrustation from marble statuary without damage, even some that had heretofore been uncleanable. We took a statue back to Scripps to practice.

Years earlier Revelle had been asked, "what is oceanography?" His answer: "anything we do at Scripps is Oceanography." One day Director Nierenberg, in search for chronically short Scripps laboratory space, caught John Asmus zapping a medieval statue with laser pulses and demanded to know what we were up to. I replied: "we are doing oceanography."

The American Miscellaneous Society (AMSOC) was founded in jest by Gordon Lill, John Knauss and Art Maxwell to maintain "coordination with visitors from outer space," for "informing animals of their proper taxonomic positions," and for other such purposes. There are no bylaws, no dues and no officers. The Society is internationally known for the Albatross Award, a stuffed bird presented at irregular intervals to an oceanographer for some "unusual achievement." In spring 1957 at a casual breakfast in our patio, AMSOC members Roger Revelle, Maurice Ewing, Harry Hess, myself and others came down to serious business when they decided to sponsor "Mohole" [photo 20]. This was to be an attempt to collect a sample of the Earth's mantle, named in honor of Andrija Mohorovicic, a Yugoslav seismologist who discovered the discontinuity between crust and mantle. The place to do this is under the sea where the overlying crust is relatively thin, typically five kilometers. The required development for keeping the drilling vessel in place, and of re-entering the hole if necessary, seemed within reach of the existing acoustic technology.

It was difficult for a government agency to deal with a whimsical group such as AMSOC, so we were reincarnated as a committee of the National Academy of Sciences (most of AMSOC had by then been elected to Academy membership). By 1961 we were drilling off the coast of Mexico in four kilometers of water. The drill penetrated 200 meters of sediment and a meter of underlying basalt. The drilling vessel CUSS I performed admirably, being constantly "underway" driven by four large outboard propellers to maintain a fixed position relative to three sonic bottom transducers. It was the first example of "dynamic positioning." Willard Bascom was in charge, and Edward Horton designed the drill string. The famous American writer John Steinbeck and Photographer Fritz Goro were aboard and recorded the event for *Life* magazine.

We returned elated, not realizing that from this moment on the project was doomed by its the very success. The early phase had been completed on time and within the allotted budget of $1.7 million. The mantle drilling was estimated at $40 million and was to be performed by a prime contractor. Until then the major companies had paid no attention. They now decided that offshore drilling could present some opportunities. Ten proposals were received. The selection process passed out of our hands to a government group who awarded the contract to Brown and Root, a company which did not have, nor did it claim, any in-house technical off-shore capabilities. When questioned about this, company president Brown is said to have replied: "I can always hire an acre of engineers."

Brown and Root turned the proposed learning experience into a one-shot one-hole engineering spectacular. Three years later, fatter an expenditure of $50 million, the project folded without accomplishment (but ten years after the Mohole demise, the Deep-Sea Drilling Project initiated an enormously successful ocean-wide sediment drilling program).

The AMSOC group shares the blame for expecting that they could continue in their usual jobs and still play a useful role in the Mohole project. I walked away with the determination never again to participate in a project unless I was prepared to give it the time required. This turned out to be a good preparation for things to come.

Beginning in the late 1970s, Carl Wunsch of M.I.T., Robert Spindel (then of Woods Hole), Peter Worcester and I at Scripps, and many others, have engaged in a collaborative effort of monitoring large-scale ocean variability by sound. I will devote the remaining talk to this effort.

The concept started with the "mesoscale revolution." For a century oceanographers focused their attention to the major current systems, the Kuroshio off the coast of Japan, the Gulf Stream off our east coast, and of other major current systems. These were held to be *stationary*, and sea-going efforts were devoted to describe this *steady* circulation in ever-increasing detail. That point of view came to a crashing halt when improved observational methods revealed time-variable meanders in the Gulf Stream, which led to intermittent shedding of large scale intensive eddies. Soon it was learned that such "mesoscale" eddies were not only part of the large-scale current systems, but filled the global oceans. In fact, 99 percents of the kinetic energy is associated with the eddies, only a percent with the major currents themselves. How it was possible that 99 percents of ocean dynamics were ignored for over a century remains one of the mysteries to be explored by science historians.

The discovery of mesoscale dynamics introduced a new dimension, time, into the observational requirement. Physical oceanography went from a 3-d to a 4-d presentation. Chasing around the oceans with a few oceanographic vessels was no longer a good strategy.

The oceans are opaque to radio waves and other electromagnetic waves, but transparent to sound (opposite to the situation in the atmosphere). It seemed reasonable to take advantage of the good acoustic properties of the oceans. Now the speed of sound is a sensitive function of temperature; accordingly the travel time of an acoustic pulse between two points is a measure of the temperatures of the intervening waters. With a network of acoustic sources and receivers we should be able to map ocean temperature in three dimensions, and in four dimensions if the transmissions were repeated on a schedule. We called the procedure "Ocean Acoustic Tomography" because of the similarity with medical tomography, but also because the words were sufficiently obscure to arouse the curiosity of the casual reader.

For twenty years, tomographic experiments were conducted in the Atlantic, Pacific and Arctic Oceans, with reasonable success and no particular public interest. Peter Worcester and I spent a short sabbatical at Angus McEwan's institute in Tasmania [photo 21] to work on the first draft of *Ocean Acoustic Tomography*. The scales of the tomographic transmission paths had grown from 20 kilometers to 1,000 kilometers. With the increased public concern about global warming, the question arose whether the experiments could be extended to a climate scale, order of 10,000 kilometers. The

Heard Island experiment was designed to test at what scale man-made sounds could be recorded to allow estimates of global warming. An international consortium was formed, informally at first, with Japanese participation. We sailed the *Corey Chouest*, an acoustic source ship working for the U.S.Navy, to Heard Island, an uninhabited Australian island in the south Indian Ocean. The unique situation of Heard Island is that there are great circle routes to all ocean basins unimpeded by land [photo 10]. The Heard Island transmissions were in fact recorded in the North and South Atlantic, the North and South Pacific, and of course in the Indian Ocean, at distances up to 19,000 kilometers. Eleven nations (including Japan) participated on an informal basis, lowering hydrophones into the sound channel.

This was an experiment where we had no prior of what would be found. At one extreme the predicted range was a few thousand kilometers, at the other extreme the signals should be recordable everywhere. As it turned out, a five-minute-test-signal on the evening before the test was picked up three and a half hours later by Kurt Metzger in Bermuda and (almost simultaneously) by Ted Birsdall on a receiver off Washington State. The signals had gotten there by very different paths, the first traveling westward past the Cape of Good Hope into the south and north Atlantic, the other eastward past New Zealand into the south and north Pacific. The demonstration of global acoustics on the afternoon of 5 January 1991 was the high point of my career.

But there were clouds to come. I spoke earlier about our 1963 “Waves across the Pacific” expedition for tracking ocean wave disturbances (not sound waves) along a great circle route from New Zealand to Alaska. We had wondered then about the mighty storms that had signaled their presence around the world. But our wonder was impersonal, at distances of 10,000 kilometers. It never occurred to me in 1991 that we were going to the very location we had identified 28 years earlier as a source region for the great wave events. The first week at Heard Island went well enough and we collected good data. But then the storm hit; of the ten acoustic sources, one went to the bottom and nine were demolished. The Captain tried to console me by saying: “Don’t worry, we were told that the equipment was designed for any possible sea state, and we have gone to some length, even to some discomfort, to test this hypothesis.” When I am now asked; “How long have you had your instruments in the water?” I say: “Ten years, and getting longer every year.” If the storm had hit a week earlier, the experiment would have been a failure, the project would have been canceled, and (as I said yesterday at the ceremony) I would not be here today. You have to have some luck.

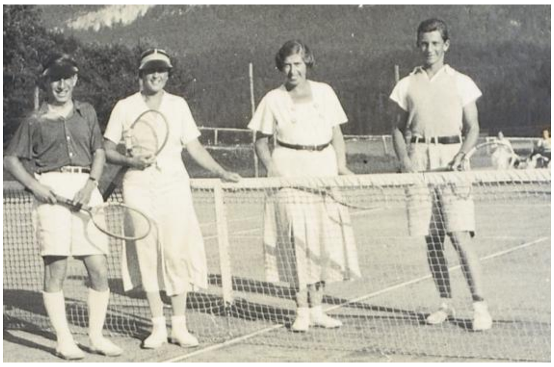
We returned elated, not realizing that from this moment on the project was in deep trouble by its the very success. We called the project called Acoustic Thermometry of Ocean Climate (ATOC). Heard Island had gotten a lot of publicity, and had aroused Greenpeace and other environmental groups to the danger of deafening whales by sound. The concern is a very real one, whales and other marine life depend crucially on sound for finding food, locating mates and for a myriad of other functions. (Whales have known about the good quality of ocean sound propagation long before ATOC.) The question is one of numbers: Is the intensity of sources at a level that could produce harm? The *Los Angeles Times* published a front-page article on an interview with a environmentalist who claimed that ATOC would kill 750,000 California Gray whales. This was based on the faulty assumption that our sources transmit 260 million watts of acoustic power. Actually we transmit at 260 watts. The discrepancy arises from the fact that the decibel unit used in air differs from that used in water. The error was subsequently corrected, but by then it was too late. We spent three years fighting a determined lobby. I was determined not to repeat the Mohole disaster. Judith and I attended eleven noisy and very unpleasant public hearings. (James Lynch has written a book on the ATOC story to be published next year.) By now there have been two years of ATOC transmissions with no indication of any significant effect on marine life. But the long delay and continued opposition to ATOC leaves the future in doubt.

We were requested to speak in a personal way of our life and our career in this commemorative lecture. The Kyoto award identifies the speakers with unusual success. The generous sponsor of this event, Dr. Inamori, wants us to share the elements of our success with a generation of young people. All I can say is: do what you enjoy, do it energetically, be daring, and grab hold of opportunities as they pass by.　Glück auf!

photo 1

photo 2

photo 3



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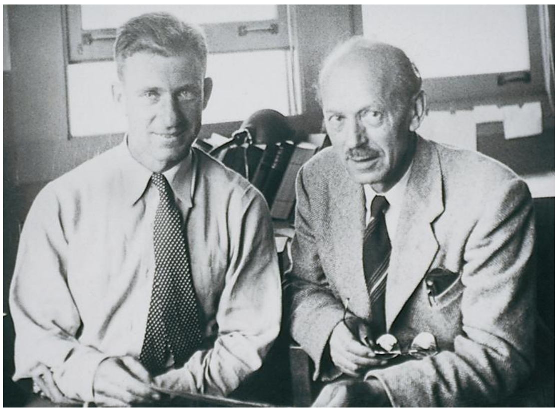
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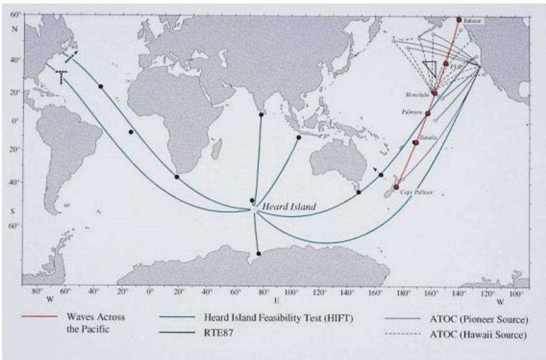

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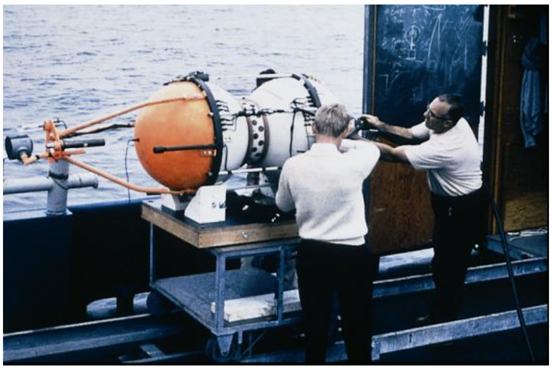
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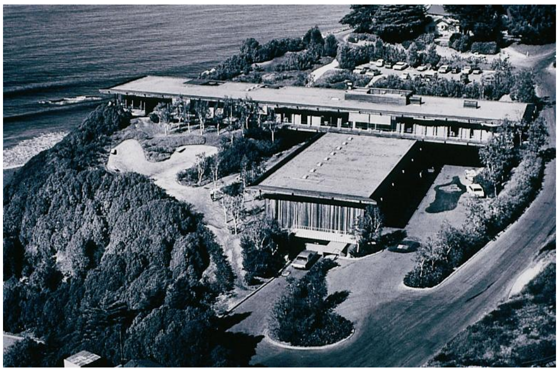
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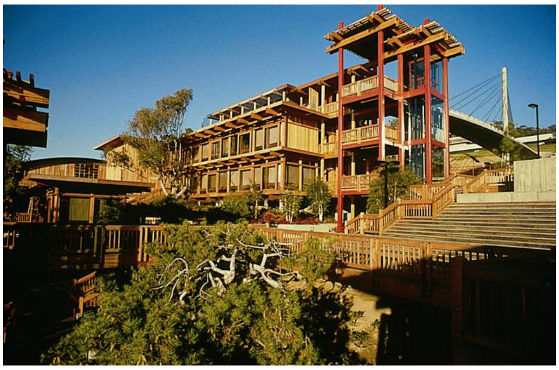
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