The following pages are taken from an introductory chapter written at the request of the Editors of the Annual Review of Earth and Planetary Sciences.* This chapter was written on a skiing vacation at San Vigilio di Marebbe; it provided a ready excuse for coming off the mountain in the early afternoon, before it got cold and icy. Only after the chapter was published did I realize that I had failed to even mention the ill-fated MOHOLE project (perhaps because I have been trying to forget it for twenty years). This omission is now remedied. There are several other modifications, and the account is brought up-to-date.**

I associate most of my boyhood prior to coming to America with life at the Eggelgut, in Altaussee, a village about 45 minutes out of Salzburg. My grandfather converted and enlarged a 17th century peasant house to the Eggelgut, located on a steep meadow between the forest and a brook. Life was centered around the lake, and around the tennis courts. I took tennis very seriously and once made it to the Austrian semi-semi finals for junior doubles. In winter we would ski on the Loser which started right behind the house. After the war Mother sold the house and most of the land, but we still visit there occasionally.

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**References to papers in which I am an author are numbered in accordance with the Bibliography in this volume.
The picture was taken on the Dachstein, a 3,000 meter high mountain not far from Altaussee. I never was any good at rock climbing; it scared me to death, so I was glad to stop when I became old enough to do so.

**Prologue**

I was an unlikely person for a career in oceanography. When I was born in Austria in 1917 the country had already lost its tenuous hold on a last piece of coastline. My maternal grandfather was a private banker in Vienna, and left enough to provide adequately for his five children, but not for his grandchildren. In 1932, at the age of fourteen I was sent to a boys’ preparatory school in upper New York state to finish high school, and to be subsequently apprenticed to a financial firm my grandfather had helped to found. I barely managed the preparatory school; it went bankrupt the following year. I then did a three-year stint at the financial firm; the firm folded the year after.

Somehow I talked my way into Cal Tech and graduated in 1939 in "Applied Physics". At the time I was in love with a Texas girl who vacationed in La Jolla, so I applied for a job at the Scripps Institution of Oceanography in the summer of 1939. The Texas romance has been outlasted for some forty years by my romance with oceanography. In 1950, when I declined an offer by C.-G. Rossby to join his Department of Meteorology at the University of Chicago, Rossby told me that "anybody with any imagination changes jobs once in a decade." But I am still here!

I loved Scripps from the time I spent the first night at the Community House, on the site now occupied by the Institute of Geophysics and Planetary Physics. Harald Sverdrup, the famed Norwegian Arctic explorer, was Director. The staff, including one secretary and a gardener, totalled about 15.

1939. When I came from Cal Tech for the summer I lived at the Community House, now the site of IGPP. Scripps was isolated from La Jolla; there was only one house in what is now called La Jolla Shores.
After the first summer, I went back to Cal Tech for a Master’s Degree in Geophysics under Beno Gutenberg. But next summer I was back at La Jolla, requesting to be admitted for study towards the PhD Degree in Oceanography. Harald Sverdrup gave my request his silent attention for an interminable minute, and then said that he could not think of a single job in oceanography that would become available in the next ten years. I promptly enrolled, and for some time I constituted the Scripps student body.

This was the time of the German occupation of Austria, and a general war seemed imminent. I enlisted in the US Army, and spent 18 months in the Field Artillery and the Ski Troops. Peacetime service became dull, and I was glad when the opportunity came to join Harald Sverdrup, Roger Revelle, and Richard Fleming in a small oceanographic group at the US Navy Radio and Sound Laboratory at Point Loma (now NOSC). A week later Japan attacked Pearl Harbor. For the next six years I worked on problems of amphibious warfare. I did not get back to the PhD dissertation until 1947, and then only under threat of dismissal. My thesis was written de novo in three weeks (see reference 6 of Bibliography) and is the shortest Scripps dissertation on record. As it turned out, its principal conclusion is wrong (15). In 1979 when UCLA (where Scripps degrees were awarded in the forties) called to offer me the Distinguished Alumnus Award, I thought for a moment they were going to cancel my degree.

During my career I have worked on rather too many topics to have done a thorough job on any one of them; most of my papers have been superseded by subsequent work. But “definitive papers” are usually written when a subject is no longer interesting. If one wishes to have a maximum impact on the rate of learning, then one needs to stick out one’s neck at an earlier time. Surely those who first pose a pertinent problem should be given some credit, and not just criticized for having failed to provide the final answer.
The underlying thread to my work consists of a theme and some habits. The theme is a kind of Earth spectroscopy: to collect long data series and then to perform spectral analysis of high resolution and reliability. We developed a system of computer programs (91) called BOMM (for Bullard, Oglebay, Munk, and Miller), which at one time was widely used. The procedure has been rewarding in the studies of ocean waves and tsunamis, tides, Earth wobble and spin, variations in gravity, and the scattering of sound and radio waves. Oceanographers have long been familiar with discrete spectra, but when I entered the field the corresponding analysis of continuous (or noisy) processes was not familiar (although it had been applied in optics and acoustics for generations). The reason, I believe, has to do with the difficulty of measuring very low frequencies with resonant analog devices. The analysis did finally become routine, but not until corresponding numerical methods had become accessible through the development of fast-speed computers.

Now to turn to the habits:

1. I do not spend much time in polishing lectures. The excuse is that, in a small class, students learn more if they participate in halting derivations and have the joy of pointing out blunders, than if they are handed the subject on a silver platter. As a student, I listened to a series of lectures by a famed Scandinavian geophysicist who had selected each word in advance, including the layout of the blackboard. I found those lectures extremely dull.

2. I become intrigued with new techniques (spectral analysis, array processing, sensitive pressure transducers, radio backscatter, acoustic sensing) before knowing what purpose they might serve. It is a case of a solution looking for a problem. Here my excuse is that if you can apply a significant technical innovation to a field of general interest, then you cannot help but learn new things. I do not propose this procedure to everyone, but for me it has worked well.

3. I do not like to read. The outcome has been that I have entered fields with little or no modern literature; in a number of cases I left the field some ten years later in a state of lively activity and an increasing rate of publications.

Walter Munk

1940. Rowboat. My first oceanographic outing.
Waves and the War, 1942-1950

In 1942, Harald Sverdrup and I were told of the Allied preparations for an amphibious winter landing on the northwest coast of Africa. The coast is subject to a heavy northwesterly swell, with breakers exceeding six feet on two out of three days during winter. Yet practice landings in the Carolinas were suspended whenever the breakers exceeded six feet because of broaching of the LCVP landing craft. The problem, simply put, was to pick the one out of three days when the waves are suitable for landing.

We started work to predict waves on the basis of weather maps. The prediction consisted of three steps. (i) The height $H$ and period $T$ of the storm-generated sea were related to the wind speed $U$, the storm fetch $F$, and duration $D$ through four dimensionless relations:

$$\frac{gH}{U^2}, \frac{gT}{U} \text{ as functions of } \frac{gF}{U^2}, \frac{gD}{U}.$$  

(Waves are either fetch-limited or duration-limited.) We scrounged together observations from oceans, lakes, and wave tanks and came out with rather pleasing scatter plots (2) extending over 5 octaves in $gF/U^2$. (ii) Subsequent attenuation beyond the storm area was estimated from wave dispersion and geometric spreading. (iii) The transformation in shallow water was computed from principles of conservation of energy flux (3). This method for predicting sea, swell, and surf was taught to classes of Navy and Air Force weather officers, and was applied widely to amphibious landings in the Pacific and Atlantic theaters of war. For the Normandy invasion, the waves were correctly predicted to be high but manageable.

The empirical relations have held with minor modifications to the present day. The principal shortcoming is that a complex wave spectrum is poorly described by just two parameters, height and period. (Pierson and Neumann subsequently extended the prediction to the entire wave spectrum.) We tried to calibrate the predicted heights and periods in terms of estimates by coxwains during landing exercises, and to compare these estimates to wave records taken at the same time and place. This led to the definitions of significant heights and periods as being appropriate to the averages of the highest third of the waves present.

These were exciting and rewarding times. In retrospect, we should not have sanctified our work by calling it a theory of wave prediction; it was empiricism, pure and simple, with a few dispersion laws thrown in. There is still no theory giving the observed wave dimensions, notwithstanding the important contributions by Phillips and Miles, though Hasselmann’s work on nonlinear wave coupling has come close to providing the right magnitudes.

At the end of this period, we applied what we had learned to geologic processes in shallow water (4), and took a first stab at calculating long-shore currents from obliquely incident waves (17). I earned some consulting money by calculating wave forces on offshore structures (10) according to $au^2 + b\cdot du/dt$, with $u$ designating a horizontal component of orbital velocity. The wave climate was established from hindcasts based on historical weather maps. One drilling rig for which I had calculated the wave forces collapsed in a storm.

Wind-driven Ocean Gyres, 1949-52

I worked next to Harald Sverdrup’s office when he discovered the relation $\beta \cdot M_p = \text{curl} \cdot F$ between the northward water transport $M_p$, the wind stress $\tau$ and the northward variation $\beta = (\partial f)/\partial y$ of the Coriolis parameter (Sverdrup 1947). Sverdrup had been analyzing observations of equatorial currents (where the geostrophic balance is singular). He did not derive the so-called "Sverdrup relation" systematically, but rather found it by playing with known equations. The result was in accord with observations. Sverdrup was worried for months about the simplicity of the result; how could it have been overlooked?

Only a year later, in 1948, Henry Stommel (1948) published his beautiful demonstration that the western boundary currents are associated with $\beta$ rather than $\tau$. Stommel’s equations reduce to the Sverdrup relation away from the boundaries, but he was unaware of Sverdrup’s work when he wrote his paper. In 1950 I combined Sverdrup’s and Stommel’s formalisms, but used a horizontal eddy viscosity $A$ rather than Stommel’s linear bottom friction (20). (In some ways this obscures the simplicity of Stommel’s vorticity balance.) In this way the zonal wind distribution, from the polar easterlies to the westerlies to the trades and the doldrums, could be readily associated with the global features of the ocean circulation; I used the term “ocean gyres” which has stuck in the literature ever since.

The paper gives a simple expression for the total transport of the western boundary current: $\beta^{-1} \int d\chi \text{ curl} \cdot F$ with the integration east to west across the oceans. This gave $36 \times 10^6$ m$^3$/s for the Gulf Stream. At the time the observational estimates gave $74 \times 10^6$ m$^3$/s. I thought the discrepancy by a factor of two might be a metric factor associated with the assumption of north-south oriented ocean boundaries, but was unable to solve this problem until I met George Carrier who did it in five minutes (22): the transport is not affected by boundary inclination. The problem is greatly simplified by Carrier’s boundary layer technique, and this made it possible to tackle the slightly non-linear problem (23).

Earth Wobble and Spin, 1950-1960

One way to study the planet Earth is to observe irregularities in its rotation. In this manner one can learn about the growth of the core, the variable distribution of glaciation, air and water mass, global winds, bulk viscosity, and so forth. In each case the information is related to certain integral quantities (moments) taken over the entire globe. This is the weakness of the method — and its strength.

Astronomers were the first to attempt to exploit, for geological purposes, the irregularities they had discovered. They did this in the naive faith that the simplicity of celestial mechanics could be carried over to messy objects like the Earth. To account for inconsistencies in the latitude measurements they spoke of the "proper motion" of observatories; and to explain a rather sudden decrease in the Earth spin around 1920, they pushed up the Himalayan complex by one foot.
My interest in this subject was first aroused by a statement in 1948 by Victor Starr that a seasonal fluctuation in the net angular momentum of the atmosphere must be accompanied by "undetectable inequalities in the rate of the Earth's rotation", the net angular momentum of the planet Earth being conserved. How big is undetectable? I started reading about clocks, and learned that M. and Mme. Stoyko of the Bureau de l'Heure in Paris had in fact discovered in 1936 that the length of day in January exceeds that in July by 2 ms. This was based on precision pendulum clocks, and subsequently confirmed with crystal clocks. A very simple calculation showed that this measured variation agreed in amplitude and phase with that inferred from the seasonal wind variation (21). Within a year we learned that the meteorologists had overestimated the strength of the westerly jets, and the inferred variation in the length of day came down by a factor of three (27). Three years later the astronomers found periodic errors in the right ascensions of the FK3 catalogue, and this led to a similar reduction in their estimates. The conclusion was the same as it had been in the first place, that a seasonal variation in the length of day is largely the result of a seasonal variation in the westerly winds (42). And so it stands today.

There was a curious dichotomy between those who measured latitude and so inferred the wobble of the Earth relative to a rotation axis fixed in space, and those who compared sidereal time to ephemeris time (later to atomic time) and thus inferred the variable spin. But wobble and spin are the three components of one vector, and for geophysical processes the observations of latitude and time had better be discussed together. For example, by considering the relative magnitude of the three components, Revelle and I put some upper bounds on the melting of the Greenland ice cap (33), and we suggested that the surprisingly large decade variation in the Earth's spin without discernible wobble could be related to variations in the angular momentum of the fluid core. After five years of scattered publications touching many diverse aspects of geophysics, I joined forces with Gordon MacDonald to prepare an encompassing geophysical discussion of the rotation of the Earth (72). This is now an active field, and I am pleased that a book by Kurt Lambeck (1980) has come out, following ours by just 20 years. One of the most interesting topics involving the Earth's rotation is the problem of tidal friction. This topic is intimately connected with the evolution of the Earth-Moon system. We estimated the tidal dissipation at $3 \times 10^{12}$ watts from the modern astronomical observations, about the same from Babylonian eclipses, and $2 \times 10^{12}$ watts from oceanographic measurements. By now (Lambeck 1980) the Babylonian observations have been reworked, additional ancient observations have been uncovered, and the global ocean tidal models have given much tighter estimates. Artificial satellites have provided independent evidence. Prehistoric data on tidal friction are now obtained by counting the number of daily striations per annulation in corals, going back to Devonian times when the year had 400 days. As I understand it, all the evidence is now consistent with a dissipation rate of $3.6 \times 10^{12}$ watts.

MOHOLE, 1957-1964

In spring of 1957 the initial NSF panel on Earth Sciences had just sat through two days of reviewing sixty-five proposals when the question came up: if cost were no object, what would be the project that could lead to the greatest advance in understanding the Earth? Harry Hess and I suggested the collection of a sample of the Earth's mantle. The place to do it is under the sea where the overlying crust is relatively thin, typically 5 km. 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.

One month later the American Miscellaneous Society was having a spontaneous breakfast in our patio. AMSOC was founded by Gordon Lill, John Knauss and Art Maxwell to maintain cooperation with Visitors from Outer Space, for Informing Animals of their Taxonomic Positions, and for other such purposes. The Society is internationally known for the Albatross Award, a stuffed bird presented at irregular intervals to an oceanographer for some "unusual achievement" (I am a proud Albatross Laureate). There are no Society bylaws, no dues, no officers. Our discussion turned to the deep-sea drilling venture which was promptly named MOHOLE, in honor of Andrija Mohorovicic, a Yugoslavian seismologist who discovered the discontinuity between crust and mantle. An AMSOC deep-drilling committee was formed on the spot, consisting of Lill, Revelle, Tracey, Ladd, Hess and me; later that month Ewing, Rubey and Maxwell were co-opted. The Committee was charged with making a $50,000 proposal to NSF for a feasibility study. The Foundation did not decline outright, but suggested that it would entertain a proposal from the same group if they attached themselves to a more substantive organization. And so the nine of us became the AMSOC Committee of the National Academy of Sciences. I. I. Rabi, who was on the Academy Council, remarked: "Thank God, we're finally talking about something besides space."

It took another year until we received a $15,000 down payment from the Foundation. Willard Bascom became Director of the NASAAMSOC staff, and he, Robert Taggart, Jack McLelland, Francois Lampietti, Ed Horton and others went to work. By 1961 they were drilling just east of Guadalupe Island in 12,000 feet of water.* The story is told by Bascom (1961).

The completion of the Guadalupe drilling ended Phase I of the MOHOLE project. The drill had penetrated 560 feet of sediment and a few feet of basalt. The drilling vessel, CUSS I of Global Marine, had performed admirably; the vessel was constantly "underway", driven by four large outboard propellers to maintain a fixed position relative to three sonic bottom transponders, the first example of "dynamic positioning". The weather was poor but things worked, a tribute to Willard Bascom. John Steinbeck and Fritz Goro were along to record the event for LIFE magazine. I remember that it was very noisy, and that we all lost our voices. But the spirit was high. When Bascom wired President Deitlev Bronk of the National

*Years later as a result of the Deep-Sea Drilling Project it was learned that the site chosen was on a tiny fragment of the Farallon plate.
Affairs of the Sea

Academy that we had reached basalt, we all thought MOHOLE was in the bag. Later I was to realize that from this moment on the project was doomed.

Phase I was completed on time, and within the allotted budget of $1.7 million. The mantle drilling, Phase II, was estimated at $40 million. The NASAMSOC Committee recommended that a prime contractor be chosen for Phase II, leaving the selection process entirely in the hands of the Foundation. It was taken for granted that the Bascom group which had performed so well under Phase I would continue to play a key role. Industry, which had been on the sidelines until the Guadalupe drilling, now showed a marked interest. On 17 July 1961 some 200 people representing 84 companies attended a briefing in the NSF Auditorium. NSF called for proposals to include information on experience with experimental research projects. William Benson, who represented NSF, stated that "any good bidder will want the AMSOC staff". There was an impression that NSF would give preference to no-fee, "all-for-science" proposals. By the time of the deadline — September 11 — ten proposals were received, including:

1. Socony Mobil, with General Motors, Texas Instruments and Standard Oil of California,
2. Global Marine Exploration, with Aerojet-General and Shell Oil.

These two proposals received the highest ratings, more than 900 out of 1,000 points, from a NSF evaluation panel. A proposal by Brown and Root, a Houston based engineering and construction firm, came in one week before the deadline and was placed fifth with 801/1000 points. The panel recommended the Socony Mobil proposal to NSF Director Waterman as being "in a class by itself". Socony spent $150,000 of their own money on preliminary technical studies and had not asked for a fee; Brown and Root asked for a fee of $1.8 million. Brown and Root did not have, nor did it claim, any in-house technical off-shore capabilities. When questioned about this, Herman Brown is said to have replied: "I can always hire an acre of engineers."

It is now history that the contract was awarded to Brown and Root. Three years later, with $50 million spent and no results, the project folded.

There has been much controversy as to what happened; Solow’s (1963) article in FORTUNE gives a good account. After the award the controversy centered on the technical competence of the people in charge (as Bascom’s group was gradually removed from the project) and on Brown and Root’s insistence on going for a 1 shot — 1 hole operation. The NASAMSOC Committee, under the able chairmanship of Hedberg, favored a gradual approach with sedimentary drilling evolving into deeper and deeper holes. Academy President Seitz chose not to publicly support the AMSOC position, and the decision making shifted to the NSF staff. The argument for one, two, or N holes was resolved when MOHOLE became NOHOLE.

I walked away with the determination never again to participate in a project unless I was willing to give it the time required. It was foolish to expect that I could continue my job at Scripps as usual and still play a useful role in the MOHOLE project. MOHOLE might have worked if we had not abdicated our responsibility. In 1962, I wrote to Hedberg: "Scherer, Sheppard, and Benson (of NSF) have now replaced the AMSOC Committee in its important functions; the only legitimate question is whether they are more qualified or less qualified than AMSOC to make the essential decisions. Were the chances for success enhanced when the project was removed from the authority of an independent group to an in-house NSF activity? And were the young men who gathered under Bascom’s daring and devoted (though sometimes willful) leadership less qualified than an amorphous engineering firm which had demonstrated no previous scientific interest in the MOHOLE venture, or as far as I know in any scientific problem? I have never been able to learn on what grounds the direct approach of a few highly skilled people, which served so successfully in Phase I, had to be replaced by the ‘acres-of-engineers’ philosophy.”

Ten years after the MOHOLE demise, the Deep-Sea Drilling Project initiated an enormously successful ocean-wide sediment drilling program with the GLOMAR CHALLENGER (operated by Global Marine) for a drilling vessel. The problem of re-entry was solved. Going to the mantle does not seem as urgent now as during the MOHOLE days.

As this is being written, the question regarding the degree of NSF in-house involvement in deep-sea drilling has arisen once again. Vannevar Bush was worried about this very
problem when NSF was founded in 1950. I think the solution, then and now, is for NSF to resist the temptation of managing any of the NSF-supported research programs.

Sun Glitter and Radar Clutter

In 1953, Charles Cox and I spent the better part of a year measuring the statistics of surface slopes from photographs of sun glitter. The principle is simple: if the sea were glassy calm, the reflected sun would appear only at the horizontal specular point. In fact, there is a myriad of sun images wherever a surface facet is appropriately tilted to reflect the sun into the camera. The horizontal specular point at the center of the glitter is brightest because the probability of zero slope is highest. The outlying glitter is from the steepest facets that are the least probable. On a windy day when the slopes are relatively steep, the glitter area is large. The slope statistics are readily derived from the distribution of intensity (film density) about the specular point (44).

We managed to get an Air Force B17 (Flying Fortress) and started to work off Monterey. There was plenty of wind, but no sun. We then transferred to Maui, Hawaii, where we had plenty of sun but no wind. Finally, on the last available flying day, we recorded over the Aleinuiha Channel with winds up to 30 knots.

It was found that up/down wind components in mean-square slope exceeded the crosswind components by only a factor of two, and that both components increased roughly linearly with wind speed. These results have been found useful in a variety of different problems. Twenty-five years later Blyth Hughes et al. (1977) repeated the measurements by a quite different method, but with substantially the same numerical results.

What was missing in the glitter experiment is a measure of the relative contribution of the mean-square slope from waves of different lengths (e.g. the slope spectrum). The strength of the glitter experiment is that it provides a useful statistic directly, in contrast to the usual procedure for taking long data series and subsequently performing statistical analyses. I was on the lookout then for an equivalent method that could give spectral distributions directly. At just about this time, Crombie (1955) conducted such an experiment in New Zealand at the suggestion of Norman Barber. Crombie backscattered radio waves from the sea surface. For a resonant interaction both wavenumbers and frequencies must add up:

\[ k_1 + k_2 = k_3, \quad \omega_1 + \omega_2 = \omega_3. \]

Subscripts 1 and 3 refer to the outgoing and backscattered radio waves, and 2 to the resonant ocean wave. For a radio backscatter, \( k_1 = -k_2 \) and so \( k_2 = 2k_3 \). The associated ocean wave frequency is \( \omega_2 = \sqrt{gk_2} \). But for resonance \( \omega_2 = \omega_3 - \omega_1 \) is the radio Doppler shift. Thus for a given radio wavelength \( 2\pi/k_3 \), the radio Doppler shift is predicted at \( \sqrt{g} 2k_3 \), and this was beautifully confirmed by Doppler measurements.

With modern equipment the Doppler line is 50 dB above background, and there is not much that one cannot do with 50 dB signal-to-noise ratio. Robert Stewart found a slight departure of the measured Doppler from the predicted Doppler, and demonstrated that this slight departure was due to surface current. For a radio wavelength, \( \lambda \), and resonant ocean wavelength, \( \frac{\lambda}{2} \), the current is measured to an effective depth, \( \frac{\sqrt{\lambda}}{2\pi} \). By conducting the measurements at a series of radio wavelengths, Teague, Tyler and Stewart (1977) could estimate the current shear. By placing the radio receiver on a moving jeep one can form a synthetic aperture and measure the directional distribution of ocean waves (139). Very roughly this falls off with angle \( \theta \) measured relative to the wind like \( \cos^2(\frac{\theta}{2}) \) at relatively high winds and high frequencies. We had guessed at something of this sort in our original wave-prediction scheme (2).

W. Nierenberg and I got a joint Stanford-Scripps effort underway, and participated in some of the analyses. We showed (116) that the radar backscatter cross section \( \sigma = k^4F(k) \) is a direct measure of the saturation constant \( B \) for the Phillips surface wave spectrum \( F(k) = Bk^{-4} \). This connects two independently measured empirical constants: the typical \(-23 \text{ dB} \) backscatter cross section of the sea surface, and the saturation constant 0.005. I believe that the opportunities for gathering ocean wave statistics by radio backscatter have by no means been exhausted.

Southern Swell, 1956-1966

To the delight of the surfing community in California and Hawaii, occasional trains of tremendously long and regular swell roll into shore in the summer months. As part of the University wartime wave activity, John Isaacs had arranged for aerial photographs to be taken over California beaches. One of these pictures provided a textbook example of regular undulations being transformed over the shelf. Working backwards and allowing for wave refraction in shallow water, I estimated a deep-water direction of SSW, and a deep-water length of 2000 feet! The inference was that we were seeing the effect of storms in the southern hemisphere winter, some 5000 nautical miles away. There had been evidence in the Atlantic of ocean swell coming from very far away, particularly as a result of the work of Norman Barber and Fritz Ursell in the U.K. Could ocean swell provide useful information about distant storms (29)?

Frank Snodgrass came to La Jolla in 1953 in what was to be a wonderful partnership for the two of us for over 23 years. He adapted a "Vibrotron" transducer to measuring pressure fluctuations on the shallow seafloor, the purpose being to explore oscillations with frequencies even lower than those of the swell. We anchored off the Mexican island of Guadalupe for a week (57). Frequency analysis of the records (using an IBM 650 at Convair) showed a series of "events" starting with 1-mm waves of 50-mHz frequency, and ending a few days later with 10-cm amplitudes at 80 mHz. We computed the source distance and time as follows: the group velocity for waves of frequency \( f \) is \( g/(4\pi f) \). This means that a wave disturbance travels at group velocity \( v \) over a distance \( x \) in a time \( t - t_0 \):

\[ v = \frac{x}{t-t_0} = \frac{g}{4\pi f}, \quad \text{or} \quad f = \frac{g}{4\pi} \frac{t-t_0}{x}. \]

The slope of the line \( f(t) \) gives the source distance \( x \), and the zero intercept gives the source time \( t_0 \). And here it was,
a distant southern swell (even though we had anchored on the eastern side of Guadalupe Island). But there was one problem: one of the events gave a source distance of half the Earth’s circumference, and even though the Pacific is big, it is not that big. Could it be that the waves had originated in the Indian Ocean and had traveled half way around the world along a great circle route, entering the Pacific between Antarctica and New Zealand?

I had by then become fascinated with array theory developed in radio astronomy, and was anxious to try it out in the oceans. Snodgrass installed a triangular array off-shore from San Clemente Island (85). The results were spectacular. The very far wave sources all were within a beam from 210° and 220° true, which is the angle subtended by the window between Antarctica and New Zealand, as seen from San Clemente. (I am not suggesting this as a method for remote sensing of Southern Hemisphere geography.)

This led to the final and most ambitious of these undertakings (96). We established six wave stations along a great circle route from New Zealand to Alaska to track wave packets originating in the great southern storm belt. Frank Petersen took the station at Cape Palliser, New Zealand. I monitored from Samoa, where my daughters Edie and Kendall (ages 4 and 2), my wife, and I lived in a palm fale in the village of Vailoa Tai at the southwestern coast of Tutuila. Gordon Groves and a radio operator went to the uninhabited equatorial island of Palmyra. Klaus Hasselmann recorded at Hawaii. The floating instrument laboratory FLIP was stationed north of Hawaii, to make up for the lack of coral islands in the cold northern Pacific. Our one graduate student at the time, Gaylord Miller, volunteered for Yakutat, Alaska. Snodgrass established the stations, using seafloor pressure recorders connected by cable to shore-based digital paper punches. We recorded for three months with 98% data return. Gordon Groves got into a battle with his radio operator, whom we had to take off the island by plane. FLIP ran out of cigarettes and I had some problem keeping them on station for another two weeks (via amateur radio).

The stations were spaced with the preconceived view that the principal loss in wave energy would be by wave-wave interaction as the southern swell crossed the tradewind sea. As it turned out, the energy loss was virtually complete.
within one diameter of the southern storms. Beyond that, for the next 10,000 km, the loss was less than 2 dB and not measurable beyond the effects of geometric spreading and dispersion.*

As far as I know there has been no further work along these lines for about 20 years. With the advent of a radio altimeter on SEASAT it has become possible to measure wave height from the sharpness of the leading edge of the reflected signal. The maps produced by Nellie Mognard et al. (1982) beautifully show the penetration northward into the Pacific Ocean of wave disturbances in the southern oceans.

*The National Science Foundation funded an educational film, "Waves Across the Pacific," which was widely distributed. I am pleased with the film but not the script; for once I would like to see a film about oceanography which shows it as it is. Things do not turn out as planned; improvisation is a way of life. The final lesson is hardly ever a response to the pre-expedition question.

Surfbeats, Edge Waves, and Tsunamis, 1958-1965

Between the ocean swell and the tides there were 10 octaves of unexplored frequency space, only occasionally excited by storm "tides" and by earthquake tsunamis. We found sea level oscillations of 1 to 2 minute periods at the foot of Scripps pier; these were clearly related to groups of incoming swell (19), e.g. the frequency $f$ of this "surfbeat" equals the bandwidth $\Delta f$ of the incoming waves. This experience gave Klaus Hasselmann, Gordon MacDonald and me a first opportunity to practice a generalization of power-spectral analysis to nonlinear processes (83), following an important suggestion by John Tukey.

Frank Snodgrass was anxious to apply his experience in measuring bottom pressure fluctuations of low amplitude and low frequency to this part of the ocean wave spectrum. He installed a longshore array of transducers to determine the dispersion relation $\omega(k)$ in the frequency range 0.5 to 60
cycles per hour (cph). The empirical $\omega(k)$ was then compared to a theoretical $\omega(k)$ for gravitationally-trapped edge waves. The agreement is so good (89) that I suspect readers have simply assumed that the plotted curves were fitted to the empirical points, rather than having been derived independently. (This might explain why the paper has not been noticed.) It is my only experience of an oceanographic experiment that gave unequivocal confirmation to a previously derived theory.

This work gave us the impetus to explore the low-frequency wave background on the California continental borderland, away from the coastal edge. The result was a dull, featureless, and quite reproducible spectrum (78) which forms the background to the tsunami studies subsequently conducted by Gaylord Miller (79). The source of this background is not known.

We pushed the measurements to lower and lower frequencies, down to the tide and eventually through and beyond the tidal line spectrum (86).

December 1968. The Faculty gave a reception for the new UCSD Chancellor William McGill at the Scripps family residence Miramar. I was then Chairman of the Faculty. The inauguration was followed by two turbulent years of student unrest.

1969. In our patio. From left: Edie, "Heavy" Palchek (the Student Body President), W.M., Judith, Sam and Kendall.
Tides, 1965-1975

The incentive to go seriously into tides came from a number of directions. The study of Earth rotation had provided the initial fascination with the global dimensions of bodily and fluid tides. Further, the ultimate limit to the prediction of the tidal line spectrum is set by the low-frequency continuum, and this limit had been ignored by the tidal community, who had been spoiled by a favorable signal-to-noise ratio. David Cartwright and I made a caustic remark (97) that "noise-free processes do not occur except in the literature on tidal phenomena."

It always pays to know the ultimate limits set by instruments or by nature. Some of the weaker tidal lines routinely included in the harmonic method turned out to be hopelessly contaminated by the noise continuum and might as well be omitted. From these considerations, Cartwright and I (97) proposed a "Response Method of Tidal Prediction", which consists of using station records to compute the transfer function between the tide-producing forces and the station response. This differs somewhat from the classical harmonic method, which independently evaluates the amplitudes and phases of the principal tidal constituents. In some tests conducted by Zetler et al. (1979) the response method comes out slightly ahead of the harmonic method, but here we have improved one of the few geophysical predictions that already works well.

The third and predominant consideration for working on tides came from an instrumental development. Frank Snodgrass had found that a newly developed quartz crystal pressure transducer was superior to the Vibrotron pressure transducer, our mainstay for some years. Starting in 1965, the quartz transducers were incorporated into capsules freely dropped to the seafloor and subsequently recalled by acoustic command from a surface vessel. (The free-fall technique became commonplace in the early 1970s.) Working with Jim Irish and Mark Wimbush we first did some deep-sea drops off California and located the $M_2$ amphidromes (the point where the tidal component has zero amplitude) in the Northeast Pacific (119, 126). This was followed by three drops between Australia and Antarctica, spanning the latitudes where the sublunar point travels around the southern oceans at the speed $\sqrt{gh}$ of free waves (Irish and Snodgrass 1972). A very naive theory predicts a resonant amplification at such latitudes. We didn't believe the theory, but made the measurements anyhow. The result was a rather dull transition from south Australian to Antarctic tides.

We had organized an international SCOR working group on deep-sea tides, and numerous measurements were being made, particularly by Cartwright in the U.K. and by Mofjeld of NOAA, Miami. Snodgrass participated in an international calibration experiment in the Bay of Biscay. The latest IAPSO publication shows 108 pelagic tide stations have by now been occupied by a number of different investigators. The results have been useful as a check on the numerical modeling of tides.

Our last drops were made in 1974 south of Bermuda in 5.5 km of water, as part of the MODE bottom experiment. We discovered unexpected and still unexplained pressure fluctuations at subtidal frequencies that are coherent over 1000 km (143)! With regard to tides, an analysis led by B. Zetler was in splendid agreement with the traditional Atlantic cotidal charts (145). Two independent drops in the same area gave the following $M_2$ amplitudes and phases:
- 32.067 cm and 2.5° Greenwich epoch,
- 32.074 cm and 2.6° Greenwich epoch.

When it comes to four-figure accuracies, it is no longer oceanography. Further, satellite altimetry looked increasingly promising for future measurements of deep-sea tides. It was time to move on.

Internal Waves, 1971-1978

In 1958, Owen Phillips proposed from simple dimensional considerations that the distribution of surface elevation variance with wavenumber $k$ varies as $k^{-4}$ ($L^2$ per unit $k_x$ per unit $k_y$). This "saturation spectrum" has turned out to be a most useful representation of high-frequency surface waves. Could something as simple and as useful be done about internal waves?

Christopher Garrett and I looked at existing evidence and found it consistent with a spectrum that falls off with horizontal wavenumber as $k^{-2}$ and with vertical wavenumber as $m^{-2}$. The original model proposed in 1972 (130) has gone through a series of revisions, which have been referred to as GM75, GM79... to make explicit the built-in obsolescence. The surprising thing has been the degree of universality of the model spectrum. This indicates a saturation as in the case of the Phillips surface wave spectrum. But whereas the Phillips spectrum is white in curvature (and vertical acceleration), the internal wave spectrum is white in shear and isopycnal slope, suggesting a different saturation process. The entire ocean column is never very far from instability, and occasional internal breakers may play an important role in turbulence and fine scale mixing. The essential work remains to be done (163).

Ocean Acoustics, 1975-

Regardless of the role played by internal waves in ocean microprocesses, there can be no doubt that internal waves are a dominant source of fluctuation in sound speed. Clark and others have recorded time series of acoustic phase and intensity over a 1000 km transmission path between Eleuthera and Bermuda. From these observations one can infer the mean-square phase rate along any one of the multiple paths that connect source and receiver. The result is $\langle \phi^2 \rangle = 1.6 \times 10^{-3}$ sec$^{-2}$. Fred Zachariasen and I have derived the theory for computing this parameter, given only the mean sound speed structure and an internal wave spectrum (148). For GM75, the result is $\langle \phi^2 \rangle = 2.5 \times 10^{-5}$ sec$^{-2}$. This was the beginning of a major effort led by Roger Dashen and Stan Flatte to derive sound transmission statistics, given the spectrum of the variability in sound speed in ocean space and time (158). Since World War II the acoustic and oceanographic communities have gone their separate ways; I think that we have made a contribution towards bridging this gap.

In 1976, Peter Worcester and Frank Snodgrass set two deep moorings, with an acoustic source and receiver on each mooring. Oppositely-directed acoustic transmissions gave
information about the 25 km of intervening ocean. Variations in the average of the two travel times told something of the fluctuations in the temperature structure; differences in the travel times (with and against the current component) gave information about the water movements. This is a powerful technique for measuring ocean features on a scale of tens of kilometers, and it whetted our appetite for acoustic monitoring of the intense mesoscale features, with typical dimensions of 100 km.

At a range of 1000 km an ideal acoustic pulse is received as a complex series of subpulses, one along each of a series of multipaths. Our work predicts the effective spread of the subpulses, and hence the time resolution between separate paths. It also predicts the decorrelation time of the pulse structure, and hence the interval at which independent samples are taken. Typical values are 50 ms and 5 minutes. On this basis, Carl Wunsch and I have estimated that we can measure week-to-week fluctuations in acoustic travel times along a fixed path to an accuracy of 20 ms. But the expected variations from mesoscale ocean eddies are many times this large. Accordingly, we have proposed to measure the variable travel times between a series of moored acoustic sources each transmitting to a series of acoustic receivers, and then to construct three-dimensional charts of sound speed (essentially temperature) by an appropriate inverse theory (157).

The idea is very simple; in the case of a warm eddy (say) all
those rays that pass through the eddy will come in early by something like a quarter second, whereas the other transmissions are not affected. If the eddy is shallow, then only the early steep paths are affected; if it is deep, then the late flat paths (near the sound axis) are affected as well.

We have formed a joint venture involving Robert Spindel of Woods Hole, Carl Wunsch of MIT, Theodore Birdsall of Michigan, and our group at Scripps. In November, 1977, Spindel put out a 2000-m deep mooring south of Bermuda, which our graduate student, John Spiesberger, monitored at a coastal station 1000 km distant. There are about a dozen distinct arrivals, and those can be clearly traced over the two-month transmission period. The key questions were: (i) whether the multipath arrivals could be resolved, (ii) whether they could be identified with constructed rays connecting source and receiver, and (iii) whether such paths are sufficiently stable for inverse monitoring. Spiesberger found the answer "yes" to each of the questions.

In 1981, we established a 300 km x 300 km array southwest of Bermuda (the Bermuda square) consisting of four sources and five receivers. There are then 20 source-receiver pairs providing information along each of 20 sections. One of the beauties of tomography is that you gain information geometrically with the product of the number of source-times-receiver moorings, rather than with the sum of moorings as in traditional oceanographic spot measurements. (In the 1981 experiment we learned painfully that the array can also deteriorate geometrically, as was the case because of an early battery failure.) The experiment produced a great deal of data, and the preliminary inversions demonstrated a westward migration of an eddy through the array in agreement with CTD surveys conducted by David Behringer (171). As this is being written, Carl Wunsch and his student, Bruce Cornuelle, are continuing their work on inverting the acoustic data with continued improvements. The definitive reductions will take yet another year.

Our long term goal is to measure the variability of ocean gyres by using acoustic transmission over one or two megameters. Henry Stommel and Larry Armi have found some evidence for a gyre wobble. The unique advantage of the acoustic method is that it yields immediately the required long-range averages, instead of depending on the sums of local measurements. We ought to be able to measure changes in the mean temperature by a few millidegrees per year, and this might now be taking place (though I doubt it) because of the injection of CO₂ into the atmosphere. "Reciprocal tomography" should yield mean currents to a precision of about
1 mm/s, and this offers the opportunity to use a "sing-around" in a triangular array to check the vorticity balance of the gyre circulation, an old dream that goes back to the 1950's. A crucial question has to do with the vertical resolution of the inverse method.

During our 1982 sabbatical in Cambridge, England, Carl Wunsch and I gave our imagination free rein in what might ultimately be done in a combined system of satellite altimetry and scatterometry (measuring surface stress), acoustic tomography and numerical modeling (172).

So much for the main topics that have kept me busy. Nearly all the work has been done in collaboration with others; the bibliography at the end of the chapter is a way to make explicit my indebtedness to so many people. My principal collaborators have been Roger Revelle, Charles Cox, George Carrier, Klaus Hasselmann, Gordon MacDonald, David Cartwright, Bernard Zeiter, Chris Garrett, Fred Zachariasen and Carl Wunsch. My partnership with Frank Snodgrass lasted through 23 happy and constructive years. He retired in 1976 to become a farmer in Oregon, and I have never quite recovered from this loss.

I have two or three graduate students at a time (the most I can manage), and work closely with them. Among them are Gordon Groves, Earl Gossard, Charles Cox, June Pattullo, Mohammed Hassan, Gaylord Miller, Mark Wimbush, Jim Irish, Jim Cairns, Gordon Williams, Peter Worcester, John Spiesberger, Mike Brown, and now Bruce Howe. I have learned more from them than they have learned from me.

Among my teachers are three men in particular: Harald Sverdrup (a Norwegian) taught me how to write English, and how to treat each observation with great care and respect (so much of this is lost in computer analyses and plots). Roger Revelle introduced me to the romance of work at sea, and showed me his style of broad-range inquiry. Carl Eckart taught me some classical physics. I have always regretted that

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1952. Willard Bascom took this picture in Bikini Lagoon when I was about to be devoured by a shark! I was standing on the bottom with a tsunami pressure gauge which I had raised above my head and was holding there to obtain a Laplace transform of instrument response, when I noticed Bascom taking a most unusual photographic interest in this operation. I puzzled about this for some minutes, and then had a sudden revelation that made me turn around. The calibration was an incomplete step function.
I did not learn more physics before becoming absorbed in oceanography. I also regret that I am so poor at building and repairing gear (I was sheltered from this as a boy).

In 1958, we started a branch of the University-wide Institute of Geophysics (later Geophysics and Planetary Physics) on the Scripps campus. Roger Revelle and Louis Slichter made this possible. My wife, Judith, chose the laboratory site and the multi-level, one-story redwood construction. At the time, I was working on solid-earth geophysics, and this is reflected by the early appointments. I became rather lonely when my interest returned to the sea. We are now fairly evenly divided, and for many years we split the job with Freeman Gilbert looking after solid-earth geophysics. In 1982, I stepped down as Associate Director after being in the job for 24 years. Freeman Gilbert was willing to take over, and the Institute is in very good shape. The birth and coming-of-age of IGPP has been one of my most rewarding experiences.

This biographical sketch would be unbalanced without some comments on an association with the United States Navy which spans my entire career (except for an interlude in World War II when my security clearance was suddenly withdrawn). The Office of Naval Research has given our work generous and effective support ever since ONR was formed, not only with money but in other ways as well. I owe a deep gratitude to this remarkable organization. At the same time I have been able to serve the Navy in different ways. In 1946, Bill Von Arx and I surveyed the circulation of Bikini lagoon and assessed its flushing rate prior to an underwater nuclear explosion. In 1951, working with Roger Revelle, John Isaacs, Willard Bascom, and Norman Holter, we monitored at close range the oceanographic effects of a very large thermonuclear explosion. And in recent years, largely through my association with JASON, I have been involved in a diverse set of Navy problems.

1952. Looking at a giant coral during OPERATION CAPRICORN. This is my favorite picture.
The 1951 nuclear test IVY-MIKE almost brought my scientific career to an end. Revelle, Isaacs, and I had expressed to high AEC authority our fear that the thermonuclear shock to which Eniwetok Atoll was to be subjected might trigger a submarine landslide. This, in turn, could generate a tsunami of destructive dimensions over much of the Pacific. Accordingly, quiet plans were made for a possible evacuation of many low-lying areas all over the Pacific. Scripps moored two small rafts to a nearby seamount 36 miles from ground zero, with wave instruments attached to each mooring. I was aboard one of the rafts. The Scripps vessel *Horizon* stood within sight of both rafts. Observers on the rafts were to signal any suspicious event to the *Horizon*, which maintained open contact to the flagship *Mt. McKinley*, so that signals could flow instantly to Navy personnel standing by at the evacuation sites.

I should stress that the probability for a destructive wave was very, very small, and in fact nothing happened. After witnessing the explosion at this close range, and seeing no wave signal for 11 minutes thereafter (the computed time was 6 minutes following the landslide), we transferred to the *Horizon* and steamed north at full speed to avoid radioactive fallout (unsuccessfully as it turned out). We returned in two days to pick up the rafts and instrumentation. I unspooled the record, checking the time marks made prior to my leaving the raft. Within 90 seconds following the final time mark was a record signature equivalent to a huge tidal wave. (I do not know what went wrong.) It is true the "event" occurred too late to be consistent with computations, but I rather think

*Note:* There are very few earthquakes in this area of the Pacific.
that under the existing stress (and having in mind the possibility of a delayed landslide), I would have given the signal, and thus set into motion the evacuation of thousands of people from hundreds of sites. Under the circumstances, I would have been too embarrassed to return to Scripps and would have left the ship at the next landfall, Tongatapu.

I came home and married Judith Horton (my second marriage). Judy has been a willing confidante in all subsequent affairs of the sea, often an instigator. She took the $04^h$ to $08^h$ watch in recording southern swell rolling onto the beach at our $fale$ in Tutuila; she navigated our Landrover through a sea of Ukranian mud on our way to the oceanographic station at Gelendsik; and she helped celebrate Austrian Independence Day at Schönbrunn. Judy was responsible for taking the plunge of starting the Geophysics Institute at La Jolla, and then saw to it that it was elegantly housed on the Scripps cliff.

Judy's great grandfather came to San Diego to practice medicine. As a young girl she was a disciple of San Diego sculptor Donal Hord, and she does life-sized figures in terra cotta. She studied architectural design with Richard Nentra at Bennington, and was admitted to Harvard Graduate School of Design. I walk to work from SEICHE, which has been a continuing project for both of us for thirty years (I am the resident plumber and electrician).

We have two daughters, Edie and Kendall; our oldest daughter Lucian was born with a defective heart and died at the age of five. Neither of our daughters are scientists (though my younger daughter married a chemist), nor did I make any effort to steer them in that direction. The girls have minds of their own; but I take pride that they are also good skiers.
1962. Judith and I on our first trip to the USSR. We had shipped our Land Rover to Helsinki, and entered the Soviet Union at the Finnish border. A month later we left Russia at Odessa. The Cuban Missile Crisis occurred while we were driving south.

We have taken advantage of an oceanographer's opportunity to travel. Judy and I have been to Russia three times, the first time during the Cuban missile crisis. We took part in establishing a bond between American and Soviet oceanographers following World War II, and I regret that this bond has now weakened. I have been to the Peoples Republic of China twice in the last five years, the second time as chairman of the first U.S. oceanographic delegation (159a). I was asked "Was it your father who worked on ocean waves?"; or "Did you have a relative with a name similar to yours who worked on tides?", etc. When I replied that they must be referring to my own early work, the response was: "But you are working in acoustics!". Evidently a Chinese oceanographer whose thesis is on tsunamis still works on tsunamis when he retires. The succession of promiscuous affairs that have characterized my career (and which I chose for a title of this biographical sketch) is alien to the Chinese culture.

April 1968. Visiting Soviet Armenia. Oceanographer R.V. Ozmidov is to the right.
We have lived in Venice, where I worked on the *aqua alta* problem. But Judy soon turned our attention to a project with John Asmus for cleaning statues with laser pulses, and for preparing holograms for three-dimensional viewing of Renaissance figures. (We wrote a joint sculpture-oceanography paper (13)). We go to Venice, to Südtirol and to Austria whenever there is an opportunity; we still have my grandfather’s land on a lake near Salzburg.

We love Cambridge, England, and have been there for three sabbaticals, two in residence at Churchill College. (My mother read Botany at Newnham College in 1912; her tutor was Harold Jeffreys.) We spent six months at the other Cambridge in 1967. Judy had been awarded a coveted Radcliffe Institute Fellowship which gives women a chance to resume their careers after a family interruption. (Frank Press arranged a MIT visiting professorship for me.) Judy’s plan
was to work at the Harvard School of Design, where she had been a student when she came down with polio at the age of twenty-one (she has had to walk on crutches ever since). We went to Robinson Hall where Judy had been assigned a drafting table. We were taken to the basement and then to a desk; it turned out to be the very same desk that had been Judy's when she was a student there twenty years earlier. "It looks just the same," Judy said, "and as far as I can tell the place hasn't been swept since."

Roger Revelle's theme in talking to students is that "you have to be lucky." He is so right. And my luck crested in October 1982 when so many of our friends with whom I had worked and lived came to La Jolla and helped celebrate my birthday.

Literature Cited


BIOGRAPHY

Walter H. Munk

Date of birth: October 19, 1917 (Vienna, Austria)

Education: B.S., California Institute of Technology (1939)
M.S., California Institute of Technology (1940)
Ph.D., University of California (1947)

Memberships, Honors and Awards:

Member, National Academy of Sciences (Chairman, Geophysics Section, 1975-78)
Member, American Philosophical Society
Foreign Member, The Royal Society, London
Member, Deutsche Akademie der Naturforscher Leopoldina
Member, American Academy of Arts and Sciences
Member, American Geological Society
Fellow, American Geophysical Union
Fellow, American Association for the Advancement of Science
Fellow, American Meteorological Society
Fellow, Acoustical Society of America
Guggenheim Fellow, Oslo University (1948); Cambridge (1955 and 1962)
Overseas Fellow, Churchill College (1962 and 1981-82)
Fulbright Fellow, U.K. (1981-82)
Senior Queen's Fellow, Australia (1978)
Arthur L. Day Medal, American Geological Society (1965)
Sverdrup Gold Medal, American Meteorological Society (1966)
Alumni Distinguished Service Award, California Institute of Technology (1966)
California Scientist of the Year, California Museum of Sciences and Industry (1969)
Josiah Willard Gibbs Lecturer, American Mathematical Society (1970)
Doctor Philosophiae Honoris Causa, University of Bergen, Norway (1975)
Maurice Ewing Medal, American Geophysical Union and U.S. Navy (1976)
Agassiz Medal, National Academy of Sciences (1976)
Professional Achievement Award, UCLA Alumni Association (1977)
UCSD Alumnus of the Year (1978)
The Captain Robert Dexter Conrad Award, Department of the Navy (1978)

Positions:

Assistant Professor of Geophysics (1947-49), Associate Professor of Geophysics (1949-54), Professor of Geophysics (1954-present), Scripps Institution of Oceanography, University of California, San Diego
Associate Director, Institute of Geophysics and Planetary Physics, University of California, San Diego (1959-1982)
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Walter H. Munk*

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1946


1947


1948


1949


1950


1951


1952


*The papers are organized according to the year of publication; numbers are generally in order of acceptance for publication, and therefore not sequential. An exception is #169 which was written in 1952 but remained classified until 1981. Paper 115 (written in 1969) is still classified. Some informal contributions are numbered according to the preceding publication followed by a, b, ... (e.g. 129a, 129b in 1971).
Walter Munk


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5f High-frequency spectrum of ocean waves. *Encyclopedia Britannica*.


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1977

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finding his Cambridge laboratory in shambles: "his first task during his career, full of glimpses such as returning from the War and reporting that he spilled the beans. When the manuscript was returned to me upon protesting, I was told that Sir Edward would be gravely displeased by the editors with the above quote and many others deleted. Upon protest, I was asked whether he would enjoy writing a preface for the presentation of the Maurice Ewing Medal to Sir Edward Bullard. I had just completed the dedication to Sir Edward when I was asked to write the dedication to Sir Edward Bullard - dedication.* In: AIP Conf. Proc. No. 46, Topics in Nonlinear Dynamics; A Tribute to Sir Edward Bullard, ed. S. Jorna, pp. v-viii., New York: Am. Inst. of Physics.


15e Professor Walter Munk at graduate commencement 1978.

1979


1980


1981


1982


1983


This picture of Arthur and me (on the fertility chair) was taken in our patio by Fritz Goro just after the MOHOLE trials at Guadalupe Island (1961).
A Celebration in Geophysics and Oceanography - 1982

In Honor of Walter Munk