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INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS, A-025
SCRIPPS INSTITUTION OF OCEANOGRAPHY

LA JOLLA, CALIFORNIA 92093

January 6, 1987

Dr. Anton L. Inderbitzen
Executive Secretary
NSB Committee on NSF's
Role in Polar Regions
National Science Foundation
Washington, DC 20550

Dear Dr. Inderbitzen:

Henry Stommel's letter to the Committee is a persuasive statement for undirected science. It is no more directed to science in Polar Regions than any other kind of science. For myself, I have no problem with Henry's position.

I am impressed with how often rapid progress follows the development of a new tool for observations. We were brought up to think that it should be the other way around; first the ideas and then the tools. For Polar work the required tools include the platforms. We have been planning for an acoustic component to a Greenland Sea experiment, and I was amazed how incomplete the record is for the distribution of temperature and salinity. First of all, the changes are rather small and one needs more than the usual precision. Secondly, the sampling in time is so sparse that no one has resolved the time history of the seasonal changes.

I wish to make a plea for support towards the tools of exploring the Polar oceans. The proper use of these tools and the analysis of the observations can be left to the initiative of the individual investigators provided there is support for good ideas.

Sincerely yours,

Walter Munk

Walter H. Munk

WHM/jrd

NATIONAL SCIENCE FOUNDATION
WASHINGTON, D.C. 20550

2 January 1987

Dr. Walter Munk
Scripps Institution of Oceanography
A-025
University of California at San Diego
La Jolla, California 92093

Dear Dr. Munk:

Enclosed is a letter to me from Dr. Henry Stommel on the subject we wanted you to address for the National Science Board Committee on NSF's Role in Polar Regions. Dr. Rita Colwell, Committee Chair, would appreciate your comments and views on the letter for the Committee. An early response would be greatly appreciated.

Thank you for your help.

Sincerely,

Anton L. Inderbitzen

Anton L. Inderbitzen
Executive Secretary
NSB Committee on NSF's
Role in Polar Regions

Henry Stommel's letter to the Committee is a persuasive statement for undirected science. It is no more directed to science in polar regions than any other kind of science. For myself I have no problem with Henry's position.

¶ I am impressed with how often rapid progress <u>follows</u> the development of a new tool for observations. We were brought up to think that it should be the other way around: first the ideas and then the tools. For polar work the required tools

766 Palmer Ave.
Falmouth, Mass. 02540
November 24, 1986

Dr. Anton Inderbitzen
Division of Polar Programs
National Science Foundation
1800 G St. NW
Washington, D.C., 20550

Dear Tony,

Thank you for your invitation to appear before the committee, but I think that my current knowledge of the status of polar research is insufficient to make any useful comment. I do remember in former years however that the Office of Polar Programs was an especially helpful and active program office. Perhaps it is particularly close to scientific needs because it is one of the programs in NSF that actually does logistical things, like arranging for shipments etc., for the scientific workers whom it sponsors. Your Division shares actively in the nuts and bolts of the scientific work, and I think this is reflected in it's exceptionally wholesome and realistic relationship with the scientific community that it serves.

I do have some comments on the subject of trying to impose priorities and goals upon the scientific community at large. My general impression is that our national scientific effort works very well because it is largely undirected. From the outside observer's point of view we scientists certainly must appear to be running in all directions, like the workers in a disturbed beehive. Nobody is apparently telling us what to do, no overall plan is discernible. Yet slowly we make amazing progress. New ideas and techniques spring from the most unsuspected quarters. Our national science program is productive, I think, because it is undirected from above.

The direction that actually operates so successfully comes mostly from within the individual investigators ourselves. We are motivated by an inner compulsion to make discoveries, and gain recognition by bright new ideas. And we know that in finding ways to tease new knowledge from nature we must depend upon our own individual judgements of what is the best thing to try next. The scientific reward structure recognizes the basic individual nature of new thought. It does not recognize priorities set by governing committees. Who ever got a Nobel Prize for following the guidelines of a Government Commission?

When you come to think of it, these same principles of minimum interference in human activity are fundamental to our form of government. The Congress does not tell us what the purpose of our lives is, it does not prescribe a path for the pursuit of happiness. It simply serves as a support structure within which we all have the opportunity to use our abilities in the most appropriate way that we individually decide to. The reward structure of the market society decides the rest. It is dangerous to tamper with the reward structure, as recent changes in Communist China so clearly reveal.

For those who have the responsibility of maintaining the support structure it might be best for them to think of themselves as "repair men" - people who keep a good system going. As tempting as it may be, they ought to eschew the impulse of trying to set the pace, or telling scientists what is worth studying next. Statements about priorities, like " The Global Climate Initiative", just tend to befog things. Perhaps they help to raise budget levels allowed by the Congress, but I think that a clear avowal of the fact that scientific progress actually grows most luxuriantly from what appears to be chaos in the laboratories would be more helpful and truer. It isn't really chaos at all: just a milling crowd of scientists each trying to do the very best he can to choose and solve the best problems that he - and nobody else - can think of.

Yours sincerely,

Henry Stommel

Henry Stommel

Arctic Ocean Interagency Group Meeting

November 14, 1986
Room 453
Joseph Henry Building
21st & Pennsylvania Avenue NW
Washington, D.C.

9:00am Opening Remarks - R. Goody

9:15am Federal Agency Programs
- National Science Foundation - P. Wilkniss
- Office of Naval Research - L. Johnson
- National Oceanic and Atmospheric Administration - N. Ostenso
- National Aeronautics and Space Administration - S. Wilson

Noon Working Lunch

1:00pm Discussion of the Arctic Ocean Sciences Board and the Greenland Sea Project

2:00pm Discussion

3:00 Adjournment

NATIONAL RESEARCH COUNCIL
COMMISSION ON PHYSICAL SCIENCES, MATHEMATICS, AND RESOURCES
2101 Constitution Avenue Washington, D.C. 20418

OCEAN STUDIES BOARD

OFFICE LOCATION:
JOSEPH HENRY BUILDING
21ST STREET AND
PENNSYLVANIA AVENUE, N.W.
(202) 334-2714

24 October 1986

MEMORANDUM

TO: Dr. G. Leonard Johnson
Dr. Ned Ostenso
Dr. Stanley Wilson
Dr. Peter Wilkness
Dr. Jerry Brown

FROM: Mary Hope Katsouros MHK

SUBJECT: Arctic Ocean Interagency Group

The Arctic Ocean Interagency Group meeting has now been scheduled for Friday, November 14, 1986 in Room 453 of the Joseph Henry Building, 21st and Pennsylvania Avenue, N.W., Washington DC. The meeting will convene at 9:00 am and adjourn at 3:00 pm. There will be a working lunch to enable as much discussion and exchange of views as possible.

The purpose of the meeting is to review agency programs and to discuss the activities of the Arctic Ocean Sciences Board and, especially, the Greenland Sea Project. A draft agenda is enclosed.

If you have any questions, please do not hesitate to call me. If you are planning on bringing members of your staff, please call me or Judy Marshall with their names. We are looking forward to seeing you on November 14th.

cc: R. Goody

bcc. W. Munk ✓

1987 Research Briefing

Proposed Topic

AIR-SEA-ICE INTERACTION

BACKGROUND: Important processes forming global deep water masses at high latitudes have been identified. They have implications for global climate variability on interannual and decadeal (or longer) time scales. The dominant process is deep convection induced by heat loss from the surface and ice formation. (Also important in this context is the associated upwelling of nutrient-rich water and high biological productivity of sub-polar waters.) The recent dramatic increase in computing power will soon make it possible to integrate sophisticated interactive models of the air-sea-ice system. These models will need to be tested with regard to their parameterization schemes, and their capability to reproduce observed variations. Our present ability to model the air-sea-ice system exceeds our ability to validate the model calculations. Thus, the greatest need is for carefully designed multiyear observing programs for model validation.

THE OPPORTUNITY: The technical means of making these observations are now available--mainly satellite remote sensing and in situ sensors, both drifting and moored for ocean and ice observations. New satellite borne imaging systems of unique value in the polar regions are the Special Sensor Microwave Imager (SSM/I) scheduled for launch on DMSP in 1987, and the Synthetic Aperture Radar (SAR) on the European satellite ERS-I, scheduled for launch in 1989 or 1990. These observing systems, along with the currently operational visual and infrared imagers, will assure the needed long-term monitoring of the polar regions. It will be of particular importance to assure in advance the efficient processing and sitributions of data from the new satellites. Because of the research opportunities presented by satellites, a briefing on such a topic appears to be timely for 1987.

Significant research topics are the Marginal Ice Zone Experiment (MIZEX) and the Greenland Sea Project (GSP). MIZEX has two summer field experiments in 1984 and 1985 and will have one more in spring 1987 and, possibly an additional one later. MIZEX is largely ONR funded aimed at small and mesoscale processes in the marginal ice zone of the Greenland and Bering Seas.

The Greenland Sea Project is a proposed five-year program of observations and modeling of the atmosphere, the ice, and the ocean of the Greenland Sea, and their interactions among themselves and with the marine biology of the region. The results will be used to understand the role of the Greenland Sea in the oceanic contribution to climate-related processes and to tie seasonal and interannual sea ice variations to large-scale dynamics of the atmosphere and ocean. This is a program with international participation and would be a unique opportunity for the U.S. scientific community.

CANDIDATES FOR PANEL: Richard Goody, Norbert Untersteiner, D. James Baker, Gunther E. Weller, and Seelye Martin.

Staff: Mary Hope Katsouros.

1987 Research Briefing

Proposed Topic

ATMOSPHERE-OCEAN INTERACTION ON TIME SCALES OF 10^3-10^5 YEARS

BACKGROUND: During the past half-million years, the Earth has experienced major changes in the global environment as climate oscillated between conditions of an ice age and conditions similar to those which obtain today. Studies of geological records on the sea floor and in ice cores have firmly established several things about these oscillations. First, they are caused by relatively small changes in solar radiation received by the Earth--changes that are in turn driven by variations in the geometry of the Earth's orbit on time scales of 10,000 to 100,000 years. Second, they involve global changes in rainfall and temperature, in the distribution of animals and plants, in the ventilation rates of the ocean, in the relative sizes of the global reservoir of reduced and oxidized carbon, and in the patterns of oceanic and atmospheric concentration of CO_2. Owing to the large size of the oceanic CO_2 reservoir, we know that a crucial part of the response mechanism must lie in the ocean.

THE OPPORTUNITY: In effect, nature has conducted a grand experiment to help us understand how the climate system really works on these time scales. This is a once-in-a-century opportunity to identify major mechanisms of atmosphere-ocean interaction, and to determine how sensitive the system is to changes in the radiation budget. This opportunity has only recently been made available by advances in the study of ice and marine-sediment cores; by improvements in geological chronology; and by our growing ability to model atmospheric and oceanic mechanisms. We therefore now have the chance (1) to discover the mechanisms by which changes in CO_2 and climate have occurred long before the industrial era; and (2) to find out how well our computer models can account for the changes observed on geological time scales. When our models pass this test, they can be used with more confidence to forecast the results of the climate experiment now being undertaken inadvertently by human actions on time scales of decades.

CANDIDATES FOR PANEL: John Imbrie, Roger Revelle, D. James Baker, Richard Gammon, and James McCarthy.

Staff: Mary Hope Katsouros.

1987 Research Briefing

Proposed Topic

HYDROTHERMAL PROCESSES AT PLATE BOUNDARIES

BACKGROUND: Active hot springs were first discovered on spreading centers in 1977. This followed about a decade of speculation and relatively unfocused exploration. The first discovery, on the Galapagos Spreading Center in 86 W, was quickly followed by finds on the EPR at 21 N and between 11 and 13 N (1979 and 1982 respectively) and on the Juan de Fuca and Explorer Ridges in the northeast Pacific (1983). Most recently hot springs have been found on the slow-spreading MAR at 22 and 26 N (June 1986). In addition hydrothermal activity has been found in the Gulf of California (Guaymas Basin, 1982) where the ridge axis is buried under about 500 meters of terrigenous/biogenic sediments. Fresh lead-, zinc-, and silver-rich sulphides have been recovered from the buried section of the Gorda Ridge (Esconaba Trough) off northern California.

Through its funding of the regional exploration and of the ALVIN, the NSF is the overwhelmingly dominant player in this area. After taking an early "nibble" the ONR has not been involved. NOAA and USGS, as part of their EEZ responsibilities, have used ALVIN and, this summer, SEACLIFF (U.S. Navy) in the northeast Pacific. At present the foreign "competition" has been limited to the French work on the EPR at 11-13 N using CYANA. Soon their 6-km boat, NAUTILE, is expected to be an important player especially on the deeper parts of the MAR and at back-arc spreading centers and fracture zones beyond the depth range of ALVIN (4.1 km). At present the ALVIN/AII combination is unchallenged in terms of payload, technical sophistication, experience, logistics, and so on. This will change as the NAUTILE becomes fully operational and as the Japanese progress in their aggressive plans for a 6-km vessel (they already have a 2-km vessel operational).

THE OPPORTUNITY: Why are hot springs important? The most obvious reason is their relation to ore deposits. Massive sulfide ore bodies, such as sustain the bulk of the industry in Canada, Australia, and to a lesser extent, the Soviet Union, are formed in submarine settings at oceanic spreading centers. However, the deposits on land are "fossil" with ages ranging from hundreds to thousands of millions of years. The high temperature systems ($\tilde{}$350 C) observed on the EPR and MAR are associated with ore bodies formed by precipitation from the issuing hydrothermal fluids. This process can be observed in detail from a submarine. Both fluids and precipitates can be sampled. Through thermodynamic modeling the mechanisms responsible for the solution compositions can be identified and the precipitation processes studied. Since, in a few cases, the ore bodies are on the same scale as exploited bodies on land (greater than 2-3 million tons sulfide ore) the analogy between active and fossil systems is very good. Combining the chemical interactions and geo-dynamics gives a very clear insight into the way these bodies are formed, an insight that is beginning to aid in exploration. Mining of the active ore bodies depends on the economic trade-off between the difficulties of operating at 2.5 km water depth and the advantages of the "mining camp," which is moveable and can be amortized over many deposits. This boils down to the frequency of occurrence of deposits of some minimal size and grade. Numerical values are, of course, closely held.

The second important facet of hot spring studies concerns the effects that seawater-basalt reactions at high temperature have on the reactants. The effects on the oceanic crust are known to be profound, drastically altering the composition of the material reinjected at subduction zones as compared to pristine tholeiite. There are large effects on the composition of the seawater. Since a volume of water circulates through the 350 C isotherm at ridge axes approximately every 10 million years these hydrothermal systems act as a geochemical "flywheel" and smooth out compositional excursions caused by changing inputs from the continents.

The third aspect, and certainly the best known, is the astonishing new ecosystems that these hydrothermal areas represent. These are entirely chemosynthetic with the large organisms being "powered" by symbiotic bacteria in enormous abundances. It is turning out that this arrangement is much more common than had been thought. Hydrogen sulfide is the key ingredient, not heat. Thus in situations where both hydrogen sulfide and dissolved oxygen are available simultaneously --natural gas seeps, the oxic/anoxic interface in fjords, the Los Angeles sewage outfall--similar organisms are being found.

Suitable niches are widely available in nature along with the physiological adaptations required to take care of the associated toxic gases and metals.

This is a good time for review in that we are facing something of a "breathing space" with no major expeditions in prospect for 1987. It is generally agreed that hydrothermal activity is inextricably part of the seafloor spreading process and hence is a phenomenon global in scope. There is now time to reflect on where to go from here. There are several obvious directions (I will speak only for geochemistry). We need to extend the kinds of environments studied especially to greater depth. This will get into higher P and hence T regimes. Back-arc basins and fracture zones are obvious targets. Sediment covered ridge axes are very important. While the primary heat source is still the mantle-derived magma, the hot fluids also react with the sediment fill. Thus a wide variety of ore bodies can be formed, typically the Pb-Zn-Ag-Au association, with the metals being derived from the sediments. So far two have been sampled, the Red Sea Brines (marine evaporites) and the Guaymas Basin (see above). The Gorda Ridge is an immediate target. The Okinawa Trough (Yangtze and Yellow River sediments) is another. It is proving impossible to drill zero-age crust on the open ocean ridges. Hence we cannot "see into" the reaction zone directly but must make (sometimes extended) inferences based on solution compositions, sections of oceanic crust exposed on land, etc. Sediment covered ridge axes can probably be drilled successfully to the highest temperature the technology permits. Once active springs are found on the southern Gorda then a drilling program there will become an exciting prospect especially since the ridge is within the U.S. EEZ.

There has been no formal review of the hydrothermal work that is up to date and comprehensive. The field is fragmented and moving so fast that almost every diving expedition brings back something new and unexpected.

CANDIDATES FOR PANEL: J. Edmond. Staff: Mary Hope Katsouros.

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LA JOLLA, CALIFORNIA 92093

9 July 1987

IN CONFIDENCE

Dr. Robert C. Beardsley, Chairman
Department of Physical Oceanography
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543

Dear Bob:

Here is the letter for Hank Stommel. If you can think of some way to improve the letter, I would be very glad to make the necessary changes. If you think it is all right, I would suggest you forward it to the Committee.

Let's hope this one flies!

Sincerely yours,

Walt

Walter H. Munk

WHM/jrd

9 July 1987

Committee on the National Medal of Science

Dear Sirs:

I am very pleased to write you on behalf of Henry Stommel; Stommel is the most influential oceanographer of our time.

I will single out just a few accomplishments. The first thing that comes to mind is Stommel's lifelong preoccupation with the Gulf Stream and with related problems of the North Atlantic circulation, both deep and shallow. Stommel, more than anyone else, is responsible for the present flavor of this subject, as *the* authorized biographer of the Gulf Stream.

The oceans are stratified by temperature and salinity, and this two-component system behaves in strange and unexpected ways. Stommel's invention of the perpetual salt fountain, to which he referred as 'an oceanographic curiosity', has since developed into a discipline all its own called 'double-diffusion'. Somewhat related are Stommel's original ideas about the control of salinity in deep estuaries by hydraulic transitions over the sill, and this too has led to modern developments with application to estuarine pollution.

The formation of water masses has been a classic problem in oceanography since the days of Nansen and Helland-Hansen. Stommel was the first person to organize an expedition to make some direct measurements associated with the formation of a water mass. He played the leading role in a field experiment, MEDOC, which documented the overturn of Mediterranean water in winter.

Finally I want to refer to Stommel's collaboration with Gordon Riley and Dean Bumpus in 1949, which led to the first attempt of a quantitative ecology in the marine environment. The present high level of activity in population dynamics is an outgrowth of this pioneering effort.

Stommel has spent many months at sea, measuring the Gulf Stream, the Somali and Kuroshio currents, and the equatorial undercurrents. He played the leading role is a series of field experiments: MODE and POLYMODE (mesoscale dynamics), GEOSECS (geochemical tracers), and INDEX (Indian Ocean Dynamic Experiment). These experiments are now household words among oceanographers. Stommel also does laboratory experiments; some of the most insightful of these are of the sealing wax and string type. He and L. F. Richardson were able to derive important deductions from observing pairs of parsnips thrown into a Scottish lake.

Stommel pioneered some of the modern observing techniques. He took infrared measurements from an airplane in 1953; he was one of the first to discuss the applications of surface drifters and SOFAR floats. He used abandoned submarine cables for measuring and interpreting the electric potential between widely spaced points; he laid an underwater cable off Bermuda to obtain long time series of subsurface temperature.

This will suffice as indication of Stommel's broad interests. If there is a connecting and continuing theme, I think it is the concept of conservation of potential vorticity. R. W. Stewart, in a recent review, writes perceptively: '...Stommel was the first person working in geophysical fluid dynamics to really [understand] vorticity well enough that it became a reliable part of his intuition.'

If this is the theme, what is the orchestration? It is a testing of these (and other) concepts by the simplest and most direct means available. Stommel has said it best himself: 'Too much of the theory of oceanography has depended upon purely hypothetical physical processes. Many of the hypotheses suggested have a peculiar dreamlike quality, and it behooves us to submit them to especial scrutiny and to test them by observations.'

Stommel's work is characterized by simplicity, insight, and by his love of the sea. He has brought great honor to the U. S. ocean community, and this has been recognized by his election to the National Academy of Sciences; The Royal Society, London; the Soviet Academy of Sciences and the Academie des Sciences de Paris.

Sincerely yours,

Walter Munk

Walter H. Munk

WHM/jrd

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Department of Physical Oceanography
Robert C. Beardsley, Chairman

30 June 1987

In Confidence

Dr. Walter H. Munk
Mail Stop A-025
Institute of Geophysics and Planetary
Physics
La Jolla, California 92093

Dear Walter:

I want to thank you for helping in this endeavor. John Knauss first suggested to me that we nominate Henry for this award, and I asked Carl Wunsch, Joe Pedlosky and Jim Luyten to help write the various sections of the nomination and pick the outside reviewers. So, please don't hesitate to contact any of us if you have questions.

Enclosed is a copy of the nomination plus a complete CV and list of papers for your reference. Your letter is due in Washington before October 1, 1987. If you don't mind, could you please send me a copy of your letter. I would like to add it to the file here and will keep it confidential.

Thanks again for your help. We've tried to keep this a secret from Henry, so hopefully he will receive the award and it will be a surprise.

Sincerely,

Bob

Robert C. Beardsley

RCB:kmb
Encl.

Form Approved
OMB No. 3145-0035
Exp. Date: 11/30/87

Privileged Information

NOMINATION FOR NATIONAL MEDAL OF SCIENCE

Nominee

Name: Henry M. Stommel

SS#: [redacted]

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Telephone: (617) 548-1400, ext. 2487

Biographical Data

1. Year and place of birth: Wilmington, Delaware, in 1920
Please check if naturalized citizen ☐

2. Education

B.S., Yale University, 1942
M.A. (Hon.), Harvard University, 1961
Ph.D. (Hon.), Goteborg Universitet, 1964
Ph.D. (Hon.), Yale University, 1970
Ph.D. (Hon.), University of Chicago, 1970

Major Discipline(s):
Astronomy

3. Positions Held

Instructor in Mathematics and Astronomy, 1942-44, Yale University
Research Associate, 1944-60; Physical Oceanographer (non-resident), 1960-78; Senior Scientist, 1978 to present, Woods Hole Oceanographic Institution
Professor of Oceanography, 1960-63, Harvard University
Professor of Oceanography, 1963-78, Massachusetts Institute of Technology
Guest Lecturer, 1969-70, Laboratoire d-Oceanographie Physique du Museum National d'Histoire Naturelle, Paris, France

4. Honors

Phi Beta Kappa, Sigma Xi
Member, American Academy of Arts and Sciences, 1959
Member, National Academy of Sciences, 1961
Sverdrup Medalist, American Meteorological Society, 1964
Albatross Award, 1966
Fellow, American Geophysical Union, 1972
Henry Bryant Bigelow Award, 1974

(continued "Additional Comments")

NOTE: If more space is required for any category, please continue under "Additional Comments," page 4.

Nominee: Henry M. Stommel

Narrative Statement Describing Nominee's Qualifications for a Medal

Henry Stommel is the creator of modern dynamical oceanography. Through his own sometimes astonishing insights, and 40 years of unceasing collaboration on almost all aspects of physical oceanography, he set the context in which the entire subject of physical oceanography has progressed from a purely descriptive taxonomy to a quantitative branch of physics.

His science is best characterized by an uncanny ability to recognize a question worth asking and then to answer it in the most deceptively simply physical terms with a minimum of mathematics. The characteristics are evident in his most famous paper, "The westward intensification of wind-driven ocean currents", published in 1948. He recognized that the existence of the very strong westward intensification of the current systems of the world oceans (called, in the North Atlantic, "the Gulf Stream") was a peculiar phenomenon which should be explained as a consequence of the equations of fluid mechanics. Stommel then showed that the Gulf Stream could be understood from simple considerations of angular momentum conservation in a fluid (vorticity conservation).

This paper bears the earmarks of a true scientific classic: with hindsight it is all perfectly obvious. Only by examining the literature of the time does one perceive that before Stommel, no one even recognized the existence of the Gulf Stream as a dynamical entity requiring explanation. The paper spawned an enormous literature, in which the original model was elaborated to include non-linearity, stratification, topography, time dependence, etc. Much of this elaboration was the result of Stommel's own work or with collaborators. But the 1948 paper remains the clear cornerstone on which <u>all</u> theories of the ocean circulation, including the most elaborate of today's numerical models, are built.

Stommel went on from the 1948 paper to produce a remarkable set of ideas about how the ocean "works". They include (with A. Arons) the only extant (30 years later) notions of the global scale abyssal circulation, the theory of the thermocline, the central demonstration of the extremely long response time of the circulation to changing forces, the invention of what has become the entire sub-field of double-diffusive convection, the demonstration of the strange properties of bottom water formation in the global ocean, the beta-spiral method for absolute velocity determination... Some of his most significant work has been published very recently, at a time when most scientists are contemplating retirement. Frustrated by the difficulties encountered by the non-linearities of the partial differential equations governing the older thermocline theory, Stommel and collaborators recently re-formulated the problem in a new form, found a new range of possible solutions, and have spawned a renaissance of the theory of the large scale circulation, 40 years after Stommel's opening up of the field. He continues to strike out in completely new and innovative directions. His recent work (with N. Hogg) on baroclinic point vortices, the "hetons", has illuminated from a fresh viewpoint the interaction dynamics of strong oceanic eddies and the important process of baroclinic instability in the oceans and the atmosphere.

In understanding Stommel's contributions, it is important to recognize that a large part of his career has been spent in making observations first-hand, on ships. Many of his most important contributions have come from his drive to work at sea. To

(continued "Additional Comments")

Nominee: Henry M. Stommel

List of Pertinent Contributions and/or Publications (limit to 20)

Stommel, H., 1948. The westward intensification of wind-driven ocean currents. Transactions, American Geophysical Union, 29, 202-206.

Stommel, H., 1951. Entrainment of air into a cumulus cloud. II. Journal of Meteorology, 8, 127-129.

Stommel, H. and H. G. Farmer, 1953. Control of salinity in an estuary by a transition. Journal of Marine Research, 12, 12-20.

Stommel, H., 1954. Circulation in the North Atlantic Ocean. Nature, 173, 886-888.

Veronis, G. and H. Stommel, 1956. The action of variable wind stresses on a stratified ocean. Journal of Marine Research, 15(1), 43-75.

Stommel, H., A. B. Arons and D. Blanchard, 1956. An oceanographic curiosity: the perpetual salt fountain. Letter to the Editors, Deep-Sea Research, 3, 152-153.

Stommel, H., A. B. Arons and A. J. Faller, 1958. Some examples of stationary planetary flow patterns in bounded basins. Tellus, 10, 179-187.

Robinson, A. and H. Stommel, 1959. The oceanic thermocline and the associated thermohaline circulation. Tellus, 11, 295-308.

Stommel, H. and A. B. Arons, 1960. On the abyssal circulation of the world ocean. II. An idealized model of the circulation pattern and amplitude in oceanic basins. Deep-Sea Research, 6, 217-233.

Stommel, H., 1962. On the smallness of sinking regions in the ocean. Proceedings of the National Academy of Sciences, U.S.A., 48, 766-772.

Anati, D. and H. Stommel, 1970. The initial phase of deep water formation in the northwest Mediterranean during MEDOC '69 on the basis of observations made by Atlantis II, January 25-February 12, 1969. Cahiers Oceanographique, 22, 343-351 + 24 charts.

Stommel, H., 1972. Deep winter-time convection in the western Mediterranean Sea. In: Studies in Physical Oceanography, a tribute to Georg Wüst on his 80th birthday, Arnold L. Gordon, editor, Gordon and Breach, Vol. 2, 207-218.

Leetmaa, A., P. Niiler and H. Stommel, 1977. Does the Sverdrup relation account for the Mid-Atlantic circulation? Journal of Marine Research (Richardson volume), 35, 1-10.

Stommel, H. and F. Schott, 1977. The beta-spiral and the determination of absolute velocity field from hydrographic station data. Deep-Sea Research, 24, 325-329.

Stommel, H., 1979. Determination of water mass properties of water pumped down from the Ekman layer to the geostrophic flow below. Proceedings of the National Academy of Sciences, U.S.A., 76, 3051-3055.

Luyten, J. R., J. Pedlosky, and H. Stommel, 1983. The ventilated thermocline. Journal of Physical Oceanography, 13, 292-309.

Bryden, H. L., and H. M. Stommel, 1984. Limiting processes that determine basic features of the circulation in the Mediterranean Sea. Oceanologica Acta, 7, 289-296.

Hogg, N. G., and H. M. Stommel, 1985. The Heton, an elementary interaction between discrete baroclinic geostrophic vortices, and its implications concerning eddy heat-flow. Proceedings of the Royal Society of London, A, 397, 1-20.

Luyten, J., and H. Stommel, 1986. A beta-control of buoyancy-driven geostrophic flows. Tellus, 38A, 88-91.

Luyten, J. , and H. Stommel, 1986. Gyres driven by combined wind and buoyancy flux. Journal of Physical Oceanography, 16, 1551-1560.

Nominee: Henry M. Stommel

Proposed Citation (limit to 1 or 2 sentences)

Henry Stommel is awarded the National Medal of Science for his original, penetrating and fundamental contributions to the physics of ocean circulation.

Additional Comments

4. Honors (continued)
   Foreign Member, Soviet Academy of Sciences, 1976
   Maurice Ewing Award, 1977
   Rosenstiel Award, American Association for the Advancement of Science, 1978
   Agassiz Medal, National Academy of Sciences, 1979
   Huntsman Award, Bedford Institute of Oceanography, 1980
   Bowie Award, American Geophysical Union, 1982
   Grand Prix d'Oceanographie de Monaco, 1982
   Membre d'Honneur, Societe de Geographie, Paris, 1983
   Crafoord Prize, Royal Swedish Academy of Sciences, 1983
   Foreign Member; The Royal Society, London, 1983
   Foreign Associate, Academie des Sciences de Paris, 1984
   Albert Defant Medal, German Meteorological Society, 1986

(continued from page 2)

name only a few, he was midwife to the technology of the SOFAR float; his experience with long trans-Pacific hydrographic sections gave rise to the global geochemical tracer program called GEOSECS; he founded the so-called Panulirus station at Bermuda, which 33 years later is the center piece of knowledge of oceanic time series behavior and he provided a focus for the major programs studying monsoon response of the western Indian Ocean.

He is also a raconteur, entertainer and popularizer. In this latter capacity, he is the author (at last count, more are coming) of three non-technical books, including one with his wife Elizabeth on the famous disastrous summer of 1816. Although not a brilliant lecturer, he has been a truly exceptional teacher, stimulus and goad to several generations of formal and informal students and colleagues.

Nominee: Henry M. Stommel

References (limited to 3 persons familiar with technical aspects and not from nominee's home institution)*

Name: Dr. Walter H. Munk

Address: Mail Stop A-025
Institute of Geophysics and Planetary Physics
La Jolla, California 92093

Telephone: (619) 534-2877

Name: Dr. Francis P. Bretherton

Address: National Center for Atmospheric Research
P.O. Box 3000
Boulder, Colorado 80307

Telephone: (303) 497-1684

Name: Prof. Edward N. Lorenz

Address: Massachusetts Institute of Technology
Department of Earth, Atmospheric and Planetary Sciences
Room 54-1620
Cambridge, Massachusetts 02139

Telephone: (617) 253-4850

Signature Robert C Beardsley

Date June 29, 1987

*The Committee requires supporting letters from the referees listed above. These may by submitted after the nomination form deadline but not later than October 1.

Henry Stommel
WOODS HOLE OCEANOGRAPHIC INSTITUTION
WOODS HOLE, MASSACHUSETTS 02543

Nov 7 '87

Dear Carl and Walter

Doug Webb lives across the road from me. Sometimes we find a moment for relaxed conversation. I want to share a recent discussion.

Doug is designing a float that moves up and down, reports to satellites, swims laterally at about 10 km/day, and gets it's power from temperature differences encountered. It could avoid entrapment by eddies by swimming through them.

Several such floats could police the horizontal component of velocity around the perimeter of a tomographic array, obtaining space-averaged measures of low frequency velocity structure — that might be useful in comparisons with the tomographic measures.

Doug is already working with Davis and Price (separately) on other float projects. To date, however, he has no scientific collaborator for the swimming floats. He is enthusiastic about the device and would like to proceed with it.

Do you have any ideas or suggestions that might help get this idea translated into a real system? I write to you both because of your interests in the closely related tomographic techniques.

Hank

Carl
Telem.

Action?

Command? compose
To: c.wunsch
CC:
Subject: hank's letter.

Text:
i really don't know what to do about hank's note. i have not had any recent reports how good a job he has done on the tomographic sources. but i am bothered about the lack of calibration and testing that was characreristic of doug's work. with regard to the deep float he is doing with russ, i have not heard any enthusiastic reports. under those circumstances i am reluctant to push for support for a new ideas that doug has come up with.

(when i say "he" i mean of course doug webb). walter

H. STommel

.
Send? y

Msg posted Nov 18, 1987 6:18 PM EST MSG: IGIH-3239-4961

Command? bye

This mail session is now complete.
MAIL DISCONNECTED 00 40 00:00:11:01 87 32

@

Posted: Sat Nov 21, 1987 12:00 PM EST Msg: BGIH-3243-8824
From: C.WUNSCH
To: w.munk
Subj: hank's letter

Walter..I've been at Woce and Topex meetings for two weeks so am just catching up.

As to the Webb business, I have not yet responded to Hank either. At this stage, Russ's float work, almost all the SOFAR float work in the world, and the MIT tomography effort are dependent nearly 100% on Doug Webb, something that I (and Russ) are acutely aware of.

Doug's talents (and they are very real) lie in novel techniques - their formulation, and tentative demonstration. This new idea is typical of that . His very great weaknesses lie in his disogranization, and lack of interest in many of the critical details (like adequate testing programs).

He should really be working with some enlightened, cash rich company that would pay him to work with scientists in the initial development stages of something, but which would take away the projects as soon as it was clear they needed to be developed to a more operational stage.

But no such organization exists, and I am inclined to tell Hank that Doug is so over committed already, and so much depends upon his ability to meet a potentially enormous demand for WOCE floats (Russ speaks of thousands), that he should be actively discouraged from taking on anything more. In a practical way, I don't know of anyone working with float technologies who isn't already swamped with new problems (I include in that the issue of combining the RAFOS floats more directly with tomography).

Carl

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INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS, A-025

LA JOLLA, CALIFORNIA 92093

23 NOV.

Dear Hank

Doug Webb's float sounds intriguing, like most of his ideas. What a good idea to get power from Temp. differential.

My only concern is that Doug already has so many things in the fire. And he

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LA JOLLA, CALIFORNIA 92093

does have the tendency to start one thing before thoroughly testing the last thing.

He ought to be able to work on new ideas and let others worry about testing and completion.

I hope you are well -

Walt

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
Cambridge, Massachusetts 02139

DEPARTMENT OF EARTH, ATMOSPHERIC, AND PLANETARY SCIENCES
Telex: 92 1473

November 23, 1987

Dr. Henry Stommel
Woods Hole Oceanographic Institute
Woods Hole, MA 02543

Dear Hank:

Doug Webb is a great asset to oceanography and I have always tried to encourage him to explore his many ideas for new instrumentation. At the present time though, I'm reluctant to encourage him to take on yet another project. He has made what are becoming ever more vast commitments to provide floats for Davis, Price, and French oceanographers (and probably many others I don't know about). The numbers being discussed by Davis go into the thousands. He is also the "sole source" for the MIT/French tomography development. He has designs for new acoustic sources, water samplers, fast fish, etc. A number of us have worried aloud what would happen to physical oceanography in the next 10 years if something happened to Webb.

Doug is trying to make his way as a commercial vendor, but he still treats his vendees (if that's the word for people like myself who buy things) as though they were co-pi's with him on a contract at WHOI - with not much attention for things like quality control, delivery deadlines, testing, etc. If Doug can get his commercial house in order, then it would be fun to help him take on something else that could be very interesting. Even if things were under control there, my impression is that finding Doug the right partner for another float variant wouldn't be easy right now. The community is still trying to digest the Davis/Webb popup technology, the Rossby RAFOS system, and we are struggling to combine RAFOS with tomography. I would guess that most of the potential users are already over their heads with float problems.

I'm sorry to sounds so negative; I do have really high regard for Doug. But I'm also a little exasperated by the experience of trying to work with him and his tendency to jump onto the next (and by definition more exciting) engineering problem before he has really solved the last one. Doug really needs an engineering partner who can carry things to the deployment stage, while Doug dreams up new ideas. I don't know how to arrange that for him.

Sincerely,

Carl Wunsch

xc: Walter Munk

(Message Inside)

PRESIDENT'S COMMITTEE ON THE
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WASHINGTON, D.C. 20550

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Professor Walter Munk
Institute of Geophysics & Plantetary
Physics A-025
Scripps Institution of Oceanography
University of California
La Jolla, CA 92093

This is to acknowledge receipt of your letter in support of the nomination of Henry Stommel for the National Medal of Science.

We appreciate your interest and effort in support of this most distinguished award.

Chairman
President's Committee on
the National Medal of Science

Henry Stommel
766 Palmer Ave.
Falmouth, Mass. 02540

Dec 21, 87

Dear Walter,

Thank you indeed for your kind note of sympathy. On Dec 13th I had a rectal hemmorage, and in 4 hours passed out from loss of blood. Painless, and it would have been an easy way to go. But the doctors stabilized things, got a bleeding polyp out, and have put me on my feet again. So, of course, I'm happy to be well and home again. And I haven't spoiled my family's Christmas.

With my love to you and Judy

Henry