Stommel

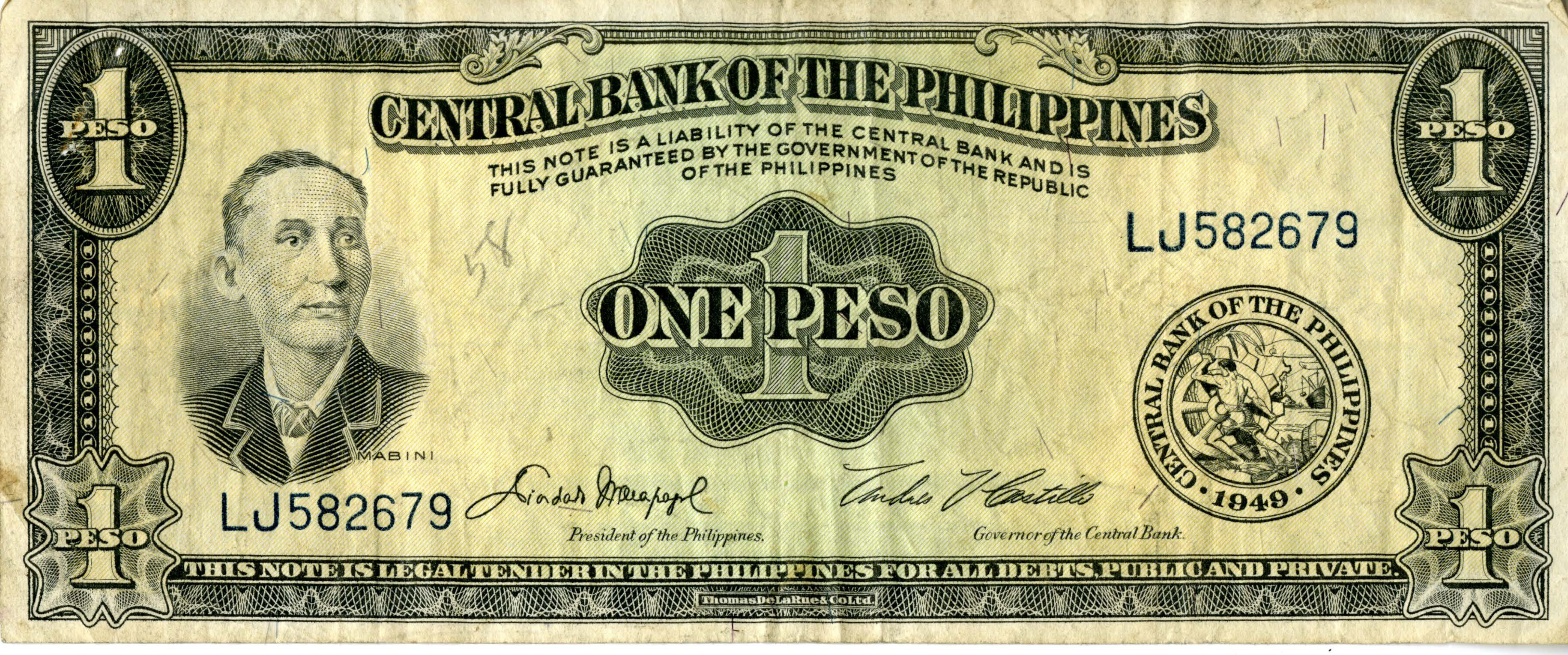
HENRY STOMMEL

Woods Hole, Mass.

Dear Walter :-

Thank you ever so much for sending me the two wonderful Phil. Trans. They are wonderful pieces of work and I will treasure them.

Henry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President of the Philippines.
Governor of the Central Bank.
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Stommel file

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF METEOROLOGY
CAMBRIDGE, MASSACHUSETTS 02139

Dear Walter :—

Here is a one peso idea —

Our invitation to Kamenkovich never came off. But Fedorov says we should keep trying and to ask for one of these

Kitaigorodsky
Kamenkovich
Ozmidov
for example

Hank.

SAN DIEGO: UNIVERSITY OF CALIFORNIA
LA JOLLA, CALIFORNIA 92038

3 August, 1966

DR. WALTER H. MUNK

I agree entirely with your response to Stommel's letter.

I believe that the three dimensional study of large scale events is important by itself. I also believe that the large scale study may shed substantial light on high frequency events also, as I discuss below. What I thus do not understand about his approach is the implication that each of the smaller scale studies is a prerequisite to that of larger scale. I believe that this may be so only if (in my inexact language) the <u>phase</u> relationship of high frequency events is a universally vital requirement for all studies. I cannot convince myself that the <u>intensity</u> relationship of high frequency events is not an equally important matter to correlate with low frequency occurrences.

If high temporal frequency events are progressive and energetic they may possess a high Q. Were this the case the wave length of their envelope variations would be of the order of λQ. Hence for, say, lunar fortnightly planetary waves, the envelope might have a wave length of only 10 or so cpo! Alternatively if the high temporal frequency events are not progressive and are set up by large scale interactions, such as turbulence by winds, the envelope of intensity is again of large dimensions. These points both argue that a rather sparse deployment of stations may reveal quite substantial and significant insight into events of much higher frequency than is implied by the phase coherency constraints.

JOHN D. ISAACS

LA JOLLA: INSTITUTE OF GEOPHYSICS AND PLANETARY PHYSICS
LA JOLLA LABORATORIES

July 26, 1966

TO: PROFESSOR JOHN ISAACS

Would you approve my reply to the enclosed letter by Stommel?
I am glad for suggestions for changes or elimination.

Walter H. Munk

jd

Enclosures

26 July, 1966

Dr. Henry Stommel
Dr. Don Pritchard
Dr. Hugh J. McLellan
Dr. N. P. Fofonoff

Hank Stommel's draft of July 18 is an interesting attempt to organize possible developments into a rational system. May I observe the following?

The accompanying sketch is a frequency-wave number diagram. The units for the latter are in cpo, where "o" applies to any ocean. For simplicity, I would regard the x-axis as representative of the east-west component of wave number.

If the observed phenomenon were the result of the advection of frozen features, then they would fall along an ω, k - line as shown. Features propagating with the speed of surface planetary waves would be along a curve tilted in an opposite direction. One can draw other things on such a diagram.

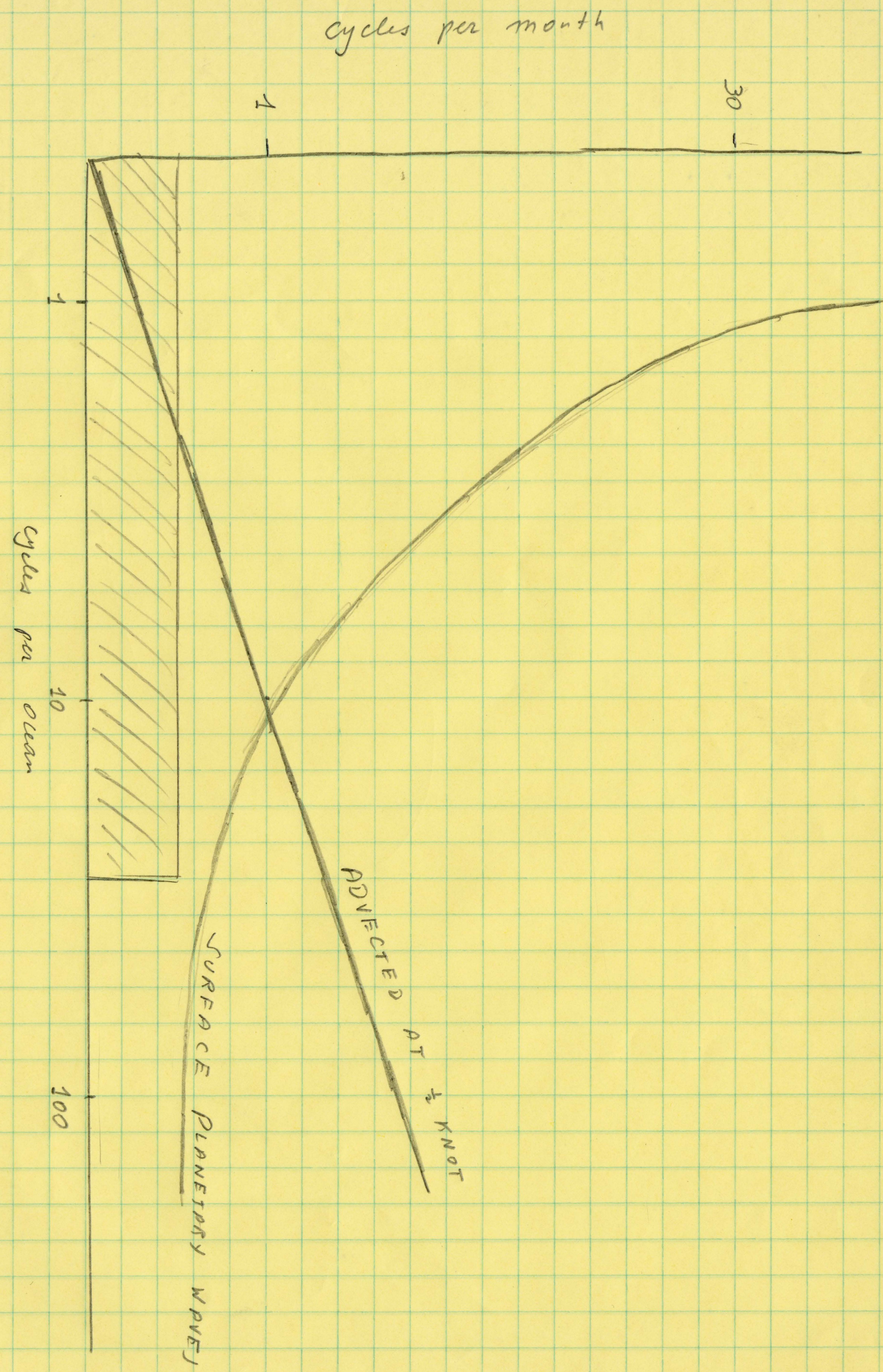
Last week John Isaacs discussed some surface temperature analysis by Jim Johnson of BCF. These consisted of monthly averages in two degree squares. By the sampling theorem, one can cover the shaded rectangle in the ω, k - diagram. The surface temperatures show a meaningful coherency in space and time, and very roughly features seem to be consistent with advection at, say, one-half knot.

A global sparce buoy array a la Isaacs would serve to give you better ω coverage at very low wave numbers, and this would not off hand seem like the best next development. However, John regards these bouys as a subsurface appendix to the surface temperature measurements, and not as an array. This would seem to me a very usefull thing to do.

John is also thinking of other uses of his buoy program, but I wanted to bring up these particular points in augmentation of Stommel's remarks on the bottom of page 9.

Walter H. Munk

jd/mtd
cc: John Isaacs


cycles per month
1
30
1
10
100
cycles per ocean
ADVECTED AT ½ KNOT
SURFACE PLANETARY WAVES

WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS 02543
AREA CODE 617-548-1400

July 19, 1966

2° sq
Jim Johnson BCF

Dr. Don Pritchard
Dr. N. P. Fofonoff
Dr. Walter H. Munk
Dr. Hugh J. McLellan

Enclosed is a somewhat expanded and more detailed version of Appendix II of the EFFECTIVE USE OF THE SEA. Unfortunately in the course of filling in detail I have made the tone too personal, but I think it includes some ideas and points that are insufficiently discussed in the above Appendix.

Two weeks ago I sent a copy to HJM to see if it is the kind of thing which he wanted according to his letter of July 1. He replied that it is the sort of thing he needs, but of course before it can be useful it needs much more study: (1) are there other projects that should be inserted in the program? (2) what are the needs in instrument development for the program? (3) what are the ship needs? (4) what are the views of engineering specialists? (5) what are the dollar needs?

I should be very grateful to you for your collaboration in helping to produce a balanced document for future ONR use.

Yours truly,

Henry

Henry Stommel

HS:mr
Enclosure

July 18, 1966

Draft - H. Stommel

A ZERO ORDER LOOK AT PHYSICAL OCEANOGRAPHIC MEASUREMENT FOR THE NEXT SEVEN YEARS

1. The Need to Look Ahead

Oceanographers are presently struggling to free themselves from too heavy orientation toward geographical exploration, as exemplified in the work of the International Geophysical Year and the International Indian Ocean Expedition. They are seeking full-scale geophysical schemes of measurement which will reveal basic information about oceanic physical processes. An example of such a global experiment is the work of Snodgrass, et al. (1966)* on the propagation of ocean swell across the Pacific in which observations were obtained simultaneously at six oceanic sites along an arc of a great circle from New Zealand to Alaska. Clearly we need to formulate other types of observational study of the very complicated field of motion within the ocean. The Panel on Oceanography of the President's Science Advisory Committee has issued a report entitled EFFECTIVE USE OF THE SEA. Appendix II of that report suggests that something like the program shown on the diagram (figure 1) will eventually evolve. But the time scale of this evolution, and the efficiency with which the program will be carried out, cannot be foreseen clearly, and is a matter of concern to oceanographers.

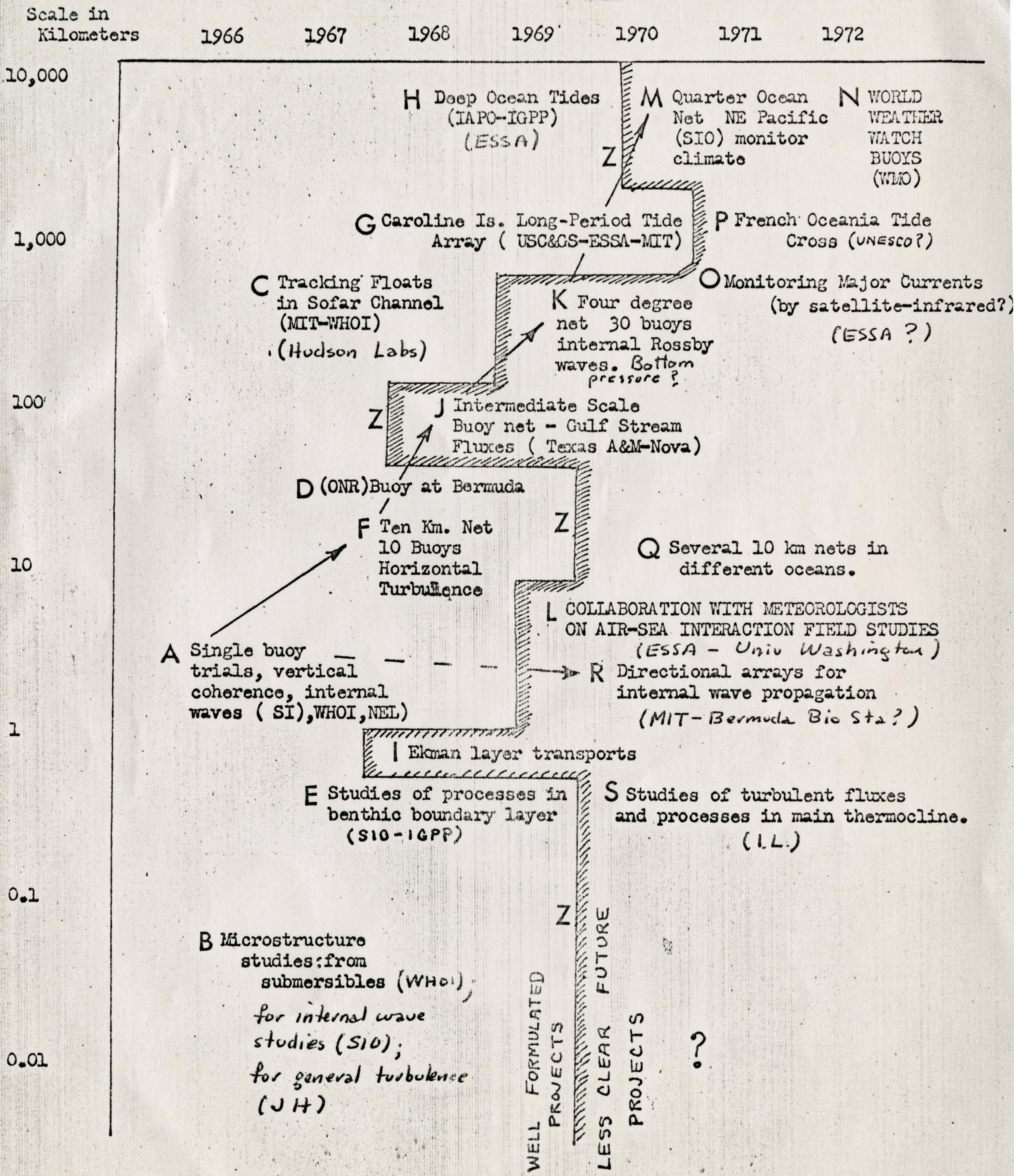
2. Efforts to Formulate a Program of Large-Scale Measurement

This concern has become manifest in many ways: in the deliberations of international groups like the Intergovernmental Oceanographic Commission

* 1966 Snodgrass, F. E., G. W. Groves, K. F. Hasselmann, G. R. Miller, W. H. Munk, and W. H. Powers: Propagation of ocean swell across the Pacific. Phil. Trans. Roy. Soc. London A 259, 431-497.

FIGURE 1

OUTLINE OF FUTURE LARGE SCALE PROJECTS IN PHYSICAL OCEANOGRAPHY



of UNESCO in Paris, the Scientific Committee of Oceanic Research of ICSU, and at the national level in many countries. There is a growing consensus among physical oceanographers to find ways to shift the emphasis in oceanography from exclusive preoccupation with the methodology of geographical exploration, to a new methodology centered on revealing the physics of the processes at work in the ocean.

In the United States the Geophysics Branch of the Office of Naval Research has led the way in encouraging oceanographers to develop plans for future programs of measurement and appointed, under the chairmanship of Dr. Donald Pritchard of Johns Hopkins, an Ad Hoc Committee on Eulerian Measurements in the Ocean to prepare recommendations for the immediate future. In order that the recommendations of the PSAC Panel and of the Pritchard Ad Hoc Committee be implemented it is necessary to formulate future plans in ever increasing detail, and also to try to look as far ahead as possible. The following Plan is meant as such a glimpse of future possibilities for the next seven years. The outline is a rough sketch of possible plans and projects that may be expected or desired in the immediate future, projecting present ideas about seven years ahead. The projects are listed in a coordinate space which has calendar year as abscissa, and scale of phenomena investigated as ordinate.

3. The Seven Year Plan

A. Single buoy trials are already underway at the Woods Hole Oceanographic Institution and at Scripps. These are mostly devoted to testing the new equipment, trying to discover weaknesses of the design of the current meters, recording systems, etc. Such trials are essential for getting preliminary estimates of the statistics, power spectra, etc., of the variables to be measured, and for evaluating the requirements for sampling and density of measurement in the vertical and in time. The instrumented mooring is likely to be the basic element of later arrays made up of many moorings arranged in various geometrical configurations. Therefore, much must be learned about its characteristics as a measuring tool, before some of the more elaborate experiments listed in the outline can be designed definitely. Even after the array experiments have begun we must expect further work with single moorings -- especially in the testing and verification of new instrumentation.

B. Microstructure studies have received stimulus from the success of new continuous recording salinity-temperature-depth recording devices such as the Bisset-Berman STD. At present this instrument is being used mostly as a device supplementary to the Nansen bottle and reversing-thermometer station. However, it is clear that the microstructure so revealed is interesting and important for its own sake: it probably is related to the mixing processes which characterize the dynamics of the main thermocline, and which play a dominant role in the general circulation of the ocean. Moreover, this small scale structure cannot be entirely neglected in planning and interpreting the data from moored systems (A, above) because there is a need to evaluate the effect of space-aliasing from data sampled at low wave-number in the vertical. Initially these studies can probably be carried forward using existing STD instruments from surface ships and present submersibles. But very soon special platforms capable of staying with given parcels of water for periods of several days will be necessary in order to monitor the development of these microstructures with time, and from preliminary plans drawn up by the Instrumentation Laboratory at M.I.T. these servo-controlled platforms may be moderately expensive to develop: for example one development project is estimated to cost $3,000,000 over a four-year period. If measurement of turbulent fluctuations on this scale proves feasible it seems probable that it will evolve into a separate study of its own, S.

C. Neutrally buoyant floats have been tracked since 1955 when Swallow developed a successful float and pinger. The chief obstacle to wide use of such floats for measurement of ocean currents has been their limited range; however, recent work at Woods Hole and M.I.T. has demonstrated the feasibility of acoustic ranges in the Sofar Channel of 800 miles and plans are afoot for carrying out a pilot experiment tracking several long range pingers for several years between Bermuda and the West Indies. This is not a Eulerian measurement of course, but the drifting float has certain advantages in naturally filtering out high energy high frequency components of velocity which are hard to achieve in moored current meter work. It seems therefore that the float will be useful auxiliary to long-term buoy experiments (such as K). The float is also likely to find special application in regions of peculiar interest such as in undercurrents, deep troughs, the centers of intense eddies, etc. If the acoustic range can be extended beyond 800 miles the float should become a tool of great interest, and could be used for constant monitoring of such remote streams as the Antarctic Circumpolar Current, etc.

D. (ONR) Buoy at Bermuda. The mainstay of oceanographic research has been the Nansen water-bottle and reversing thermometer station. It is therefore logical to try to extend the usefulness of this type of measurement by putting suitable sensors on a moored buoy so that stations can be made frequently for long periods without the need for anchoring a ship in one place for a year. In this manner a relatively high density of sampling can be obtained for extended periods of time. There is evidence of an interesting high degree of coherence in the cospectrum of sea-level given by the Bermuda tide gauge and the steric level computed from the PANULIRUS station data. Because the stations are taken only twice a month this coherence can be demonstrated only for frequencies less than one cycle per month. The ONR buoy would make it possible to extend the cospectral analysis to higher frequencies -- at least up to one cycle per day. It should yield information of considerable interest toward our understanding of the dynamics of the deep ocean. After a year or two at Bermuda the buoy can be moved elsewhere, for other types of experiments -- perhaps as a part of projects J or O or perhaps on the Faero-Shetland Channel.

E. One of the most practicable places to make measurements in the ocean is in the bottom boundary layer. At present very little is known about the dynamics of this region -- for example, can we obtain reliable and representative estimates of the mean stress on the ocean bottom due to the ocean currents above? This is an important and at present unknown ingredient in the theory of the general circulation of the ocean. But there is much more to discover about the bottom boundary layer than the answer to this simple question. In the next few decades we can expect considerable engineering development and construction for industrial purposes on the ocean bottom. There will be a need for detailed information about the microscale processes on the bottom.

F. As the first experiments (A) using single buoys are successfully completed, it will become feasible to make measurements over various horizontal scales. These will probably first be small horizontal scales of a kilometer or so, and gradually the scale will be extended. Thus there is a linear progress in the sequence A - F - J - K - M, which progressively encounter larger scale phenomena. The Ten Kilometer Net (F) is on a scale appropriate for study of internal waves of periods near to that of the tides. The nets might be maintained for relatively short periods - perhaps three months. A study of horizontal turbulence might require rather longer periods of measurement however, and perhaps could be done under project J instead. Should it turn out that internal waves are able to propagate for significant distances, perhaps a development of directional arrays, R, will evolve.

G. Recent analysis of tides of long period by Wunsch makes it fairly certain that the fortnightly and monthly tides are not purely equilibrium tides nor is their scale as large as that of the tidal producing potential or even of the oceans, but much smaller -- of the order of 1000 miles. The response of the ocean to these components of the tides therefore are in the nature of geostrophic Rossby waves. In order to learn whether these waves have a progressive character or are locked to features of bottom topography it will be necessary to make measurements with digital tide gauges at an array of mid-ocean islands. The Marshall and Caroline Islands are arranged in the Pacific in such a way that a line 40° in longitude of about ten gauges extended in the east-west direction is possible. Four years of measurement are necessary. Simultaneity makes it possible to suppress large scale noise. If the results of this study look promising it might be possible to arrange a true cross some day in French Polynesia, (P) where the islands are more suitably disposed for a two-dimensional array.

H. Plans have been laid under the auspices of the International Association of Physical Oceanography for an international survey of the high frequency tides over the whole ocean, using newly developed bottom pressure gauges. (Snodgrass, Munk, and Eyries).

Z Z

I. It should be possible to make measurements of wind-drift transport in the upper boundary layers of the ocean, using the new buoy technology. This will help to make better estimates of the stress of the wind on the sea: in addition it would be a useful supplementary study to make in collaboration with meteorologists (L) when they someday embark upon field studies of air-sea interaction and vertical exchange processes in the air over the sea, making measurements on various horizontal scales. In the past, meteorologists have tended to depend exclusively on the existing network of observing stations, and when they attempt to undertake studies on a smaller scale (such as air-sea interaction) often have not made provision for the rather extensive meso-scale observing nets which would be desirable. An example was the 1953 Anegada stress studies made by a joint U.S. - British team in the West Indies. Micro-measurements were made at single sites; large-scale measurements were available from the existing weather reporting net; but there was an important intermediate scale which was not measured, and the lack of this information about intermediate-scale horizontal temperature structure made the results uncertain. At the time it was impossible to finance an intermediate-scale net -- which would have been more expensive than the original operation -- but informal sources of information indicate that the British and Dutch are finally going to attempt such a study, probably by 1970. The studies most probably will be in the Caribbean. The U. S. Trust Territories in the Pacific might be an even more favorable site.

J., K. Oceanographers at Texas A & M and at the University of Miami have expressed an interest in making detailed measurements of transport fields in the Gulf Stream on the Blake Plateau off the Carolinas. We can also anticipate experiments designed to determine the properties of irregular motions in the deep mid-ocean, as indicated by the project K, designed for discovering more about the nature of the type of eddies observed by Crease and Swallow off Bermuda in 1959-1960.

M. Even larger scale networks of buoys have been contemplated, for example the suggestion by John Isaacs at Scripps Institution of Oceanography that it would be feasible to monitor large portions of the Pacific Ocean by a rather widely dispersed network of buoys designed to detect persistent anomalies in surface temperature. Evidence of these has already been brought forward on the basis of conventional ship reports of surface temperature, and it has been suggested that these anomalies have important influence on climatic conditions of western North America. Perhaps schemes such as Isaacs' will in the future help forecast these persistent anomalies, and in this way contribute to long-range weather forecasting.

In a field like this one it is not possible to determine very far ahead the kind of experiments that oceanographers will find interesting and profitable to pursue. Projects A through H seem to be in rather clear focus, and can be reasonably formulated and planned. On the other hand, projects I through S are not so clearly formulated, and it seems that they can only be listed tentatively as a rough indication of what the future may hold in the way of Eulerian measurement. The dividing line is shown by the hach?ured line Z. The outline is written in terms of field operations. Parallel outlines indicating ship needs, instrument-development needs, financial requirements, data-analysis requirements, ought to be drawn up and analyzed because preparation for these next steps are not presently being made. For example, S, studies of turbulent fluxes in the main thermocline can probably be made only with the construction of a special robot probe -- and this could involve three years of engineering development. At present there is no master plan to provide guide lines for those planning future needs of oceanography, and it seems that therefore under the present mode of planning and operation the program of work indicated in the outline will require twenty-five years instead of seven.

28 August 1969

Dr. Henry Stommel
Room 54-1416
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Hank:

Your note concerning the mid-ocean dynamics experiment sounds ambitious and worthwhile. I would support it if anybody asked my opinion, provided the people who run it are competent and deeply committed. I am sure those are your views also.

I know that both of ~~you~~ us are interested in bottom pressure sensors. The Hewlett-Packard crystal looked awfully good, but suddenly in mid-summer, Hewlett-Packard cancelled out on us with the view that they could make more money selling to the oil industry. I flew up to see Mr. Hewlett, and apparently succeeded in convincing him to make a special delivery of two units only on a trial basis, no guarantees.

Frank and I will plan to learn something about the units after they get here; and if they look as good on the sea bottom as they do on the bottom of the atmosphere, then we should do something about it.

I wish I knew your whereabouts. Judy and I will be in Italy in October for reasons that are not oceanographic.

Your friend,

Walter H. Munk

WHM:cg

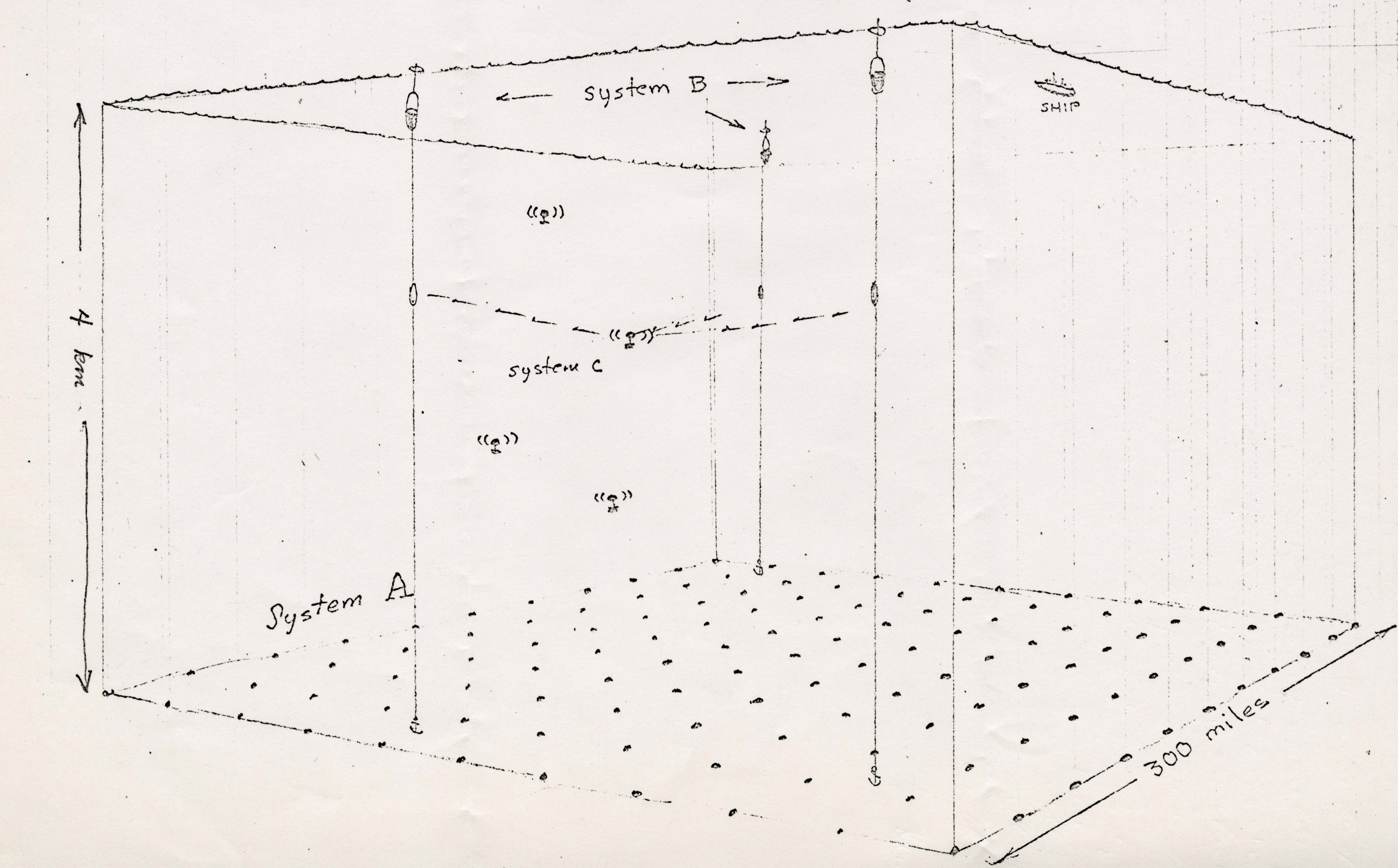
HENRY STOMMEL
ROOM 54-1416 M. I. T.
CAMBRIDGE, MASS., 02139

Walter,
Carl & I have had this notion of something for Lincoln Lab to do. Does it seem worthwhile to you? And could it be useful for study of tidal interaction with the bottom?
Henry

August 11, 1969

Last week several representatives of the Lincoln Laboratory met with Dr. Carl Wunsch, Dr. Jule Charney and myself to discuss possible types of research in meteorology and oceanography which might be appropriate for them to undertake in the next few years. Lincoln Laboratory has previously undertaken rather large projects such as the missile tracking laboratory at Kwajalein, and several imposing seismic arrays in Montana, Norway, and Alaska. At present their sponsoring agency has instructed them to explore other geophysical-type applications for their technical competence, and this statement of a fundamental oceanographic problem and means of studying it realistically grew out of the discussion. I believe that this project would be very important for the development of understanding of ocean circulation, and that it could establish a new level of achievement for scientific study of the ocean. The cost of \$10-\$15 million dollars comes from a source for which present oceanographic funding is not competing, and is considerably less than what would be involved by other schemes such as a 100 moored-current-meter array.

Henry Stommel



system B
SHIP
system C
System A
4 km
300 miles

Mid-Ocean Dynamics Experiment

Is the dynamics of the ocean similar to, or very different from, that of the atmosphere? For about twenty years the theoretical ideas concerning these two fluid envelopes of the earth have been markedly contrasting: on the one hand we view the atmosphere as a highly non-linear fluid-dynamical flow, with large eddies playing an essential and dominant role; on the other we view the central ocean as a steady smooth flow, conforming to the so-called Sverdrup dynamics, in which transient motions can be computed by perturbation theory. This latter interpretation may actually be as incorrect and irrelevant as the Hadley theory is to the atmosphere, and indeed certain measurements of deep central oceanic flow, such as those conducted by Swallow and Crease off Bermuda give clear warning that this may be the case. The purpose of this proposal is to indicate a feasible experiment which can be carried out to decide this matter. It appears to be well within the technological capability of the Lincoln Laboratory. It would provide the data needed for experienced meteorologists to diagnose the dynamics of the ocean, and would provide the data necessary for developing realistic numerical models of the ocean.

We envisage obtaining synoptically a map of bottom pressure over a limited area in mid-ocean, similar to an atmospheric pressure map on land. The area may be chosen to lie in the center of a subtropical gyre (north of the Marshall Islands) where the Sverdrup-relation is assumed to obtain; the area should be large enough to contain several oceanic eddies of a scale similar to those observed by Swallow and Crease: say a 5 degree square. There should be enough bottom pressure gages to resolve these eddies: with a 30 mile spacing, 121 gages. They should have a sensitivity of about 0.3 cm., and since absolute pressure at the bottom of the ocean with such high sensitivity seems unattainable, we should use only variations from a fixed pressure established at the bottom upon settling on the bottom. The pressures recorded or preferably telemetered acoustically, should be half hour means every half hour. The plan of the bottom pressure gage layout is shown in the sketch as system A.

System B is a set of three or four moored hydrophones set out in a pattern to obtain good base lines for acoustic telemetering from the pressure gages, and from System C.

System C is a set of pinging constant-level floats - similar to ones being tested by Webb and Rossby - but probably smaller on account of the small range - floating at four different depths. These are tracked by arrival times of signals at System B. We envisage about 40 of these floats in the area at any time. As they float out of the area they will need either to be recalled to the surface and replaced at regions along the boundary where there is an inflow, or new ones need to be inserted at these points. In the latter case we estimate that 500-1000 floats will be needed as replacements during two years of operation to keep the population density at about 40. System C will therefore require nearly constant attendance of a ship.

System D will be a computer-numerical-model for prediction of float positions. This will be desirable for (1) determining positions where new floats should be placed (2) to build scientific understanding of the experiment during the experiment, so that modifications of spacing etc. can be made. System D could be on board the attending vessel, or it could be ashore, with facsimile transmission of charts to the ship.

System E is optional, and not shown on the figure. It consists of a moored-current-meter array: it could be provided by a cooperative arrangement with the U.S. Coast Guard National Data Buoy Project, or perhaps in collaboration with the Soviet Union which is demonstrating marked capability in this field.

System F is not part of the field experiment. It is the whole complex of theoretical and numerical studies which will be carried out to understand the dynamics of the field study, to evaluate the statistical properties of the ocean eddies, and their role in the general circulation of the ocean. We anticipate that the eddies will be found to play a dominant role in the dynamics of the oceanic circulation, and that our whole theoretical concept of ocean currents, developed over the past twenty years will be changed.

In a short statement such as this, it is, of course, impossible to discuss details of the systems to be used, auxiliary observations, pilot programs, test procedures, simulation studies, etc.

of our national history, I may mention that the recent president of the AGU, Frank Press, is now President Carter's science advisor. Another Fellow of the AGU, Robert Frosch, has been nominated as administrator of the National Aeronautics and Space Administration. Another, Jack Schmidt, is a U.S. Senator. Four of the recently selected Congressional Fellows are members of the AGU. Numerous other geophysical colleagues, in high positions within the federal government, may be cited.

Inasmuch as most of the nearly 12,000 members of the Union are much younger than I, perhaps I may be pardoned for offering some advice at this point. My advice is brief and is in three parts: devotion to one's profession, perseverance, and longevity. Bowie himself was a sterling example of these qualities by his devotion to expanding and strengthening the AGU and by his own powerful and pervasive contributions to geodesy, isostasy, and cartography over a long professional career in the U.S. Coast and Geodetic Survey.

At a session of the AGU about 10 years ago, Sidney Chapman, the Bowie medalist of 1962, was observed to be listening intently to a series of papers. A friend leaned over and whispered to him, 'Sidney, I find it rather strange that you sit here listening to papers by youngsters less than half your age.' Sidney gave him that famous sidelong, quizzical glance and replied, 'My dear sir, I am older than almost everyone. If I could not learn from my young colleagues, I would be in a desperate plight.'

Every year of my life, I have a keener appreciation of Sidney's remark.

On this note, I will conclude by reporting that over the years, I have learned most of what I know from my young students and collaborators at the University of Iowa and from colleagues elsewhere, many of whom are much brighter than I am. It is only in the context of perseverance and longevity that I could possibly consider accepting this splendid Bowie Medal.

James A. Van Allen

Eighth Presentation

WALTER H. BUCHER MEDAL

to

BRUCE C. HEEZEN

for original contributions to the basic knowledge of the earth's crust

It is with a deep sense of honor that I have prepared this citation for Professor Bruce Charles Heezen as the recipient of the Walter Bucher Award at our American Geophysical Union Banquet Dinner. Bruce has gained a world-wide reputation for his continuing and total dedication to the understanding of submarine geologic features, from ripple marks to the Mid-Oceanic Ridge. The physiographic maps drawn with his distinguished research associate, Marie Tharp, still serve as our first conceptual introduction to the physiography of our planet.

Professor Heezen continues to strive toward having explanations for what he observes from his exceptionally perceptive eyes. He and Marie have provided first insights into many concepts now considered mundane. An example is the first realization of the continuity of the globe-circling Mid-Oceanic Ridge. This discovery quickly led to the present understanding of our dynamic earth and to processes of sea floor construction and destruction.

Another example of his tremendous breadth of interest is his recent work in deep submersibles such as *Alvin*, *Seacliff*, and the Nuclear Research Submersible *NR-1*. He has, I believe, logged more hours looking at the bottom of the ocean (*The Face of the Deep*) through the porthole of research submersibles than any other oceanographer.

While writing about the tectonic implications of the spreading Mid-Oceanic Ridge, he entered the field of sediment dynamics with his correlation of cable break failures to the erosive as well as carrying power of submarine turbidity currents. He led the field by first recognizing the importance of this process as a major distributer of coarse material onto the abyssal plains, another feature he helped discover while working as a graduate student under the late Professor Maurice Ewing (the sixth Bucher medalist).

Heezen's name is also found associated with other important phenomena observed in the deep ocean, such as the occurrence of microtectite strew fields, with some thought-provoking concepts concerning their cosmic significance. He also was one of the first oceanographers to recognize the importance of abyssal bottom currents as a significant submarine geologic process.

He has worked knowledgeably on data that sweep the entire spectrum of modern oceanographic research, from potential temperature and salinity to sediment types and rock formations to earthquake epicenters and the interpretation of seismic reflection profiles.

Although his far-ranging ideas may at times seem controversial and not always accepted at the outset, it is his ability to draw very broad implications from observational data that stands as his major contribution to our present understanding of the planet. It is always interesting to read one of his many (327) papers because there is often a stab at some broad global concept hidden in the discussion section. His writings more often than not lead to research proposals designed to prove him wrong, but later work tends to confirm his speculations.

It is with deep appreciation, for what we all have learned from Bruce's dedicated efforts, that I present him to you, Mr. President, to receive this Walter H. Bucher Award for original contributions to the basic knowledge of the earth's crust.

Charles D. Hollister

Acceptance and Response

Thank you, President Maxwell, Dr. Hollister, and all of those who have judged our work worthy of this great award. To have one's work so highly praised and so grandly acknowledged is of course a very great satisfaction, but perhaps more importantly it is a great spur and impetus to further efforts. I am very grateful for both satisfaction and impetus.

This honor is to me a double one, for it was Walter Bucher who as my teacher instilled a lifelong interest in the global patterns of the earth's crust. When Maurice Ewing was selecting the scientifc problems to be pursued at the newly established Lamont-Doherty Geological Observatory, it was to Walter Bucher that he turned for advice and counsel. I was then a student of both men, and the course of my subsequent work was in great measure the result of opportunities opened by Ewing and the priorities and infectious enthusiasm of Bucher.

I must share this honor with my students and associates and particularly with Marie Tharp. I think it is appropriate that I acknowledge the indispensable support of the U.S. Navy and the National Science Foundation without which our work would not be possible.

Bruce Charles Heezen

Bruce Heezen died on June 21, 1977.

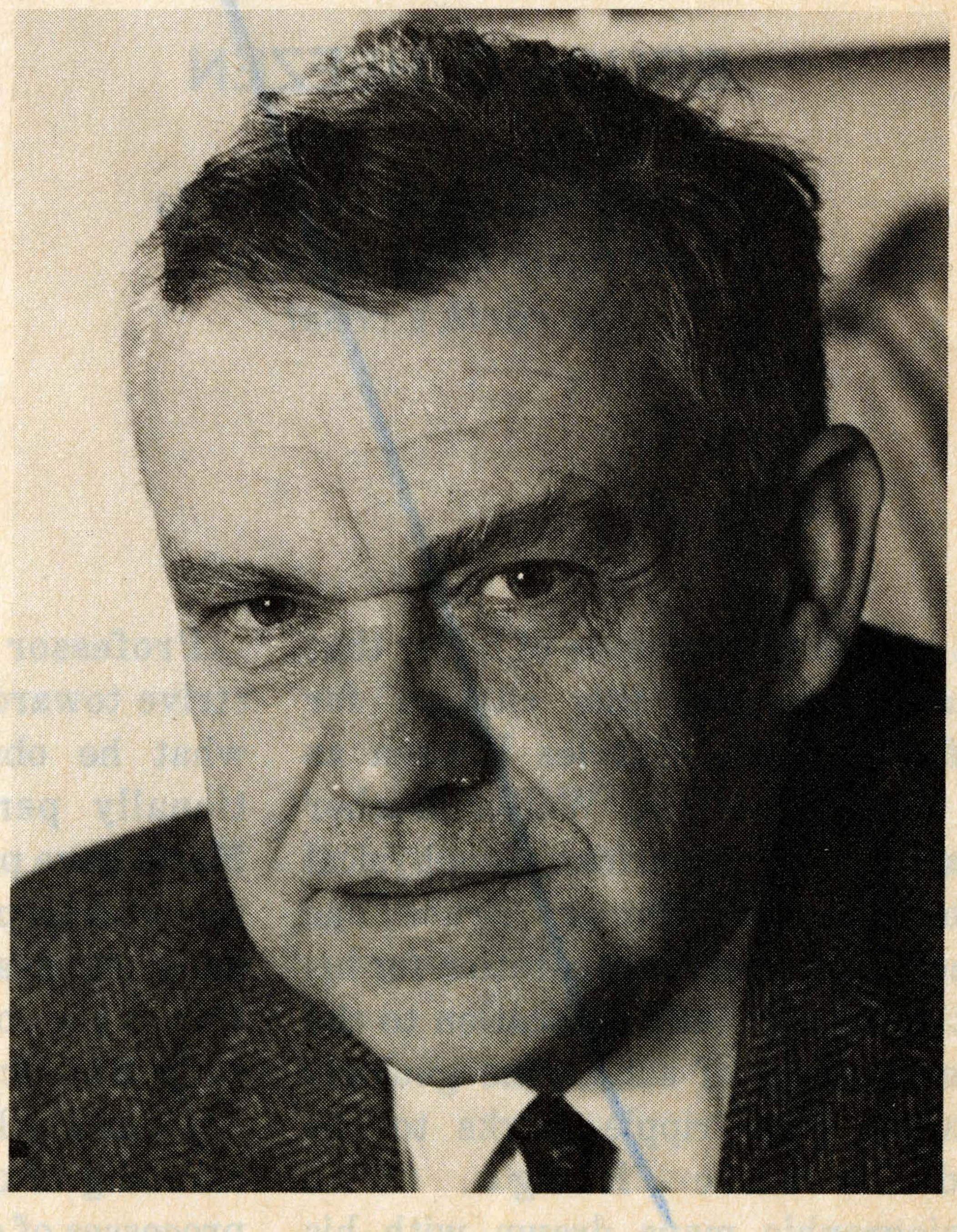
Second Presentation

MAURICE EWING MEDAL

to

HENRY M. STOMMEL

by the American Geophysical Union and the U.S. Navy for profound and imaginative contributions to physical and dynamical oceanography



The second recipient of the Maurice Ewing Medal of the American Geophysical Union is Henry M. Stommel. He was selected for his profound and imaginative contributions to physical and dynamical oceanography during the past generation. Probably the most truly creative physical oceanographer of his time, Stommel has made fundamental contributions to an astonishingly diverse range of problems, from microstructure and estuaries to the general circulation of the ocean. He has used almost all of the tools available to a physical scientist: theory, work at sea, and laboratory experiments. No other oceanographer has managed to combine the purely naturalist aspect of the field—geographical exploration at sea—with such revolutionary theoretical contributions.

Stommel's work is epitomized by his consistent ability to start with what appears to be a simple idea and carry it by some intellectual leap to the point where it yields a deep insight into some previously unsolvable problem. Many of his insights of this kind have grown to spawn virtual industries within the field of physical oceanography. Perhaps Stommel's most famous paper was his 1948 explanation of the existence of the Gulf Stream and other western boundary currents. Using very simple mathematics, he produced a model with all of the qualitative features of the large-scale ocean circulation. Over the next 15 years, working alone and with others in the role of goad, midwife, and general fountain of inspiration, he and his colleagues produced a sequence of ocean circulation models of increasing complexity and reality. But without that original insight and that very simple mathematical model, progress would have been much slower. The list of such insights that Stommel has had is very long. They range from an important idea about cumulus cloud convection, to what was initially seen as a curiosity, the salt fountain, to some seminal ideas about the structures of estuaries, to an entire model of the abyssal circulation of the ocean based upon, again, some deceptively simple ideas about the way a large-scale rotating fluid had to behave.

Many of Stommel's ideas about the general circulation of the ocean were brought together by him in his book *The Gulf Stream*, a classic scientific monograph. The volume has served as a virtual handbook of the field in the 20 years since its first publication.

Not the least of Stommel's accomplishments has been his ability to spawn so many ideas that it takes teams of oceanographers to follow them up. The key ingredient, of course, is his ability to inspire others with an excitement about the possibilities for experimental programs large and small that they had not thought of themselves. He produces so many ideas that once a program was in good hands, he would simply go on to something else. The list is very long; it includes the International Indian Ocean Expedition, Geosecs, Mode, the Sofar float program, Medoc, and on and on.

He has inspired, led, and pushed an entire generation of oceanographers. He has resisted any direct administrative power, and his impact on the field which is so enormous is solely due to his ability to lead through sheer force of personality and sense of excitement. To those capable of staying up with his quicksilver mind, jumping at an astonishing rate from one aspect of a problem to another, punctuated by a vibrant and unpredictable wit, a scientific interaction with him is an exhilarating experience. A sense of wonder about the world is what seems to motivate him. His interests range from being what he calls a 'part-time peasant' to a passion for locomotive steam whistles to almost anything gadgety or bizarre. He can find something interesting to talk about or do, whether in some squalid port in a remote corner of a remote ocean or in his own office exploring something odd seen in someone's lab.

This ability to give away ideas is perhaps uniquely Stommel's. It is characteristic of his generosity to all his colleagues and students. He has remained accessible and open to anyone with even the remotest connection with oceanography and is openhanded with all of the resources, both intellectual and material, at his disposal.

Henry Stommel's scientific career began in astronomy at Yale, took him to the Woods Hole Oceanographic Institution, thence to Harvard University, and finally to Massachusetts Institute of Technology where he is professor of oceanography. While he has had his share of formal graduate students, there have been hundreds of others who have sought his teachings in a less formal sense. He is the resident genius of the northeast, with an impact that has been world-wide.

Carl Wunsch

Acceptance and Response

Most of human history has not afforded men much chance to pursue their curiosity—except as a hobby of the rich or within the refuge of a monastery. We can count ourselves fortunate indeed to live in a society and a time when we are actually paid to explore the universe.

I have been interested in science as long as I can remember—as a boy chemist and amateur astronomer. It was as an undergraduate at Yale that I became aware, through coming up against mathematical physics, that I had a singular lack of aptitude for abstract thought of any complexity. For example, there is a book called *Foundations of Physics* by Margenau and Lindsay that I once tried to master. It was my Pons Asinorum and has remained a closed book to me, although it is still on my shelf. After graduation I found myself at Woods Hole with 'Doc' Ewing as my first boss. He really was a boss. Since then I have worked for and with other scientists, but none was so clearly a 'boss' as Doc. Single-minded, determined, driving, Doc Ewing was the oceanographic equivalent of General Patton. His magnificent achievement in global data collection is unchallenged, his team at Lamont gathered more observational material on the submarine geophysical structure than any comparable group. They really worked hard for Doc Ewing, and he demanded it.

The nickname Doc seems to have gone out of fashion these days, like wearing Phi Beta Kappa keys. It seems to have been a product of those days when Ph.D.'s in earth sciences were rare. It was symptomatic of a carefully gaged familiarity—no first name of course—its use indicated respect and loyalty to the team and to him.

Many scientific teams were built around such leaders and in such a style. Perhaps they still are. Anyway, working under such strong leadership is not my style, and I found something else to do at Woods Hole as soon as I could. Therefore it does feel a little bit strange to receive the Ewing Medal, and I hope Doc doesn't mind.

It is difficult to respond properly to such a generous citation as I have just received. First, we must remember that a citation, like a letter of recommendation for a new job, tends to emphasize the positive and, if the writer is a personal friend, somewhat exaggerates the good points one may have. Fortunately, most of us have some good points, and it does not require very many good points to do some useful scientific work.

It would appear that one's weak points are equally important in determining the direction of one's efforts. They also should be recorded.

Science has a way of constantly facing us with our limitations: we quickly see that we do not have certain mathematical skills or ability to master large amounts of information, or that our practical experience with instrumentation, electronics, programing is too limited to cope with most types of problems. Therefore much of our effort is consumed in trying to find problems that we can solve. Anyway, that is the way my mind works: it continually flits around encountering problems that I cannot begin to solve. This goes on for months or years and can be rather discouraging. It begins to seem that all the solvable problems have been solved. Then finally some idea pops up that I can do something with. It can lead to a few weeks of fun and gratification. But the price has been many months of frustration.

When I contemplate the superb skills of some of my colleagues as mathematicians, as instrument designers, as masters at squeezing information out of masses of data, as scholars of encyclopedic knowledge, as scientific administrators with considerable power of decision, I realize how limited and amateurish my own skills are. Therefore when I get an idea, I simply have to pass it on to someone else who has the skill to develop it. That is really not generosity—it is just being practical.

So I have learned something to share with you:

The self-knowledge that you are something of a hacker does not need to discourage you. Just apply yourself, and use the talents that you have. You are bound to find out something.

Henry M. Stommel

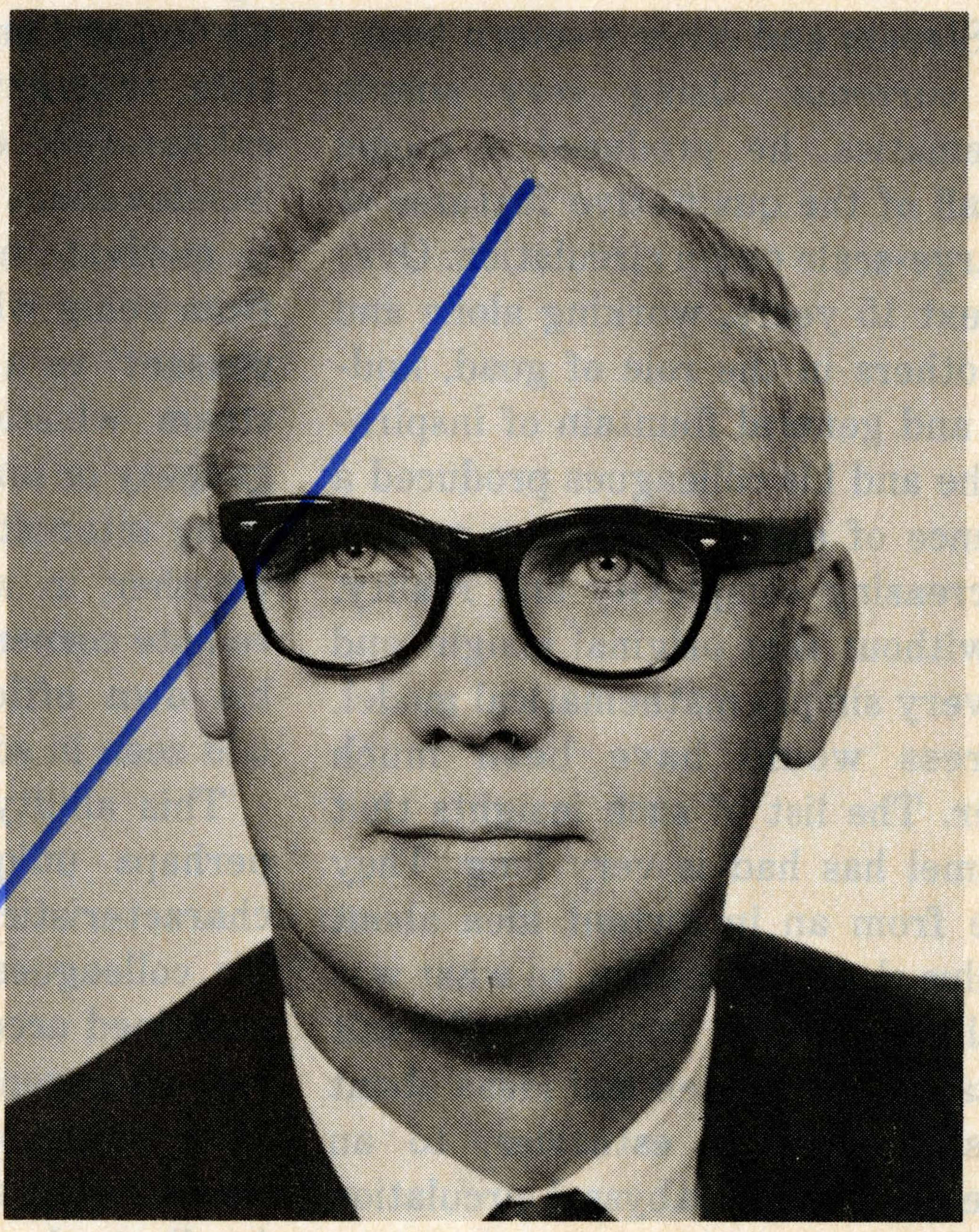
Fourteenth Presentation

JOHN ADAM FLEMING AWARD

to

FRANCIS S. JOHNSON

for original research and technical leadership in geomagnetism, atmospheric electricity, aeronomy, and related sciences



Francis Severin Johnson was born in Omak, Washington, in 1918 and was brought up in Canada, receiving his B.S. degree in physics in 1940 from the University of Alberta. He received an M.S. degree in meteorology from UCLA in 1942 and his Ph.D. in 1958. He served in the U.S. Air Force as a weather officer and forecaster during World War II. He joined the Naval Research Laboratory's optics division in 1946. As head of the high-altitude research section he participated in the development of ultraviolet spectrographs and promoted their use on V2, and later Aerobee, rockets. He published the first rocket measurements of the vertical profile of atmospheric ozone in 1951 and made the first precise evaluation of the solar constant extending into the ultraviolet region. He also correctly suggested that the Lyman alpha nightglow comes not from interplanetary hydrogen but from a geocorona that accompanies the earth in orbit.

He joined the Lockheed Missiles and Space Company in 1955, becoming manager of space physics research. He developed a theory of the geocorona and suggested that accidentally, resonant charge exchange with atomic oxygen could produce a region of protons which he called the protonosphere.

At the AGU meeting 17 years ago he first suggested that the terrestrial magnetic field interacted with the solar wind to form a cavity within which the magnetic field is confined, and he adopted Gold's term 'magnetosphere' to describe it. At that time he also pointed out the importance of field line merging, current file - Stommel

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– 707-19-00

Sept 11 '64

Dear Walter,

Your letter of Aug 28 arrived here today. Thank you very much indeed for your warning about Hewlett Packard.

I will be in Dublin Sept 24-27, in Boston Oct 6-10, but mostly in Paris (where the whole family is with me) until June. Perhaps we will see you in Rome in the Spring?

My best to you + Judy, and may you have a good vacation in Italy!

Henry Stommel

UNIVERSITY OF CALIFORNIA, SAN DIEGO

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POST OFFICE BOX 109
LA JOLLA, CALIFORNIA 92037

11 November 1969

Dr. Henry Stommel
4 Place de Bagatelle
(92) Neuilly, France

Dear Henry:

I will be glad to talk to M. Francois-Xavier Ortoli and his associates on 18 November in just the vein that you suggest. In fact, an offer to buy this instrument came through the mail a week ago.

I would like to use this opportunity to tell you a little about what is going on here. We just finished a paper on our deep-sea tide observations, and I think that some pattern does emerge from the deep pressure recordings. An M_2 amphidrome between here and Hawaii is strongly indicated, and we shall try to nail it down in summer 1970. The current observations are less useful, apparently because of a considerable baroclinic component. One month just is not good enough for that purpose.

In January and February 1970, we are taking the ELTANIN out of Adelaide to make three Antarctic one-month drops.

Judy and I were in Italy in the month of October, and I wish I could have seen you. We talked to various people who are going to be in contact with you, and I would appreciate your advice. We would like to make a one-month drop in February or March 1971, wherever there is the best chance of contributing to a study of bottom water formation. As it looks now, one instrument will have a 1 km. cable, and can carry six Hewlett-Packard temperature gauges anywhere between the bottom and 1 km. above the bottom. Two other instruments will just record temperature one meter above the bottom, currents, and pressure.

Sometime in May I would like to drop three instruments in the tide configuration somewhere in the Adriatic as a contribution to the "Venice problem".

Judy and I send very best wishes.

Sincerely yours,

Walt

Walter H. Munk

WHM:cg

Enclosure: Abstract of Tides Off Shore

November 4, 1969

Henry – The gentlemen came here today. We did our best – Thanks for the alert – Walter

Professor Walter H. Munk, Director
Institute of Geophysics and Planetary Physics
Scripps Institute of Oceanography
La Jolla, California 92037

Dear Walter:

The other day I went to inspect the deep-sea tide gauge of Eyriés - the self-contained one from which the record at 5000 m off Abidjan was obtained successfully. It is in a little factory in Asnieres, just outside of Paris. It seems to be in working condition, but the pressure case in out in the yard with vines growing around it, and spiders making their homes round about. Evidently, as result of a recent change in government, it has not been paid for and the manufacturer quite properly will not release it. It is rather tragic to see this good instrument that might be helping to advance your global deep-sea project set aside like this.

On or about November 18, M. Francois-Xavier Ortoli, the French Minister for Industrial and Scientific Development is going to be visiting Scripps. He will be accompanied by Dr. Edgar L. Piret, the U.S. Embassy Scientific Counsellor in Paris, and by his French counterpart from the French Embassy in Washington, Dr. Maurice Levi. (prm AIGRAIN ALSO)

I am certain that if you would conduct them on a visit of your Institute, show them your own deep-sea instruments, and explain your interest in the Deep-Sea Tide Program and your hopes for French collaboration, it might help them appreciate the significance of this pioneering French device. My understanding is that about $50,000 is involved, and that the instrument is even now being offered for sale to a number of other countries. It would be ashame for France to be deprived of the fruits of use of this instrument in the Atlantic.

Yours truly,

Henry Stommel

Henry Stommel

11:10 - 11:35
EYRIES

copy to Dr. Edgar Piret
Science Counseller
Ambassade des Etats-Unis
2 Ave. Gabriel
Paris 8, France.

4 Place de Bagatelle
(92) Neuilly, FRANCE

4 Place de Bagatelle, Neuilly
(92) FRANCE

November 17, 1969

Dear Walter:

Thank you very much for the letter and ABSTRACT which you sent November 11.

Your Antarctic venture sounds like a very fine idea. It will be very interesting to know what direction the current has right at the bottom. You will enjoy the ELTANIN. I had the starboard-aftermost cabin: it is rather noisy on account of the all-night movie shows that go on next-door in the scientific lounge, and I would advise one cabin further forward. Also Radok has some experience in smuggling sufficient supplies of Australian hospital brandy aboard. Joe Reid can speak for its merits.

Regarding your 1971 experiment: I think that you would do well to persuade Frassetto to plan on taking the BANNOCK well out of the Ligurian Sea, perhaps as far west as 5°E where it appears the most intense vertical mixing occurs (at least it did in 1969). I do not know what to expect in the way of fluctuation of bottom pressure in the convection region, but I think you may find large clouds of water with temperatures varying as much as 0.05°C from the mean deep-water temperatures drifting around near the bottom following the convection time. Swallow found one such mass 60 miles south of the convection region in late March. These masses would show up splendidly, I think, on the top thermometers of your 1 km string reaching up from the bottom. Perhaps we will have a little better idea of the number and general character of these "clouds" following our campaign of February 1970, when I am hoping tomuse Gilmour's abyssal bathythermograph in getting some detailed thermal sections across the convective region. As you remember, Gilmour's ABT uses H-P crystal thermometry too. I agree that we really ought to talk about this matter. There is a possibility that I, or Lacombe and Tchernia, could be of some help in gathering important background material for you in the Feb-March 1971 operation. Maybe what we need is a little symposium somewhere in which Tchernia and Lacombe, myself and Anati, Frassetto, Swallow and others report on the MEDOC '69 cruise scientific results. The Tokyo 1970 meeting seems rather far away from the Mediterranean, but perhaps you will be there, and we might get whoever isgoing there to give a summary talk of the endeavors. In any case the material we are gathering ought to be useful to you, and I will endeavor to get it to you one way or another.

Yours truly,

Henry

Henry Stommel

Stommel

Office Memorandum • WOODS HOLE OCEANOGRAPHIC INSTITUTION

TO : Walter Munk

DATE: 7 Nov 1972

FROM : Allyn Vine

SUBJECT: History and re evaluation

In looking for something else in the archives this showed up, so I made a copy for you and one for Hank.

As early proponets ~~of~~ and dreamers about computors we need to review the bidding for perspective. Certainly computors (either digital or analog) are many times better than we dared dream. And the ultimate goal is nearly as far away as ever. However much has been accomplished and as you said "the need for thinking clearly" is still important.

Isn't it too bad that Von Neuman can't still be here to see ~~the~~ more of his flowers blossom?

Allyn.

NOTES ON THE POSSIBLE USE OF ELECTRONIC COMPUTING MACHINES TO PROBLEMS OF OCEANOGRAPHY

Mr. Henry Stommel of the Woods Hole Oceanographic Institution and Dr. W. H. Munk, of the Scripps Institution of Oceanography, had the opportunity of spending a number of days with Prof. John von Neumann at the Institute for Advanced Study at Princeton, N.J., and to discuss the possible use of modern high-speed electronic computing machines in problems of oceanography.

It appears that certain types of oceanographic problems which were too complicated to be tackled in the past, or which were solved with such simplifying assumptions that the conclusions reached had little resemblance to reality, could be successfully attacked by means of such devices. It is hoped that the following comments may give somewhat of an idea of the possible uses of the machine in future oceanographic research work.

Brief Description of the Machine

The machine is known as EDVAC (electronic discrete variable automatic computing). The development is supported by contracts with the Army Ordnance and the Office of Naval Research. The electronic development is being performed at the RCA Laboratories in Princeton. The machine consists of three main "organs":

a. Control, input and output. -All orders are initiated from an ordinary teletype machine then transferred automatically onto a magnetic tape. The results also appear on magnetic tape and can be put back on teletype. Certain manipulations which are likely to occur frequently will be kept on a library of tapes. The machine is digital so that all numbers and orders must be fed in in the form of discrete numbers instead of continuously as from a graph. It is, however, possible to have graphic records, such as BT slides or tide curves read electronically and fed automatically in digital form into the machine. In a similar manner the results of calculations can be converted from tabular to graphic form by means of special attachments.

b. Arithmetical Organ. -The machine can perform the four basic arithmetic manipulations: addition, subtraction, multiplication and division. It will be shown, however, that this does not impose a stringent limitation upon its use. It can perform these calculations involving up to 20 significant figures at the rate of several thousand per second, whereas the same number of operations by manual methods would take perhaps one man-day. The extraordinary speed derives from the fact that there are no moving mechanical parts. All calculations are performed electronically by means of special vacuum tubes and flip-flop circuits. Inherent in this type of arrangement is the fact that the machine is operated on a binary number system, rather than on the decimal system, as the vacuum tubes can only react in two ways, either emit a charge of not emit a charge. The conversion from a decimal system to a binary system and then back to a decimal system is accomplished automatically by means of relatively simple circuits[1].

1/ These circuits are being developed at the RCA Laboratories by engineers who have the remarkable combination of a thorough knowledge of electronic circuits and the theory of numbers, the latter a branch of mathematics heretofore considered the abstract type of mathematics.

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c. Memory organ. -The most difficult part in the development of the machine concerns an automatic memory device which can store orders, boundary conditions, tables of functions and intermediary results of calculations for later use (whether it is 1/100 of a second later or one hour later). It is likely that the capabilities of the entire computer will be determined chiefly by the extent to which the memory organ can be developed. The memory organ consists of special vacuum tubes with fine grids on their periphery, each of the squares in the grid representing essentially a condenser which may or may not be charged. At any time the periphery presents, therefore, a checkerboard pattern which can be read photo-electrically. Each tube can memorize about 4000 digits. The memory organ represents a bottleneck in the design of the machine and must not be required to store more numbers than it has capacity for at any step in the computation. For that reason computations involving trigonometrical or other types of functions are best accomplished by storing a relatively small number of functions in the memory organ and computing intermediary values by means of high order interpolation methods.

Type of Problems for which the Machine is Suitable

The machine can perform the 4 basic manipulations of algebra, and it will be found that any problem which can be set up for a routine numerical analysis can, in the long run, be reduced to these basic manipulations. Functions, such as Trig functions, Bessel functions, can be reduced to converging series, consisting of these four basic manipulations. The following types of oceanographic problems are particularly suited:

a. Problems involving partial and total differential equations subject to complicated boundary conditions. -

1. Application of general tidal theory to an ocean with irregular bottom contours. This would involve feeding depth soundings into the machine. Such an approach may help to close the gap between theoretical and empirical tidal studies.
2. Problems involving actual distribution of temperature, salinity and other factors in the ocean. This would include numerical solutions for internal wave equation at great speed, studies of heat transfer, turbulence and variations of currents with depth under actual conditions. The large number of BT slides now available makes this type of problem particularly worth of consideration.
3. Problems involving the effect of surface wind where one wishes to make use of actual wind observations rather than analytic function fitting the data approximately.

b. Numerical solutions to high order and high degree differential equations. - Only relatively few differential equations of higher than first degree and higher than second order have been solved. If, nevertheless, a large number of of physical problems have been successfully attacked on the basis of low degree equations, it does not mean that a divine guidance has some kind of preference for differential equations of the first degree; it means rather that simplifying assumptions and approximations have been made in order to reduce problems to a point where they can be readily solved by manual methods. The following two examples illustrate this point:

1. The equations of motion have been solved under the following simplifying assumptions: steady state conditions, neglect of friction, neglect of internal waves, constant Coriolis parameter, horizontal flow and other assumptions.
2. The classic example of wind-driven currents by Ekman is based on a constant coefficient of eddy viscosity. Actually we know that the coefficient changes radically near the sea surface and that it also is affected by the stability of the water column.

If some of these assumptions had not been made, the resulting equations would be of higher order and degree than can be solved by analytic methods. It is possible to obtain numerical results for any equation by means of the computing machine.

What the Machine Cannot Accomplish

The machine does not provide a substitute for thinking. On the contrary, to use it intelligently, the problems to be analyzed must be thought through more carefully than ever. This involves familiarity with the mathematical and physical aspects of the problem as well as a feeling for the oceanography involved. In all instances it will be desirable to study the problem first in such simplified form that analytic solutions can be found so that the importance of the additional terms may be judged one by one. If this is not done, certain analogies and general principles might be lost in a purely numerical solution.

Because of its great speed, the machine is suited for problems involving relatively large numbers of computation. For example, for a fixed type of boundary condition one might try a large number of assumptions regarding the effect of stability on the coefficient of eddy viscosity. Other types of oceanographic computations, such as the computation of dynamic topography, including all corrections for thermometers and other factors, can be handled more efficiently on punch cards.

Conclusions

One might say that the machine helps to put mathematics where it belongs, as a tool rather than an object of investigation. At present a large amount of time is often spent in purely mathematical manipulations which have little bearing upon the physical problem (for example Lamb, Hydrodynamics, consists of approximately 80% of discussion of mathematical transformations). Few of us know how to take a cube root. We are accustomed to use a slide rule, or, for greater accuracy, special tables or tables of logarithms. The machine may help to do the same thing on a higher plane.

EDVAC is being developed as a research instrument. The long range possibilities behind its development are (a) to improve methods of weather forecasting and (b) to explore the possibility of influencing the weather by means of properly spaced and time trigger actions. Since problems in meteorology over the ocean are at least 30% oceanographic, it appears that a simultaneous investigation of certain oceanographic and meteorologic problems would be absolutely essential to the ultimate goal.

The Army has now in operation an ENIAC machine (electronic numerical integrating and computing), which is now in Philadelphia but is to be moved shortly to Aberdeen Proving Grounds in Maryland. EDVAC will not be ready for use for about two years. Nevertheless it is desirable at this time to consider the following two possibilities: (a) "pilot" experiment on ENIAC on a relatively simple problem to learn about its possibilities; (b) long range planning on types of problems which may be handled by EDVAC after it is completed. Since an efficient use of EDVAC would involve modifications of the instruments used to gather the primary data in the ocean, it does not seem too early to start thinking about the possibilities.

December 17, 1946

Walter H. Munk,
Scripps Institution of Oceanography

Henry Stommel,
Woods Hole Oceanographic Institution

UNIVERSITY OF CALIFORNIA, SAN DIEGO

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7 January 1977

Dr. Henry M. Stommel
Massachusetts Institute of Technology
Room 54-1416
Cambridge, Massachusetts 02139

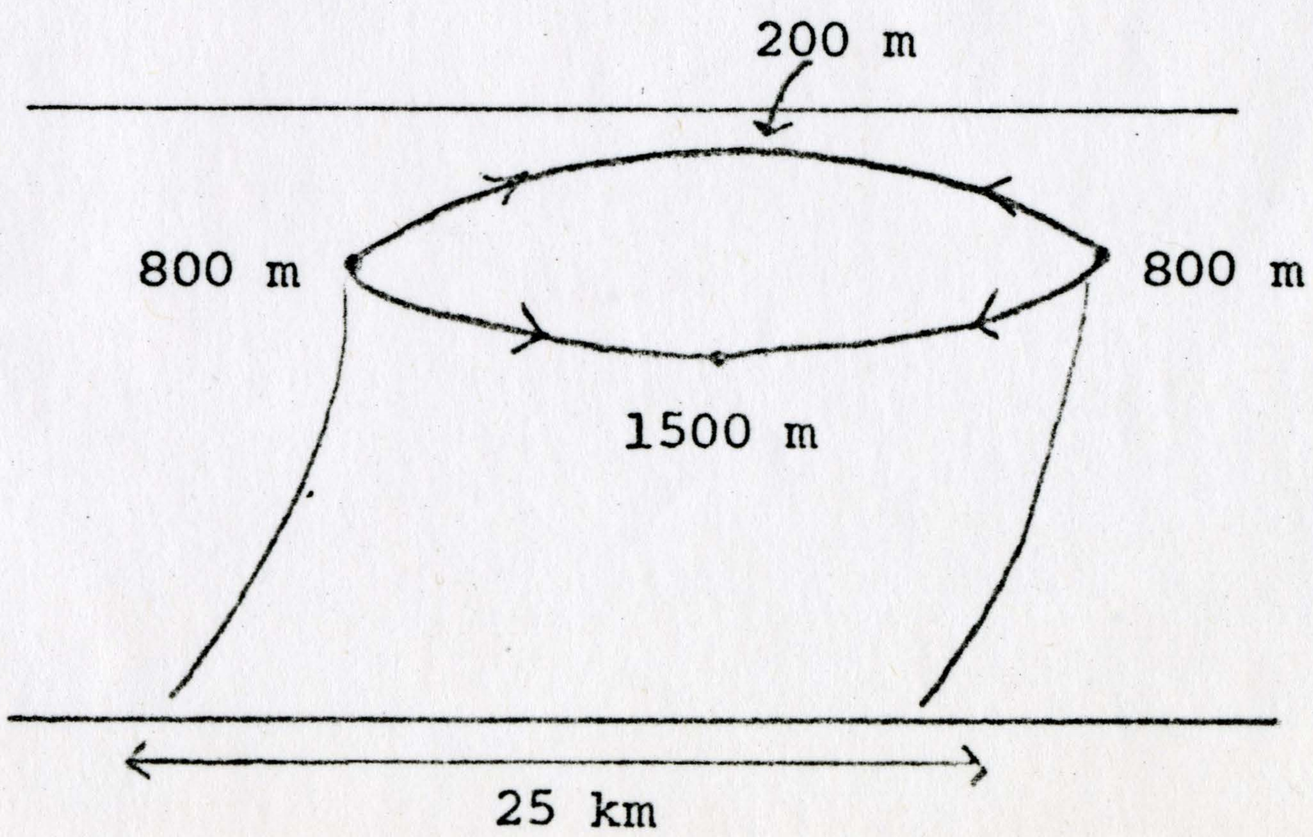
Dear Hank:

I enjoyed the product of your portable typewriter aboard your flight to Hamburg. I am usually too lazy to do anything on such long trips, and I admire your energy for doing so.

I am glad that you found it interesting to think about the acoustic means of monitoring the ocean. If you have the time, you might look at two things that are enclosed: (i) a talk that I gave at the 30-year anniversary of ONR, which has something about the subject near the end (you may wildly disagree with some of the things that I have said); and (ii) Peter Worcester's doctor's thesis. This is quite short and easy reading, and you may find it enjoyable to look at the pretty pictures. Peter has done a wonderful job.

I see no way to use the acoustic method for distinguishing between temperature and salinity. One learns about sound velocity, and this is 90% temperature. Perhaps one can do a good job of combining one's information with TS relations?

The average is both vertical and horizontal, but in the special sense in that it is an integration along sound rays. For our proposed geometry this looks as follows:



The key technical difficulty is to do good position-keeping on the two capsules; and although I think this can be done, it is really a frightful nuisance. There is also the problem of very precise clock time for reciprocal shooting. All this can be avoided by working with differences--upper path minus lower path--~~but~~ by placing the transducers near the depth of the null of the first internal mode, wouldn't such differences give a lot of information, namely a measure of the density gradient (more precisely the temperature gradient) and a measure of the velocity gradient? To obtain more information, one can think of one of two things: (i) either to place a series of transmitters and receivers along each of the two moorings so that one measures the properties along many different paths, with the hope that these can then be disentangled by a clever matrix transformation, or (ii) by using a horizontal configuration with its appropriate variety of ray paths. I think that distances up to 100-150 km. are quite feasible at the 2 kHz frequency, and one likes to use the higher frequencies because this is how one gets the precision.

You write of your concern because of possible security classification. Our work is really totally free, and we would be happy to accommodate a Pakistani graduate student, and for that matter, those from the PRC. I don't see what the Navy has to lose by an acoustic monitoring array; they may in fact gain, because we can provide them with up-to-date charts of acoustic conditions. Now the question of getting information about meso-scale eddies from the dead reckoning of nuclear submarines is, of course, an entirely different matter. I will try to find out whether such logs could be archived for reference in a future and more peaceful world.

Did you know that Cecil Green gave us an endowment to be used for Visiting Green Scholars? The income is quite adequate, and we can use it to make somewhat generous arrangements for people to come here. It has been so many years since you wrote your article in the American Scientist about the Gulf Stream during your previous visit. Are you in the mood to come again?

Sincerely yours,

Walter

Walter H. Munk

WHM:cg
Enclosures

cc: G. O. Williams
P. F. Worcester

De mber 27 ,1976

Dear Walter,

Please excuse this terribly messy letter typ d on a portable aboard the PA to Hamburg this evening. I was very much excited by what you told me about your ideas and hopes for acoustic monitoring of eddy motions over ranges of about 20 _ 50 km. It really would be marvellous if such wond rful averaging procedures could become routine in oceanographic measurement. Can we hope for temp and salinty as well as velocity? Do we get both components? or does this require three installations? And how does it average over depth as well as horizontally?

I am a little concerned over the question of classification - how does this affect the availability of the information to scientists of the world at large. How does it affect the ability of others to participate in such work in a meaningful way? Can Indian graduate students do research on the data in a way that implies full understanding of how the data is obtained, for example? There are many scientists at home and abroad who would like - I am sure - to enter into this acoustical data world, but who are somewhat leery because they anticipate security obstacles at some future point.

For several years I have had the feeling that there must be much information about mesoscale eddies in the navigation logs of nuclear submarines - perhaps a wealth of information on the world-wide geographical distribution of eddies and their characteristics. It is clear that there are many good reasons why this information must be witheld - one hopes that it is at least preserved in some sort of archive for future use when classification can be dropped - and not simply dumped in orderto reuse the tapes. This is an example of a form of possible data source which the open oceanographic community must at present ignore. I mention this business of the submarines only as an extreme example of estrangement between open and secret science. My picture of NSF's post-1980 programs is that they will be in the open area - where all scientists can actively participate - and have the equipment freely available as well as the data - indeed involving a frank and open exchange even with Russians and Chinese. Is it realistic to suppose this can occur in such a sensitive area as the sophisticated acoustic program which you suggest? Please understand that I have no animus against this extremely important and interesting work - only that I am concerned about its feasibility in the field of international "big" science as envisaged for post-IDOE.

These are issues which are probably better brought up personally between friends rather than publically later in a larger fofum. Please let me know how you view this matter.

I did not make a copy of this personal letter.

Hank

Henry Stommel
IfMK Kiel

Jan 27, 1977

Dear Walter,

Thank you for the very fascinating documents on acoustic oceanography that you sent to me. It is very thought provoking, & I look forward to hearing your ideas about scientific problems that we can apply it to.

GREEN
FELLOW

I think Chickie & I will visit Scripps in late June for 1 week to attend Dennis's FINE workshop. We hope to see you + Judy then

Henk

Mail Code A-025
-----92093

1 March 1977

Dr. Henry M. Stommel
Massachusetts Institute of Technology
Room 54-1416
Cambridge, Massachusetts 02139

Dear Hank:

I just received the very brief note on the beta spiral by you and Friedrich Schott, plus your handwritten note, "Walter, I hope you like this."

Frankly, Henry, I am amazed that this very simple procedure was overlooked. I can see no reason why what you did wasn't done 20 years ago, or even earlier. In fact, it should have been done at the time Harald Sverdrup and Bob Reid first worked on the so-called Sverdrup transport.

I found it a little bit difficult going. I think it would help me if I could see a preprint of Leetmaa <u>et al</u>. for the Richardson Memorial Volume.

See you in mid-March. Gustaf Arrhenius sent me his equatorial proposal, which is unique because of its broad involvement of geology, geochemistry, marine meteorology, etc., etc. Perhaps this particular proposal would have to be reviewed by the combined workshops rather than the Physical Oceanography Workshop alone.

Sincerely yours,

Walter H. Munk

WHM:cg

Walter, for your info.
Hen

HENRY STOMMEL
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FILE

December 5, 1977

Dr. Philip Handler, President
National Academy of Sciences
2101 Constitution Ave.
Washington, D.C. 20418

Dear President Handler:

I am very much concerned about your announcement of October past as Chairman of the National Research Council of a somewhat more stringent internal security policy concerning "potential sources of bias", in particular paragraph 4, which requires that members of committees etc. maintain at the secretary's office an up-to-date file on personal views or conclusions or activities which might appear to others to compromise their independence of judgement. This ruling seems to be (i) dangerously vague and inconsistent, (ii) personally offensive, and (iii) probably ineffective as a defense under determined attack from outside.

Our universities and international scientific bodies do not require maintaining such a vaguely defined file. Should we in good conscience submit in writing the names of foreign countries with which we have special ties? or report honors from them as being possibly corrupting? If we have some strong religious or moral beliefs, must we record them with our secretary? Unless these things can be more clearly defined then we must leave the decision as to whether they are important to the individual, which of course is exactly what most organisations do in the first place, and the Academy itself preferred in a day when it placed a higher confidence in the honor of those associated with it. I believe that we, who work for you with no other compensation than gratification of our impulse to be of national service - mixed with lust for influence in policy-making - are better able to judge our ethical conflicts than any watch-dogs within the Academy. With paragraph 4, you are intruding far too deeply into matters of private conscience.

If I were to begin to fear what others might think, and conduct my affairs, formulate my opinions, and arrive at my conclusions - always with a paramount concern of how they might appear to others - then I think my independence of judgement would be really jeopardised. In the Peoples' Republic of China, I read, citizens have regular sessions of "self-criticism" in public. This is a well-known mechanism of repression and intimidation. If we apply your test of "how will it look" to yourself, are you not concerned that it will appear that you are toying with the same deadly machine?

Perhaps it is an excess of Yankee stubborness and independence to balk at subscribing to such a file. Presidents Bronk and Seitz did not seem to feel the need of it, however. And I remember that President Sproul, in 1949, of the University of California expressed surprise that I objected to signing his Regents' famous Oath. We used to have a somewhat similar oath for teachers here in Massachusetts which was finally struck down by the courts. There are differences of course, but one instinctively feels that there is some underlying similarity: a formal attempt to document grounds for possible malfeasance - a basic mistrust of the good judgement and ethical soundness of the people one turns to for advice. I cannot escape the sense of personal insult.

One can understand your concern to shelter the Academy and the committees of the Research Council from exposure in the press of glaring improprieties. Certainly Washington is teeming with investigative reporters who would gleefully attempt to fling the Academy down from Olympus. Leaving aside the question as to whether there is a place for Olympians in a Republic, it would appear that the Academy, having descended to the public arena in making policy on so many present-day issues, is bound to get a bit muddied anyway - and the file in question may not be of much help. Perhaps one must learn to swim in mud.

I have, accordingly, resigned from the U.S. GARP Committee and its MONEX Panel. If you believe that it is also inappropriate for me to remain one of the Nominating Committee for the Ocean Science Board, I will gladly resign from that too.

Should an occasion arise in the future when my advice or experience might be useful to the Council - under circumstances not requiring compliance with the file - I shall be most pleased to be of any assistance that I can. With a deep appreciation of the service to science and the nation which you and the Council are trying sincerely to make, I remain,

Yours truly,

Henry Stommel

Henry Stommel