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AFFAIRS OF THE SEA

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PROLOGUE

I have been asked by the Editors to describe my experience as a student, teacher, and investigator, and to relate this experience to current trends in our field.

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 managed to get 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 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 20. After the first summer, I went back to Cal Tech for a Master's Degree in Geophysics under Beno Gutenberg. But next summer

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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 any one 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 NUSC). 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 and is the shortest Scripps dissertation on record. As it turned out, its principal conclusion is wrong (1,2). Last year 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 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 to oceanographers (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 change did finally occur, but not until corresponding numerical methods had become accessible through the development of fast-speed computers.

Now to return 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 the lectures uninspiring.
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 other people's papers. The outcome has been that I have entered fields with little or no modern literature, to have left them some ten years later in a state of lively participation and an increasing flux of publications.

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 do the landing during the one-third time when the waves are lowest.

We started work to predict waves on the basis of weather maps. The prediction consisted of three steps. 1. 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:

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

(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 (3) extending over 6 octaves in gF/U^2. 2. Subsequent attenuation beyond the storm area was estimated from wave dispersion and geometric spreading. 3. The transformation in shallow water was computed from principles of conservation of energy flux (4). 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 Neuman subsequently extended the prediction to the entire wave spectrum.) We tried to calibrate the predicted heights and periods in terms of estimates by coxswains 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 (5), and took a first stab at calculating longshore currents from obliquely incident waves (6). I earned some consulting money by calculating wave forces on offshore structures (7) according to $au^2 + b \cdot \mathrm{d}u/\mathrm{d}t$, 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.

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 raised 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 (8). 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 (9). 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 (10). 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 ephemeral 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 (11), 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 (12). This is now an active field, and I am pleased that a book by Kurt Lambeck is about to 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 3×10^{12} watts from the modern astronomic observations, about the same from Babylonian eclipses, and 2×10^{12} watts from oceanographic measurements. By now 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 3.6×10^{12} watts.

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 (13).

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 Alenuihaha Channel with winds up to 30 knots.

It was found that up/down wind components in mean-square slope exceeded the crosswind components by 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. Just this year, 25 years since our experiment, Blyth Hughes 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 to the mean-square slope from waves of different lengths (e.g. the slope spectrum). It is remarkable that the very concept of continuous (or noisy) spectra was unknown to oceanographers in 1946 (when we worked on wave prediction), at a time when these spectral methods were being routinely applied in optics and acoustics. The reason is, I believe, that spectral intensities are most readily measured with resonant filters, and these are simple in optics and acoustics, but difficult to come by at low oceanographic frequencies. It was not until computer algorithms provided numerical filters, equivalent to the analog filters, that the concepts of continuous spectra were really accepted by the oceanographic community.

The strength of the glitter experiment is that it provides a useful statistics 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. In 1952, Crombie and Barber reported such an experiment. They backscattered a radio wave from the sea surface. For a resonant interaction both wavenumbers and frequencies must add up:

$$\mathbf{k}_1+\mathbf{k}_2=\mathbf{k}_3, \qquad \omega_1+\omega_2=\omega_3.$$

Subscripts 1 and 3 refer to the outgoing and backscattered radio waves, and 2 to the resonant ocean wave. For radio backscatter, $k_1 = -k_3$ and so $k_2 = 2k_3$. The associated ocean wave frequency is $\omega_2 = \sqrt{g/k_2}$. 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 λ and resonant ocean wavelength $\frac{1}{2}\lambda$, the current is measured to an effective depth $\lambda/4\pi$. By conducting the measurements at a series of radio wavelengths, Stewart 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 (14). Very roughly this falls off with angle θ measured relative to the wind link $\cos^2(\frac{1}{2}\theta)$ at relatively high winds and high frequencies. We had guessed at something of this sort in our original wave-prediction scheme.

W. Nierenberg and I got a joint Stanford-Scripps effort underway, and participated in some of the analyses. We showed (15) that the 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 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 our 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 n. 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 (16)?

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 (17). 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 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 (18). 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.

This led to the final and most ambitious of these undertakings (19). 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 shorebased 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.[1]

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-generated tsunamis. We found sea level oscillations of 1 to 2

[1] 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.

minute periods at the foot of Scripps pier; these were clearly related to *groups* of incoming swell (20), e.g. the frequency of this "surfbeat" equals the bandwidth Δf of the incoming waves. This experience gave Klaus Hasselmann and me a first opportunity to practice a generalization of power-spectral analysis to nonlinear processes (21), following an important suggestion of 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 $\frac{1}{2}$ 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 (22) 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 (23) which forms the background to the tsunami studies subsequently conducted by Gaylord Miller (24). The source of this background is not known.

We pushed the measurements to lower and lower frequencies, down to the tides and eventually through and beyond the tidal line spectrum (25).

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 (26) 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 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 (27) 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 amphidrome (the point where the tidal component has zero amplitude) in the Northeast Pacific (28, 29). 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 (30). A very naïve 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 $5\frac{1}{2}$ 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 (31)! With regard to tides, an analysis led by B. Zetler was in splendid agreement with the traditional Atlantic cotidal charts (32). 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 (33) 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 finescale mixing. The essential work remains to be done.

OCEAN ACOUSTICS, 1975–

Regardless of the role played by internal waves in ocean microprocesses, there can be no doubt that they 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 $\langle \dot{\phi}^2 \rangle = 1.6 \times 10^{-5}$ 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 (34). For GM75, the result is $\langle \dot{\phi}^2 \rangle = 2.5 \times 10^{-5}$ sec^{-2}. This was the beginning of a major effort led by Roger Dashen and Stan Flatté to derive sound transmission statistics, given the spectrum of the variability in sound speed in ocean space and time (35). Since WWII 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 wetted 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 time 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 (36). 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, Birdsall at 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 identification of each of these arrivals with a distinct ray path remains to be done; this will be my principal task in 1979. If all goes well, we shall put out 4 sources and 6 receivers in late 1980 and attempt to monitor a MODE-sized area by acoustic means over a period of four months.

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 Zetler, Fred Zachariasen, and Chris Garrett. 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 have been Gordon Groves, Earl Gossard, Charles Cox, June Pattullo, Mohammed Hassan, Gaylord Miller, Mark Wimbush, Jim Irish, Jim Cairns, Gordon Williams, Peter Worcester, and now John Spiesberger and Mike Brown. I have learned more from them than they have learned from me.

Among my teachers are threee men in particular: Harald Sverdrup taught me how to write, 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 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, with Freeman Gilbert looking after solid-earth geophysics. 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.

The 1951 nuclear test IVY-MIKE almost brought my scientific career to an end. Revelle, Isaacs, and I had expressed to high authority our fear that the thermonuclear shock to which Eniwetok Atoll was to be subjected might trigger a submarine landslide.[2] 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 Flag Ship 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 records, checking the time marks made prior to my leaving the raft. Within 90 seconds following the final time mark, my wave recorder had malfunctioned, giving a signature equivalent to a huge tidal wave. It is true the "event" occurred too late to be consistent with computations, but I rather think that under the existing stress (and having in mind the possibility of a delayed landslide), had I seen this record signature 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 left the expedition at the first landfall, and not come home.

S. Vigilio di Marebbe
January, 1979

[2] There are very few earthquakes in the area of the Pacific atolls.

Literature Cited

1. Munk, W. H. 1947. Increase in the period of waves traveling over large distances; with application to tsunamis, swell, and seismic surface waves. *Trans. Am. Geophys. Union* 28: 198–217
2. Munk, W. H. 1949. Note on period increase of waves. *Bull. Seismol. Soc. Am.* 39: 41–45
3. Sverdrup, H. U., Munk, W. H. 1946a. Empirical and theoretical relations between wind, sea and swell. *Trans. Am. Geophys. Union* 27: 823–27
4. Sverdrup, H. U., Munk, W. H. 1946b. Theoretical and empirical relations in forecasting breakers and surf. *Trans. Am. Geophys. Union* 27: 828–36
5. Munk, W. H., Traylor, M. A. 1947. Refraction of ocean waves: a process linking underwater topography to beach erosion. *J. Geol.* LV: 1–26

6. Putnam, J. A., Munk, W. H., Traylor, M. A. 1949. The prediction of long-shore currents. *Trans. Am. Geophys. Union* 30:337–45
7. Munk, W. H. 1948. Wave action on structures. *Petrol. Tech.* 11:1–18
8. Munk, W. H., Miller, R. L. 1950. Variations in the Earth's angular velocity resulting from fluctuations in atmospheric and oceanic circulation. *Tellus* 2:93–101
9. Mintz, Y., Munk, W. H. 1951. The effect of winds and tides on the length of day. *Tellus* 3:117–21
10. Mintz, Y., Munk, W. 1954. The effect of winds and bodily tides on the annual variation in the length of day. *Mon. Not. R. Astron. Soc., Geophys. Suppl.* 6:566–78
11. Munk, W., Revelle, R. 1952. On the geophysical interpretation of irregularities in the rotation of the Earth. *Mon. Not. R. Astron. Soc., Geophys. Suppl.* 6:331–47
12. Munk, W., MacDonald, G. J. F. 1960. *The Rotation of the Earth: A Geophysical Discussion.* Cambridge Univ. Press. 323 pp.
13. Cox, C., Munk, W. 1954. Statistics of the sea surface derived from sun glitter. *J. Mar. Res.* 13:198–227
14. Tyler, G. L., Teague, C. C., Stewart, R. H., Peterson, A. M., Munk, W. H., Joy, J. W. 1974. Wave directional spectra from synthetic aperture observations of radio scatter. *Deep-Sea Res.* 21:989–1016
15. Munk, W., Nierenberg, W. A. 1969. HF radar sea return and the Phillips saturation constant. *Nature* 224:1285
16. Munk, W. 1951. Ocean waves as a meteorological tool. *Compendium Meteorol.* 1090–1100
17. Munk, W. H., Snodgrass, F. E. 1957. Measurements of southern swell at Guadalupe Island. *Deep-Sea Res.* 4:272–86
18. Munk, W. H., Miller, G. R., Snodgrass, F. E., Barber, N. F. 1963. Directional recording of swell from distant storms. *Philos. Trans. R. Soc. London Ser. A* 255:505–84
19. Snodgrass, F. E., Groves, G. W., Hasselmann, K. F., Miller, G. R., Munk, W. H., Powers, W. H. 1966. Propagation of ocean swell across the Pacific. *Philos. Trans. R. Soc. London Ser. A* 259:431–97
20. Munk, W. H. 1949. Surf beats. *Trans. Am. Geophys. Union* 30:849–54
21. Hasselmann, K., Munk, W. H., MacDonald, G. J. F. 1963. Chapter 8. In *Time Series Analysis*, ed. M. Rosenblatt, pp. 125–139. New York: Wiley
22. Munk, W. H., Snodgrass, F. E., Gilbert, F. 1964. Long waves on the continental shelf: an experiment to separate trapped and leaky modes. *J. Fluid Mech.* 20: 529–54
23. Snodgrass, F. E., Munk, W. H., Miller, G. R. 1962. Long-period waves over California's continental borderland. Part I. Background spectra. *J. Mar. Res.* 20:3–30
24. Miller, G. R., Munk, W. H., Snodgrass, F. E. 1962. Long-period waves over California's continental borderland. Part II. Tsunamis. *J. Mar. Res.* 20:31–41
25. Munk, W. H., Bullard, E. C. 1963. Patching the long-wave spectrum across the tides. *J. Geophys. Res.* 68:3627–34
26. Munk, W. H., Cartwright, D. 1966. Tidal spectroscopy and prediction. *Philos. Trans. R. Soc. London Ser A* 259:533–81
27. Zetler, B., Cartwright, D., Berkman, S. 1979. Some comparisons of response and harmonic tide predictions. *Int. Hydrogr. Rev.* In press
28. Munk, W. H., Snodgrass, F. E., Wimbush, M. 1970. Tides off shore–transition from California coastal to deep-sea waters. *Geophys. Fluid Dynam.* 1:161–235
29. Irish, J. D., Munk, W. H., Snodgrass, F. E. 1971. M_2 amphidrome in the northeast Pacific. *Geophys. Fluid Dynam.* 2:355–60
30. Irish, J. D., Snodgrass, F. E. 1972. Australian-Antarctic tides. *Am. Geophys. Union Antarctic Res. Ser.* 19:101–16
31. Brown, W., Munk, W. H., Snodgrass, F., Mofjeld, H., Zetler, B. 1975. MODE bottom experiment. *J. Phys. Oceanogr.* 5:75–85
32. Zetler, B., Munk, W. H., Mofjeld, H., Brown, W., Dormer, F. 1975. MODE tides. *J. Phys. Oceanogr.* 5:430–41
33. Garrett, C. J. R., Munk, W. H. 1972. Space-time scales of internal waves. *Geophys. Fluid Dynam.* 2:225–64
34. Munk, W. H., Zachariasen, F. 1976. Sound propagation through a fluctuating stratified ocean: theory and observation. *J. Acoust. Soc. Am.* 59:818–38
35. Flatté, S. M., ed., Dashen, R., Munk, W. H., Watson, K. M., Zachariasen, F. 1979. *Sound Transmission Through a Fluctuating Ocean.* Cambridge Univ. Press
36. Munk, W. H., Wunsch, C. 1979. Ocean acoustic tomography: a scheme for large scale monitoring. *Deep-Sea Res.* 26A:123–61