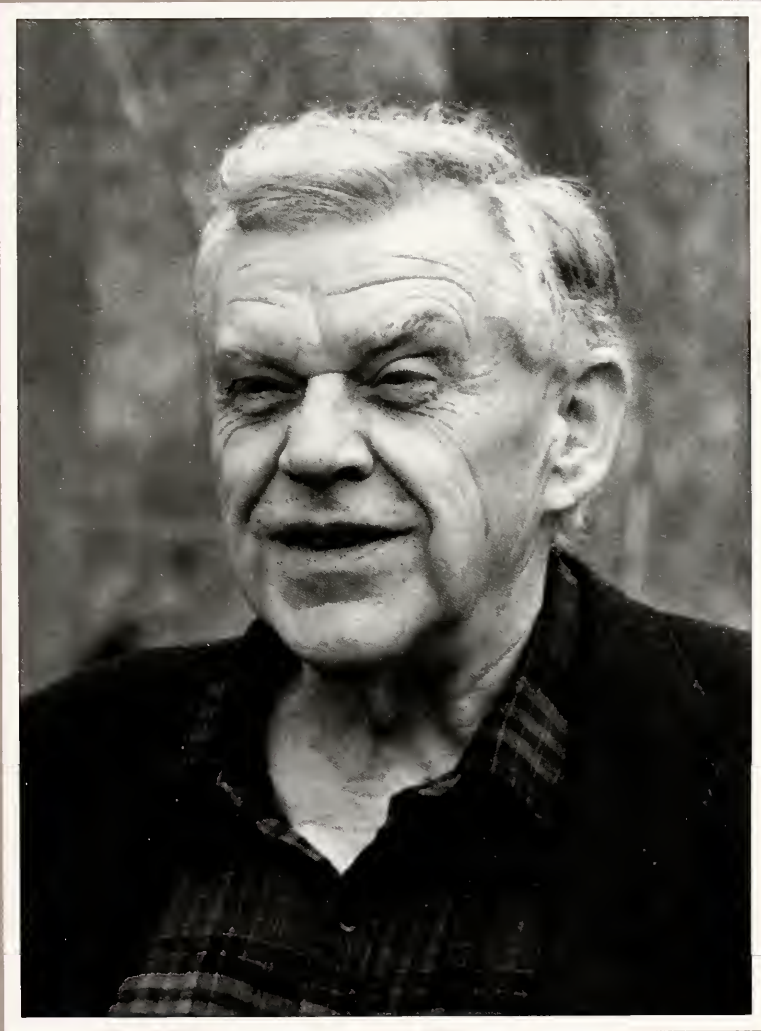


Oceanus

Volume 35, Special Issue 1992

A Tribute to Henry Stommel



Most 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 to live in a society and at a time when we are actually paid to explore the universe.

Henry Stommel, 1974

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Woods Hole Oceanographic Institution
Woods Hole, Massachusetts 02543,
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Foreword



For most of the past 50 years, Henry Melson Stommel was the most influential figure in oceanography. Through his simple brilliance, his personal magnetism, and his great zest for life, he inspired legions of oceanographers.

How can you pay tribute to a man who made room in his heart and life for so many colleagues and friends? What made Hank Stommel such a remarkable human being? This volume has given some of us an opportunity to describe aspects of our relationships with Hank. Each of us has an image of who Hank was, how he worked, how he engaged us. There will be a tendency to solidify this into the *real* Hank Stommel. When this used to happen, the image never got a chance to jell—Hank would come along and blow the preconceptions sky high. He can't do that anymore, so we can only keep the reality by attempting to record the breadth of his vision and the depth of his passion.

What made Hank a truly remarkable person was his vast interest in the phenomenal world. His contributions to the science of oceanography are monumental and have been described and reviewed many times, most recently by George Veronis in the *Journal of Marine Research* [50(1992):i-viii]. Excerpts from that article follow the Table of Contents. As George notes, Hank's influence on the oceanographic community was also great—he brought people together in the interest of learning something about the world, whether specifically oceanographic, scientific, or about growing prize-winning zinnias, about tractors, trains, human nature, and human foibles. The common binding factors were his humor, humility, generosity, and prodigious curiosity.

Hank was a complex man with tremendous personal courage and a keen sense of

the pain of human existence. He steered away from confrontation and conflict, trying to defuse situations through humor, although he was not afraid to be honest and straightforward. He deplored the political shenanigans, back-room deals, and accumulation of personal power that have become a part of the academic establishment. As much as possible, Hank worked to keep the attention on the scientific questions, raising the discussion from the personal level.



Vicky Cullen

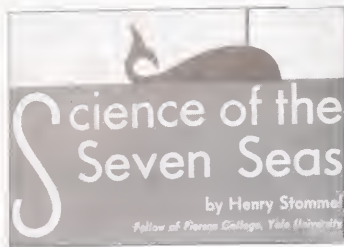
Hank Stommel

The 60 contributions to this volume provide the genuine flavor of the man and illustrate the ways in which he infected us all with his humor and care. We have also chosen to include unpublished pieces of Hank's writing about science, as well as some of his contributions to the Falmouth, Massachusetts, newspaper *The Enterprise*, under his nom de plume, Starbuck.

We have included everything that was submitted for this issue, whether specifically solicited or not. To those who would have liked to contribute and who were not asked directly, we can only apologize sincerely for this oversight. We tried to cover the bases. ❖

Jim Luyten and Nelson Hogg
Guest Editors

James Luyten and Nelson Hogg are Senior Scientists in the Physical Oceanography Department of the Woods Hole Oceanographic Institution.



❖ Page 8



❖ Page 67



❖ Page 105



❖ Page 52

Table of Contents



Foreword.....	1
Henry Melson Stommel by George Veronis.....	4
Biography from <i>Science of the Seven Seas</i>	8
❖ <i>Reminiscences</i> ❖	
<i>Arnold Arons</i>	9
<i>Wilton (Tony) Sturges</i>	10
<i>Jeffries Wyman</i>	11
"On Honorifics" by Starbuck.....	12
<i>George Veronis</i>	13
<i>Joseph Pedlosky</i>	14
<i>Henry Charnock</i>	15
<i>Kirk Bryan</i>	16
<i>John Knauss</i>	16
<i>Warren Wooster</i>	18
<i>Arthur (Rocky) Miller</i>	19
"When Science Failed" by Starbuck.....	20
"Gambling Frenzy" by Starbuck.....	21
<i>John Swallow</i>	22
<i>Bruce Warren</i>	23
<i>Redwood Wright</i>	24
<i>Walter Munk</i>	24
1958 Letter to Deep Sea Research.....	25
<i>Allan Robinson</i>	28
Tribute to Walter Munk.....	29
<i>Alan Faller</i>	30
"Trial by Gutenberg" by Starbuck.....	31
<i>Edward Spiegel</i>	34
<i>Willem Malkus</i>	35
<i>Lou Howard</i>	35
<i>James Crease</i>	37
<i>Sloat Hodgson</i>	37
"My Friend John Moore" by Starbuck.....	38
<i>Nick Fofonoff</i>	40
<i>Robert Walden</i>	40
<i>Robert Knox</i>	41
<i>Curtis Collins</i>	42
<i>Arthur Voorhis</i>	43
Letters Henry Stommel Wrote to His Children.....	44
<i>Bob Beardsley</i>	48
<i>D. James Baker</i>	48
<i>George Seaver</i>	50
Why We Are Oceanographers.....	51
<i>Barbara Gaffron</i>	64

<i>Peter Niiler</i>	64
<i>Valery Lee</i>	65
The Albatross Award (Article from <i>The Euterprise</i>)	66
“Not Afraid of Life” by Starbuck	68
<i>Robert Heinmiller</i>	69
“We’re Over the Ridge!” by Starbuck	70
<i>William Schnitz</i>	71
<i>Harry Bryden</i>	71
<i>Joseph Reid</i>	72
<i>W.S. (Wally) Broecker</i>	73
<i>Peter Rhines</i>	73
<i>F.C. (Fritz) Fuglister</i>	74
<i>Valentine Worthington</i>	76
<i>Cecelia Fuglister</i>	76
<i>Shirley Webb</i>	77
The Ocean and William Leighton Jordan, Esq.	80
<i>Carl Wunsch</i>	92
<i>Myrl Hendershott</i>	93
<i>David Halpern</i>	95
<i>Bob Munns</i>	95
<i>James Luyten (The Pencil Is Mightier than the Computer)</i>	96
“Close Encounter” by Starbuck	97
<i>William Young</i>	98
<i>Robert Weller</i>	100
<i>Jean Claude Gascard</i>	101
<i>William Simmons</i>	102
<i>Walter Zenk</i>	106
<i>Nelson Hogg</i>	107
<i>Charles (Chip) Cox</i>	108
<i>Bernard Saint-Guilly</i>	108
“High-Iron” by Starbuck	109
<i>Michèle Fieix</i>	110
“Counting Our Blessings” by Starbuck	111
<i>James Luyten (Annealing)</i>	112
<i>Philip Richardson</i>	113
<i>John Van Leer</i>	114
<i>Dennis Moore</i>	115
<i>Gabe Csauady</i>	115
The Woods Hole Marine Exhibit.....	116
<i>Roberto Frassetto</i>	122
<i>Oleg Mamaev</i>	124
<i>Rui Xin Huang</i>	126
“The Viper’s Jaws” by Starbuck	127
National Medal of Science Nomination by Harmon Craig...128	
Curriculum Vitae for Henry Stommel	131

Cover photo by Holly Smith Pedlosky



❖ Page 94



❖ Page 55



❖ Page 35



❖ Page 129

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Henry Melson Stommel

September 27, 1920–January 17, 1992

by George Veronis

Excerpted with permission from

Journal of Marine Research, Volume 50, 1992

❖ ❖ ❖

Henry Stommel's heart stopped beating shortly after midnight on Friday, January 17, 1992, four days after he had undergone surgery for liver cancer at Deaconess Hospital in Brookline, Massachusetts. His death brought to an end the career of a man who, for 45 years, was the most significant scientific contributor to the development of oceanography and who brought a rare degree of harmony and collegiality to the field.

When Hank arrived at the Woods Hole Oceanographic Institution (WHOI) in 1944 there was little reason to suppose that anything momentous had taken place. As an undergraduate at Yale he had been advised by a counselor that, since he evidently had no talent for science, he should take up law. In 1944 he was a second-year graduate student in astronomy at Yale and a conscientious objector to war. The job at Woods Hole was a way of serving his country without going to the battlefield.

The Yale astronomy department from which he had come was strongly focused on celestial mechanics, and Hank had developed an interest in the marine environment through his study of celestial navigation, one of the courses that he taught. He had read a lot about the ocean and decided to prepare a synthesis that he dedicated to the students in the Navy program in which he had been teaching. It was a trait that was to stay with him throughout his life; he would be heard. Over a three-week period he wrote a 208-page book, *Science of the Seven Seas*, which was published by Cornell Maritime Press in 1945. In later years he was somewhat embarrassed about that qualitative survey, but it sold more copies than all of his other books combined.

Toward the end of the war he moved in with a group of WHOI bachelors who occupied the former Episcopal rectory in Woods Hole. His clowning around during those bachelor years was legend, and so was his intense study of all aspects of oceanography. He liked people and he loved life and the wonders of nature. In the years that followed he took part in many oceanographic cruises where he could enjoy the social and professional camaraderie of his colleagues in a setting devoid of the distractions of modern life. Life at the rectory served as an introduction to life at sea.

Hank's professional development during those early years resulted in an intriguing mix of publications. Some of them, summarizing what he had learned, amounted to a kind of report card. They were part of what he would later refer to as his "commentarial ambition." Others, reporting measurements and observations, documented his growing familiarity and involvement with the world of nature. But it was the third category that revealed his originality and true genius.

When Hank arrived at the Woods Hole Oceanographic Institution in 1944 there was little reason to suppose that anything momentous had taken place.

One of his first articles, in 1947, dealt with the computation of the dynamic height anomaly, a topic that was anything but arcane. He proposed using the observed T-S [temperature-salinity] correlation in certain regions to estimate the missing salinity when only temperature had been measured, and thereby to determine an approximation to the dynamic height anomaly. What makes that study conspicuous is not the careful evaluation of the feasibility of the method but the issue that he addressed. Even in his first year of published research papers he was already thinking about how to make up for the inadequacy of oceanic observations that has always plagued physical oceanography.

Two years later he considered a steady convection cell with upward flow in the middle and downward flow at the sides and asked whether particles heavier than water could remain in suspension. He proved that as long as the terminal velocity of the particles is smaller than the maximum fluid velocity there would be a closed region containing suspended particles. Since 1949 people in many different fields have come to the same conclusion. Though he felt that the result would be applicable to cloud physics and to a suspension of particles or organisms in the sea, it was the study of the physical process that intrigued him.

Of course, it was the westward intensification paper that put the name Stommel permanently into the dictionary of ocean circulation. In 1955, when he was invited by Jule Charney to visit the Institute for Advanced Study in Princeton, he and I worked on a problem together, and I asked him whether the westward propagation of Rossby waves suggested the idea to him. He said that there was no connection and told me the following story.

Early in 1947 Ray Montgomery mentioned to him a question that had been posed by Columbus Iselin, who was then director of WHOI. Why was the Gulf Stream a narrow current pressed against the western side of the Atlantic when neither the thermal driving nor the wind had any such asymmetry? Hank set up a theoretical model to treat the problem and found that he had to solve a boundary value problem involving an elliptic partial-differential equation. He had derived another such equation for a tidal problem and had learned that R.V. Southwell had just published a book on relaxation methods for the numerical solution of elliptic equations. So he decided to learn the relaxation method by applying it to the circulation model. This was before electronic computers were developed; relaxation by hand involved many months of tedious computations using a mechanical



An early photo of Henry Stommel

It was the westward intensification paper that put the name Stommel permanently into the dictionary of ocean circulation.

Stommel's scientific inquiries are enough to place him on the list of scientific immortals, but what really set him apart was his style.

calculator. Since the wind stress in his model had east-west symmetry, his initial guess for the circulation had the same symmetry. But in reducing the residuals he found that the circulation immediately started to shift toward one with a stronger current on the western side. After some experimentation he discovered that the westward shift was caused by the beta term (the variation of the Coriolis parameter with latitude). So instead of continuing with the relaxation method, he formulated a simpler model, one that contained the beta term but could be solved analytically. The publication of that analysis in the 1948 *Transactions of the American Geophysical Union* marked the beginning of modeling of large-scale ocean circulation.

Hank said that that experience taught him the value of developing models that isolated the essential physics in the simplest mathematical context. As the years wore on, he became so proficient at that and developed such a powerful intuition that in starting a discussion he would leap past all of what he considered unessential preliminaries and start with the analysis of his "simple" model. Not infrequently the listener would just hang on, hoping that as Hank neared the end, the original question might become apparent.

In looking back on his career he felt that his most rewarding observational effort was the MEDOC [MEDiterranean Deep Ocean Convection] cruise in January 1969, when he took part in an attempt to document the formation of bottom water in the northwestern Mediterranean. Sources of bottom water must be present in all circulations involving overturning, and since they are episodic, direct observation of them is 95 percent luck even if one is in the right place. Hank had no desire to go on a winter cruise to the Greenland Sea, a source of bottom water for his abyssal circulation model, but he thought that winter observations in the Mediterranean might be tolerable. After a period of apprehensive waiting in miserable seas, the MEDOC group got its chance when the mistral wind started blowing. Within a few days the surface waters had cooled enough to overturn and measurements during that period provided complete documentation of the event.

Although his main interests were directed toward theory and observations relating to large-scale circulation, Hank knew that a firm understanding of physical mechanisms was a necessary prerequisite. He analyzed a large number of nonlinear oscillators over the years in an attempt to understand the different forms of circulation that the ocean might occupy. He initiated studies in double diffusion, first with Arnold Arons and Duncan Blanchard (the salt fountain) and then with Stewart Turner (stable salt stratification). For much of his career he sought to find out whether the circulation could be described in terms of point vortices, and in the past few years he and Nelson Hogg explored the representation of heat flux in terms of point baroclinic vortices. How to determine the absolute velocity field in the ocean was a topic to which he returned many times until he finally hit upon the idea of the beta spiral with Fritz Schott.

The acquisition of a personal computer in the early 1980s enabled him to expand the scope of his studies of simple models. With it he collaborated often with Jim Luyten and more recently with Xin Huang on a variety of simple dynamical models and box models to address con-

ceptual issues pertinent to circulation and climate. He based an introductory course about ocean circulation on a set of programs that he wrote for his book, *A View of the Sea*, published in 1989.

Stommel's scientific inquiries are enough to place him on the list of scientific immortals, but what really set him apart was his style. Better than anyone else that I have met he understood the human condition and the value of humor in coping with life. A conversation or collaboration with him would be interrupted by his frequent peals of laughter brought on by a funny incident or by the joy of discovery when something worked out. What amused him the most were human foibles, his own as well as those of others. Though he had more reason than most to feel self-important, he shunned pretention; any display of vanity would start him chuckling and even howling with laughter if he knew that he would not hurt another's feelings.

From the beginning of his career he displayed an uncanny ability to bring people together. After he and Elizabeth (Chickie) Brown were married in 1950, they frequently entertained visiting oceanographers as well as Hank's colleagues from WHOI and friends from the local community. Columbus Iselin, the director of WHOI, lived on Martha's Vineyard and had little unofficial contact with visitors. Hank and Chickie filled that void even though it must have been a heavy financial burden (in early 1950 his annual salary was still only \$3,000). As Hank's fame grew, more and more visitors came to WHOI to see him—and stayed for dinner. I have the feeling that nearly every physical oceanographer in the world must have eaten one of Chickie's dinners.

Entertaining Hank's colleagues while raising three children could not have been easy for a wife who was trying to establish her own identity through writing and music. In spite of the demands on her time, Chickie became the organist of the local Episcopal church and helped to develop the close contacts with the local residents that the two of them maintained over the years. In 1980 the Stommels coauthored *Volcano Weather, the Story of 1816, the Year Without a Summer*. Chickie did most of the historical research and Hank provided the scientific interpretation.

I believe that the cohesion and collegiality that Hank brought to oceanography are unmatched in any other field. He collaborated with an astonishing number of colleagues, particularly younger ones. He would often seek out someone who was clearly struggling with an idea and help him with a detailed calculation to carry the idea further. His generosity in sharing his own original thoughts

His generosity in sharing his own original thoughts was a constant source of wonder.



Chickie and Hank Stommel



Science of the Seven Seas

Henry Stommel's first publication in oceanography was called *Science of the Seven Seas*. It wasn't his favorite publication—he is said to have tried to burn all the copies!—but he did list it in his curriculum vitae. The 208-page book was published in 1945 by Cornell Maritime Press, New York. The book jacket offered the following sketch of the author:

"Henry Stommel is our youngest author. 'I was born,' he writes, 'on Sept. 27, 1920, in Wilmington, Delaware. Brought up in

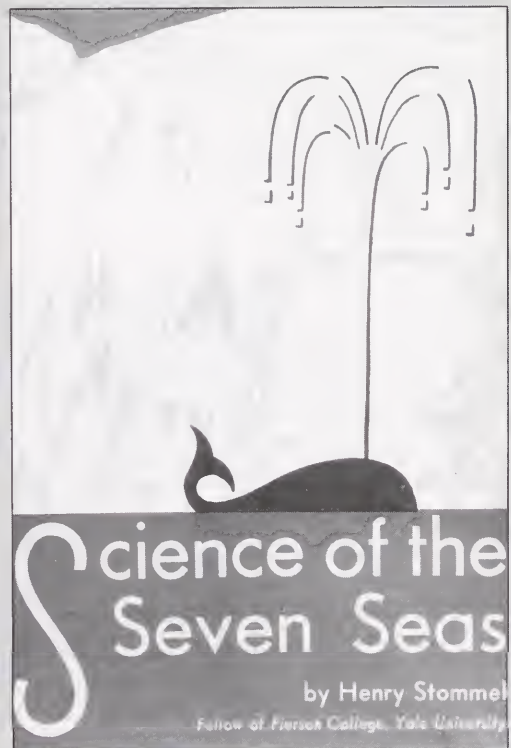
Freeport, New York, I spent much of my time in small boats and tinkering around with scientific gadgets, microscopes and aquatic bugs, chemical experiments.

'I went to Yale University, joined the Corinthian Yacht Club there, graduated in 1942 after majoring in physics. Upon graduation I was appointed an assistant in the Physics Department and then promoted to Instructor of Navigation and Nautical Astronomy. Besides physics and navigation, I also taught undergraduate mathematics as the occasion demanded. In 1943 I was appointed Fellow of Pierson College.

'Pierson College was one of four Navy V-12 colleges at Yale. It was my good fortune to become very well acquainted with the Navy trainees in our college. I suppose I still could pick all 400 of them out by first name. I found them very much interested in the science of the seas, and so I thought it might be worthwhile to set down some facts about the oceans in book form.

'In October 1944, the National Defense Research Council requested that I take up some research work for them, at which task I am now employed.

'I am sorry this biography is so small, but I really haven't lived much yet.'" ❖



was a constant source of wonder. Stature or position mattered little to him. He would look past the quirks of personalities and focus on the development of a concept. No matter what their personal differences were, his collaborators shared one characteristic—dedication to research. That was all that he looked for, and it was enough for him to bring people together. I remember looking at the remarkable assortment of individuals at a large party at his house and realizing that we would never have come together in a social setting were it not for the close friendship that each of us felt for this one man. ❖

Arnold Arons

University of Washington (retired)



One fascinating aspect of working closely with Hank was watching his almost ferocious tenacity in attacking a problem. When we got stuck, he would never give up. The next day, or even a week or a month later, he would come back with a new angle, a new version of a boundary condition, a new physical insight, and initiate a revised attack. It was clear that he was rolling the subject over and over in his mind all hours of the day. This tenacity was, of course, informed by his incredible intuition and his skill in coupling this intuition to the simplest but most penetrating mathematical formalism. I never saw him give up until at least some plausible dent had been made. After the dent, he sometimes lost interest in the problem (this was only in connection with relatively minor problems; he never lost interest in the major ones), and then it was virtually impossible to get him to return to it. One could raise the issue for further discussion; he would listen kindly and provide gentle encouragement or show you what was nonsensical, but he would not himself return to it full blast.

Once somewhere around 1955 or 1956, Hank undertook to consult me about a personal problem that turned out to be nagging him. He was frequently being addressed as Dr. Stommel, and he was embarrassed because he was not a “real” doctor. His question was: Should he go back to graduate school and get a doctorate? My immediate explosive reaction was “Under whom?” He wasn’t sure, but possibly Rossby or someone like that. I finally said something like this: “Hank, within a few years you will have more doctor’s degrees than you will know what to do with. If you were to go back to graduate school, you would be wasting precious time filling all sorts of now meaningless requirements, and worst of all, you would be subject to being examined by people whom you should be examining. Please stop even thinking about such a move.” He probably consulted others besides me; I don’t know, but I imagine others would have said the same thing. Fortunately, I never heard any such moonshine again. His sense of security became firm with rapidly growing recognition. ❖

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Wilton (Tony) Sturges

Florida State University



The main reason I adored Stommel so much was that he helped me understand that it is OK to have fun. Almost all the authority figures I can remember, whether deans, directors, professors, or parents, gave me the impression that work was serious business. Stommel seemed to think that doing science should be great good fun, and it rubbed off on me, to my enormous benefit.

In the late 60s, he was putting together the program that would be called "Pre-MODE." I remember a meeting at WHOI where Richard Scarlet and I were talking at coffee break; Henry joined us, and asked whether we had seen his steam whistle. When we said no, he insisted that we come over to his house that afternoon to see his wondrous toy. We were very junior and he had already achieved prominent status by

then, so we would have gone to see a bucket of plain water. He showed us, in great detail, his whistle from a real locomotive, connected to a large air tank in the barn. The "steam" came from a small air compressor, which took ages to work up a good head of pressure, so we went into the house for a cup of coffee while the pump went to work. He had rigged a red



Using the Compton generator, Hank Stommel demonstrates the Coriolis force for a junior high school class in 1991.

light to come on when the pressure built up.

Eventually the light came on and it was time to play. With great delight, Henry let 'er rip. Scarlet and I were astonished; the blast must have made people jump as far away as the Bourne Bridge. It echoed back and forth for some seconds. He made a comment to the effect that the neighbors weren't too keen on the thing. I don't think Chickie volunteered any comment about it.

Another time, I was walking down the hall in the WHOI Clark Laboratory, having just arrived for some meeting, and Henry passed me, pushing what seemed to be a large pile of garden hose mounted on top of a bar stool. He asked, "Tony! have you seen my Coriolis demonstration?" Knowing full well that I hadn't, he proceeded to tell me how it worked, explaining that it was designed to show schoolchildren

about the slope across the Gulf Stream. He turned the handle, making the spool of garden hose turn around, and as he was talking he walked around the stool, turning the whole contraption around with him. There was a vertical glass tube attached to the side, to act as a pressure gauge. It was a sort of Rube Goldberg, rub-your-stomach-and-pat-your-head kind of demo, and it did look pretty gosh-awful, but as he talked, and walked, and turned, damned if the water level in the tube didn't start to go up, just like he said. Nearly 3 feet. If he had been some crackpot who walked in off the street, it would have been awfully embarrassing, but Stommel was able to pull it off. He was obviously having a great good time, and demonstrating the effect of the rotation of the earth at the same time. He realized that it looked a little silly, of course, and he was laughing all the while, with a twinkle in his eyes like a schoolboy getting away with a grand prank.

I see that playfulness, that joy, so rarely. It gives me hope that perhaps I can have some of it in my life. My own father was such a good man; yet he was a prime example of what it means to work hard. I don't think he had much fun, particularly in his job, and I watched him slowly run down as he got older. Had it not been for Stommel I might not have recognized what was wrong. I suspect that there are rough equivalents in other fields; in the movie *Top Gun*, Maverick's expression was "It's time to buzz the tower." Stommel showed me how to buzz the tower, and how to do it right. ❖

Jeffries Wyman

Paris, France



My friendship with Henry Stommel was one of the warmest and longest of my life. It goes back to the days of the war when we were both working, he full time and I as a consultant, at WHOI. It took me to many new places and introduced me to many new ideas that have greatly broadened my perspective.

We were both occupied with problems centering about sonar and, more generally, submarine detection; but soon we changed direction to the study of the Navy's use of smoke screens. The latter involved the exchange of matter and energy between the air and the surface in two tropical oceans. It involved much travel. This introduced me to many new physical concepts, such as convection and eddy diffusion, and Lord Rayleigh's analysis of instability.

At that time I was living on Naushon Island, near Woods Hole, and I commuted every day by kayak, which gave me a breath of fresh air. Stommel would often come over for visits on weekends and it was then that I learned to appreciate him and his special sense of humor, expressed in the miniature railway he built in the large vegetable garden behind his house in Falmouth. I can see him now sitting in the driver's seat of the small locomotive, but, alas, the driver is gone. ❖

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On Honorifics

by Starbuck

HALF A LIFETIME AGO, when I still felt flattered at being addressed as professor, I happened to be invited to a friend's house in Coconut Grove. I stopped on the way at a neighborhood liquor store to purchase a present of a Greek aperitif for his wife. The owner was busily checking some invoices on a newly arrived shipment, and I complimented him on the variety of his wares. He explained that he was a professor of beverages at the University of Miami—an academic career the possibilities of which I had somehow never considered.

Time was that the only doctor in town was a physician. Very occasionally one might encounter an eminent divine sporting a D.D., but a non-medical doctor was something of a rarity. Woods Hole was, of course, an exception. Then, as part of our national response to Sputnik in 1957, there was a vast expansion of our graduate schools. A Ph.D. became necessary for entrance into a career of government-sponsored research.

Today there are probably more Ph.D.'s in Falmouth than there were B.A.'s two generations ago. At a recent hearing it was asserted that one housing development is exclusively inhabited by them.

Sometimes it leads to confusion. Years ago I was staying at the home of one of my oldest friends and mentors, author of a distinguished treatise on physiology. He has done fundamental research on hemoglobin. Before he turned 80 his days were filled with adventure: a pack trip through the mountain passes of Kurdistan, or a solitary perambulation through the jungles of New Guinea with two paroled cannibals for guides. For several years he paddled a kayak back and forth between Woods Hole and Stone House on Naushon every night during the winter. Today he lives and works in Rome. He is a Boston patrician of the old school and, I think, the wisest man I know.

It was after dinner, and we were sitting in the parlor of his house in Chestnut Hill, next door to the Cabots. He had recently lost his wife, and conversation seemed to do him some good. The telephone rang faintly in the hall, and we could hear the maid going to answer it. "Yes, this is Doctor ...'s residence." A pause, and then in her thick brogue: "Oh no, Mam—he's one of them doctors that never does anybody any good." ❖

The Enterprise
March 8, 1985

George Veronis

Yale University



When Hank Stommel arrived at the Institute for Advanced Study in Princeton, New Jersey, for a three-month stay in the spring of 1955, he and I had both been working on time-dependent motions in the ocean, I on geostrophic adjustment and he on the influence of the beta-effect on wave motions. We began collaborating on a study of waves in a two-layer ocean. We were grappling with the asymptotics of a sixth-degree dispersion relation when Gordon Groves, who was then at Scripps, visited and gave a seminar in which he talked about the normal-mode treatment of physical systems. That procedure was just what we needed to reduce our sixth-order system to two uncoupled third-order systems, an enormous simplification that enabled us to describe the two-layer linear response in terms of barotropic and baroclinic modes.

We both worked hard on the analysis and the write-up, and then, in his typically generous fashion, Hank listed me as first author. The paper, "The action of variable wind-stresses on a stratified ocean," was published in the *Journal of Marine Research* in 1956. I was sufficiently young and vain to agree to the order of names, particularly since our acronym for the paper was AVS, which we jokingly referred to as "A Veronis-Stommel" paper. Some time after the paper appeared, I realized that it was I who had joined Hank on that work, and that the order of the names should have been reversed. Furthermore, Hank pointed out to me that we had neglected to acknowledge the help from Gordon Groves, who had introduced us to normal modes.

No issue in the realm of academics is as thorny as that of priority of ideas and proper credits. Most of us find it easy to talk ourselves into feeling that we have prior claim. I was lucky to have encountered the problem for the first time with someone as understanding and generous as Hank, who avoided controversy by listing the other person first. But I figured that it would be better to avoid the entire issue by an objective procedure, and in publications with other people I have adopted the practice of listing names of coauthors in alphabetical order. Very few people have objected to that, perhaps because my name almost invariably comes last.

When I became editor of the *Journal of Marine Research*, I asked Hank whether he thought that I could suggest alphabetical listing of coauthors for all papers in *JMR*. His response was, "I thought you wanted to avoid controversy." ❖

We both worked hard on the analysis and the write-up, and then, in his typically generous fashion, Hank listed me as first author.

Joseph Pedlosky

Woods Hole Oceanographic Institution



My keenest recollections are of Hank bursting through the door of my office with some excited gleam in his eye announcing, "Look at this, you might find this interesting, don't you think?" or "What do you make of this?" These were always invitations to draw closer and to take up the adventure with him if you were ready for the ride.

If you ever had the pleasure of working with Hank on some scientific problem, you realized quite quickly that doing Science together was one of Hank's principal ways of getting close to other people.

I had met Hank in the early 1960s, but it wasn't until I got to WHOI in 1979 that there was much of a chance to work together, and even then it took more time than I am happy to think about before we really got together on something good. We started to work on the thermocline problem with Jim Luyten. Jim and Hank had already begun some numerical studies of the ventilated thermocline, and I was able to find a verifying analytical solution that together with Hank and Jim's original calculations really got us going.

A good deal of the fun then was in the childhood games of playing together—Hank's notes slipped under the door awaiting me in the morning, each note dated with the time of night when the thought occurred, or the telephone calls that started in mid sentence with no identification ("Daddy, I think it's Hank.") and began with a "You knooow..." or "Aaall right..." Even more exciting was his relentless pressure to get the problem clear, understand how it really worked, bring it all into the open, and finally say, as he did when we had finished, "No one will ever think about the thermocline the same way again." This wasn't a boast, but his own test of how to know when a problem was at least temporarily finished.

When we had completed the ventilated thermocline paper, before it was even published, a circulated draft of the paper was sharply criticized in the gray literature. I remember one Sunday morning getting another Stommelian call that came at a late hour for Hank, but an early hour for me. It began...(click) "Did you read X's article in *Ocean Modelling*? No? Well, you had better read it. I'll bring it right over." Two minutes later, Hank is at the kitchen door and I'm still in pajamas.

"Come in, come in."

"Oh, no no no, can't stay, can't stay—but see if you can deal with this—give me a call."

Nerves turned to relief when the soft spot of X's article was found. What amazed me was the relief Hank also felt. It was the first time I had seen him apprehensive about his own work.

Hank would ruminate about the other reasons he did Science. Work was a way to drive off what he called the "background melancholy" produced by a clear, steady vision of mortality. Work held that off and directed his imagination away from the dark places. He didn't like to dwell on the dark places, but he appreciated them and wouldn't hide them. His passion for Joseph Conrad and especially for *Heart of*

*There were
always
invitations to
draw closer
and to take up
the adventure
with him
if you
were ready
for the ride.*

Darkness and Victory was a glimpse into this dark side. Winning at roulette on the *Titanic* was the way we had talked about it together.

He responded once to the joke about the rabbi who paid off his cash debt to a dead man with a personal check placed in the coffin with, "That man understood the darkness more than any of the others."

Hank loved his life and the gift of consciousness of both life and the universe. After his first big illness he talked at length about the gift. "Just think, instead I could have been just an apple," he said. "I might never have realized I was alive."

On his 70th birthday his colleagues in our department in Woods Hole gave Hank an accordian. He had played it a little in the past and he would, he allowed, be eager to take it up again. When his indignation that year over the financial undersupport of the WHOI/MBL library became too much for him, he suggested that he and I, both duffers on our instruments, take our accordian and clarinet at tourist time down to Water Street in front of the MBL director's office and play, with an open cigar box on the curb, to raise money for the library. With any luck at all we might even be arrested for panhandling without a license. "Wouldn't that look just fine in *The Enterprize*?" he chortled.

Well, he died before we got to do it. I suppose I could do it alone now. But like a whole lot of other things, it wouldn't be as much fun without him. ❖

Henry Charnock

The University (Southampton, England)

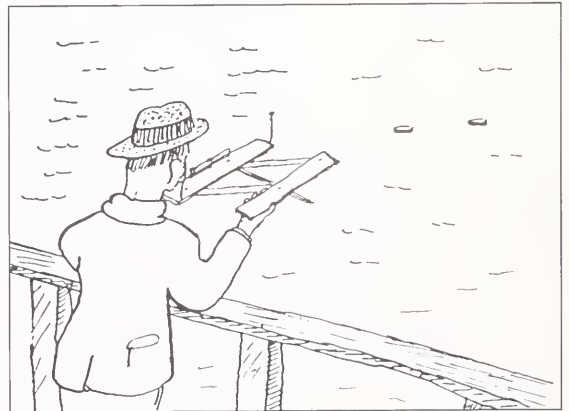


Henry Stommel had not worked for long at WHOI before he took a leave of absence (unpaid) and sailed on the *Queen Mary* (cabin class, no less) to work for a few months at Imperial College in London.

He brought a mimeographed copy of his westward-intensification paper; we found it rather simple, not then appreciating that most of his ideas seemed rather simple, after he had had them. He gave a seminar describing how patches of anomalously cold water had been found south of the Gulf Stream; we did not appreciate that as much as it deserved either.

But we did encourage him to visit Lewis Fry Richardson at his house by Loch Long in Scotland, where on January 6, 1948, they made some observations of two-particle diffusion of particles on the sea surface. It was one of the few of Henry's scientific enterprises in which he was not the prime mover. A three-page paper was promptly written and published and has become well known for its opening words—"We have observed the relative motion of floating pieces of parsnip...." but is also remarkable for its gallery of references—Defant, Einstein, Kendall, Richardson, Schmidt,

Henry Stommel sketched Lewis Fry Richardson observing floating pieces of parsnip from Blairmore Pier, using an instrument made by Richardson in his basement. Stommel later wrote that "it required keeping one eye on the parsnips, another on the compass, another on the level and the other on the distance scale." (Henry Charnock says, "I can imagine the mischievous twinkle in his eye when he wrote that 'on shipboard it would require gimbals.'")



Taylor, and Weiner. Von Weisaeker and Heisenberg had their names added in proof!

Henry's visit led to a continuing cooperation between the WHOI Department of Physical Oceanography, Imperial College, and the then National Institute of Oceanography. More important for some of us, it led to enduring friendships with Henry and his family. ❖

These experiments were often performed during coffee hour, and typically with hilariously disastrous results.

Kirk Bryan

Geophysical Fluid Dynamics Laboratory, Princeton University



My friendship with Hank goes back to two very formative years I spent in Woods Hole in the late 1950s. At that time there was a small, eclectic group on the top floor of the Bigelow Building working on geophysical fluid dynamics (though the words had not been coined yet) and meteorology. Coffee regulars among the meteorologists were Joanne Simpson, Joe Levine, and Andy Bunker. We were sometimes joined by Al Woodcock and Duncan Blanchard, and other coffee regulars were Willem Malkus, Mary Thayer, George Veronis, Al Faller, and Margaret Chaffee. Stommel's personality was the glue that held the diverse group together. You knew when he stepped into the room that he was going to come up with something interesting and amusing.

He kept the third floor of the Bigelow Building in constant turmoil with his ideas for experiments. These experiments were often performed during coffee hour, and typically with hilariously disastrous results. Some of these experiments led to more serious studies, and eventually were reported in respected scientific journals. Others, even more spectacular, went nowhere. I can still remember the puzzled look of an administrator who stopped by during coffee hour. There were a half-a-dozen people fascinated by a little tornado of dyed water in an 8-foot Plexiglas cylinder. The relevance to WHOI's research programs must have seemed very remote.

Stommel's office was an amazing clutter of paper and old instruments. Among the curiosities was an old, hand-operated printing press, which Hank allowed us to experiment with for printing notices for the joint MIT-WHOI seminars. The printing press reinforced my association of Hank with Benjamin Franklin, kindred natural philosophers with the same mellow sense of humor. ❖

John Knauss

National Oceanic and Atmospheric Administration



Henry Stommel's scientific contributions are well documented; his contributions to the fraternity of oceanographers are less so. I give two examples with which I was closely allied.



The International Indian Ocean Expedition was endorsed by the Scientific Committee on Oceanographic Research as a follow-on to the

International Geophysical Year (1957 to 1958). Henry was skeptical. The plan, such as it was, was not well thought through. What little planning that existed was top down. Those who were expected to do the work had not been consulted. He expressed these thoughts in subtle form through five issues of *The Indian Ocean Bubble*, an anonymous newsletter published in 1959 and 1960, whose publisher, editor, and chief contributor was known to all.

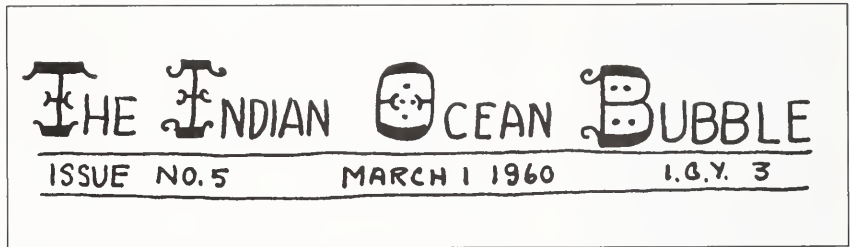
But the planning did eventually get done by those of us who were to do the work, and in later years (the International Indian Ocean Expedition lasted until 1965) Henry became an enthusiastic participant. His entry into the Indian Ocean was along the equator. I had the Scripps vessel, *Argo*, for two cruises of three months each in 1962 and 1963 to look for an equatorial undercurrent under the differing monsoon conditions, and Henry thought it would be an excellent opportunity for some of his theoretical colleagues to see how the other half lived. I was agreeable as long as all were prepared to work, so in July of 1962 Jule Charney (Massachusetts Institute of Technology) and Henry Stommel arrived in Mombasa for the second leg of the first cruise, and some eight months later Allan Robinson (Harvard University) came for the first leg of the second cruise. Each brought graduate students, including Elliot Schulman and Bob Blandford.

These were good cruises, although the results later proved very difficult to sort out. There were few equipment breakdowns. The weather was mostly good. We made a brief stop in the Seychelles. There were equator crossings and ceremonies. But mostly it was long days of work: Nansen bottle casts, reading thermometers, running oxygens, putting in buoys, recording current-meter records, etc. I still cherish a remark of Jule Charney as he stumbled out of the wheelhouse one night after spending two hours taking radar ranges every five minutes of a sometimes difficult-to-read, anchored buoy: "John, I had no idea you had to work so hard for a couple of data points." Henry Stommel's goal had been achieved.



My second example is from the same period. Vladimir Kort, the USSR's most senior physical oceanographer and director of what is now the Shirshov Institute, wanted a multiship international survey of the world ocean. Henry was not impressed. He thought we needed more focused, problem-solving research rather than one general survey at this time. Fair enough, but he also had what seemed to me to be the rather naive idea that we should go to Moscow and have a heart-to-heart talk with our Soviet colleagues, few of whom we had yet met, to sort out our differences. Somehow arrangements were made, and in October of 1962 John Swallow (National Institute of Oceanography, UK), Joe Reid (Scripps Institution of Oceanography), Henry, and I trooped off to Moscow to take on Kort and his survey proponents. I believe we were the first US oceanographers to visit the Soviet Union since World War II.

The *Indian Ocean Bubble* was published by an "anonymous" editor to comment on the organization of the International Indian Ocean Expedition.



Kort was of the old school. He knew what he wanted, but he was a gracious host, and he heard us out. More importantly, he invited his younger colleagues, not only from his institute but from others, to take part in the discussions, and it soon became clear that a number disagreed with their boss. We never convinced Kort.

There was no international world ocean survey, but the systematic surveys of the Soviet Union continued. What did result from that first trip was a number of professional relationships, friendships, and scientific exchanges that culminated in POLYMODE some years later. Once more, Henry Stommel had made a significant contribution to the fraternity of oceanographers. ❖

Warren Wooster

University of Washington



“Just how individual scientists and institutions are going to participate in this Expedition without being swindled is difficult for me to see.”

—Indian Ocean Bubble

My closest association with Hank occurred aboard *Argo* in August 1964 during what he called the “Anglo-Californian Expedition.” This was part of the International Indian Ocean Expedition (IIOE), and we worked on a phenomenon of particular interest, the reversing Somali Current, at the season when its poleward flow was most strongly developed.

Hank was ambivalent about the IIOE. On one hand, it was a prime example of the sort of top-down science planning that aroused his visceral opposition—hence his launching of *The Indian Ocean Bubble* in which, while maintaining his “gently pejorative tone” as editor, as contributor he commented, “Just how individual scientists and institutions are going to participate in this Expedition without being swindled is difficult for me to see.”

On the other hand, as suggested by his unpublished 1954 note (“Why do our ideas about the ocean circulation have such a peculiarly dream-like quality?”), he was fascinated by the opportunity to test plausible hypotheses, for example, concerning the peculiar circulation in the western Arabian Sea. While some had proposed that the Indian Ocean be surveyed with stations regularly spaced on a rectilinear grid, Hank recognized that not only would many scientists be repelled by this approach, but that the pursuit of interesting questions was more likely to both attract participants and result in significant findings.

I had just returned to Scripps after a few years in Paris with the Intergovernmental Oceanographic Commission, where I was, in a way (as coordinator of the IIOE), one of the top-down villains. Hank was forgiving enough to invite me to sail with him on *Argo* on what was in many ways a delightful cruise, especially because of Hank’s company. One can, however, remember less delightful elements—for example, the continuing nightmare of trying to make oceanographic stations in the

face of a 6-to-7 knot current with lots of shear and wire angles approaching 90 degrees. Hank and I split the watches—mine was The Queen's Own while his was The Black Watch. We shared the chief scientist's cabin as well as a large basket of Kenyan beer, purchased in Mombasa and nursed through the cruise so that on the last evening, as we approached Mauritius, there could be a bottle at each place in the mess hall. In those days, Scripps ships were generally dry while those of Woods Hole were generally not (or so we were taught at Scripps). Scientific parties are not always enthusiastic about their chief scientists, especially at the end of a strenuous cruise—and ship's company can be even more critical—but that night, we were highly regarded by all hands. ❖

Arthur (Rocky) Miller

Woods Hole Oceanographic Institution (retired)



In 1946, in my first days at the Institution, I had to find a place to live while I was in Woods Hole. There were a number of young investigators working on various projects, and in order to house them the Institution had hired the rectory of the Church of The Messiah, which was a large, old house located where the present rectory now stands. There were many rooms and a large kitchen as well as a living room with a pump organ. In the basement someone had punched a hole in one of the walls and set up an electric train and tracks. Among the eligible bachelors there were Henry Stommel, Andrew Bunker, Claude Ronne, and three or four others.

Previous to my arrival, a number of Texans had lived there, and I think they were responsible for painting the kitchen black. At any rate, there was fine camaraderie among all the residents, and there was a lot of shoptalk between them as well as partying and all kinds of pleasantries. I remember one time when Hank spent a whole hour at the organ doing what we see Victor Borge do when he lampoons an opera performance. Hank also did a whole church service with sermon, singing, and organ playing. He was a fun performer.

He was not above playing pranks on the rest of the house. Once, almost everyone was out of the rectory attending some party and the house was dark. Hank went down to the basement and put a light bulb in a fuse socket. When the fellows returned from the party, of course, someone turned on a light switch. Nothing out of the ordinary happened until other lights were turned on. Each time a light went on, the house got progressively dimmer and dimmer, until it was impossible to do anything but grope around in the dark.

The rectory was a fine introduction to the good humor and fellowship of working at the Oceanographic. ❖

Scientific parties are not always enthusiastic about chief scientists, but that night, we were highly regarded by all hands.

When Science Failed

by Starbuck

ON FRIDAYS AT FIVE O'CLOCK Columbus Iselin used to leap aboard the *Risk* to retreat to his farm at Tashmoo, leaving distinguished visitors to fend for themselves. They frequently ended up at the rectory for room and board. The Antarctic oceanographer Sir George Deacon was eating spaghetti in our kitchen on the Saturday that we discovered that the gas that gurgled up from the drain in the sink, when the plumbing upstairs was used, could be ignited in a clear blue flame.

Fearful of a subterranean explosion, we decided to remedy the matter and systematically probed the yard with drill rod to no avail. The resources of science were brought into play. Allyn Vine loaned us an old mine detector, but we didn't seem to be able to make it work. Johnny Holmes devised an electrical potential method, in which a perilously high voltage was applied to the sewer pipe where it left the cellar. A map of the voltage field out back would, theoretically, lead us to the offending pipe or cesspool cover. Somehow it didn't work either.

The desperate measure of digging down to the pipe where it was known to leave the cellar and trying to follow it came to an end when, after twenty feet, it appeared to go straight down. The methods of exact science having failed, we turned to the occult.

A visiting biologist, Dr. Hillary Moore, had often aroused our skepticism with stories of water divining in the west of England. He offered to solve our problem. As we walked toward the rectory past Little Harbor, Hillary broke off a willow branch from a tree in Frank Ryder's yard. This brought Frank out of the house, where a ladies' tea party was in progress. They all followed us to the rectory's backyard, where Hillary paced up and down delicately balancing his willow fork. As we turned on all the faucets and flushed the toilets to stimulate the power of running water, the purer scientists Art Klebba and Andy Bunker retreated upstairs with a bottle of Scotch. When Hillary finally felt the proper twitch, the twig was pointing directly at the center of the concrete garage floor. We expostulated, but old Mrs. Moore spoke up with "I'm sure my son's correct." Then George Clarke's mother discovered, with her cane, an iron cover in the driveway, but it turned out to be an empty rain cistern. The resources of science, both profane and occult, had been exhausted. Even the keen eyes of old ladies had been deceived.

George Griffin, one of the vestrymen, was also water commissioner. He represented therefore the combined power of both Church and State. When he heard of our plight, one of his men with a water-main detector came to the scene. He found the lid in no time flat, behind the garage. We eagerly dug down and, with a bar, cracked it open. There was an immense rush of escaping gas, and our troubles with the netherworld were over. ❖

The Enterprise
March 11, 1985

Gambling Frenzy

by Starbuck

WHAT MUST HAVE BEEN the single greatest night of gambling in Woods Hole history occurred at the Episcopal Rectory nearly forty years ago. Hundreds of millions of dollars changed hands before midnight—and this was in the days before there was even a thought of a state-sanctioned lottery. The minister himself was not directly involved. He was living several houses removed and had rented the old rectory to some Woods Hole bachelors.

Up to the particular night involved, gambling at the rectory had been confined to the modest weekly poker game with Jeff Allen, Bill Schroeder, Art Klebba and a few guests. But on the great night itself the house was crowded, mostly around the roulette wheel in the parlor. There were card games in the second-floor bedrooms and a very serious session of 21 on the third floor. A bank adjoined the bathroom. It was stocked with bills of various denominations, from one to a hundred dollars. A few bills with higher denominations were held by the bank in reserve, and of course the old gelatine hectograph, green ink and extra paper stood ready should more currency be required.

Upon arrival each invitee was issued some of this cash, free, and steered toward the gambling tables. Anticipating that there might be a shortage of low denominations, the bank offered larger ones for small ones at the rate of 10 to 9. Timid souls spent the evening collecting one dollar bills and exchanging them for ten dollar bills—a steady if modest profit.

The pace at roulette was brisk. Dave Barnes, the croupier, insisted upon carrying a thousand dollar bill for emergencies in his back pocket. Gil Oakley, captain of the *Atlantis*, deftly filched it, placed it on a single number, won, and the inflation was on. Rapidly the bets increased; the bank printed bigger bills; the passion of greed mounted, and by one o'clock stakes of millions of dollars were on the table. The faithful few at the 21 table upstairs, including, as I remember, Andy Bunker and Phil Shafer, were playing for a hundred million dollars a hand.

Altogether it was a remarkable evening, perhaps uncomfortably a little too much like real life. The fever of winning, the smell of money, the lure of quick profit, a certain mad recklessness permeated the air. If a toy printing device can evoke such frenzies, think what the U.S. Treasury's Bureau of Engraving is doing to us all. ❖

The Enterprise
March 1, 1985

John Swallow

Institute of Oceanographic Sciences Deacon Laboratory (retired)



Henry was endlessly helpful in solving the problems of working with a small research vessel away from its home base.

Like many people connected with the oceanographic world I enjoyed the privilege of working with Henry on some of the projects that he inspired. Often on visits to Woods Hole I had the chance to listen to him talk and to enjoy his and Chickie's generous hospitality at home.

Most of my contact with Henry was by correspondence. He was a great letter writer, if sometimes a reluctant traveler. He would say, "Look, I could write you a thousand letters or make a hundred telephone calls for the price of a transatlantic air fare."

Soon after I began making the first batch of neutrally buoyant floats, at the end of 1954, he began writing to me about his idea that there might be a deep countercurrent under the Gulf Stream. In a little more than two years, we thought we had found it. That soon led to a 15-month project measuring deep currents from R/V *Aries*, based at Bermuda. It really was Henry's project: He wrote the proposal, got the funds to support it, took part in the seagoing work, and was endlessly helpful in solving the problems of working with a small research vessel away from its home base.

That project raised more questions than it answered. But Henry had plenty of other ideas. He got interested in the Indian Ocean in the early 1960s, and we were both involved in surveying the Somali Current in the southwest monsoon of 1964. That was followed by an exploration of how deep water is formed in the northwest Mediterranean, then the Mid-Ocean Dynamics Experiment in the early 1970s, and finally a return to the variable currents of the equatorial Indian Ocean in the late 1970s.

The last time I saw Henry was in 1983, when he came to London to be made a Foreign Member of the Royal Society. I retired in that year, but Henry's correspondence did not stop; if anything it increased, not always about "work" but usually connected with the sea somehow. He wrote to me sometime last year asking about Admiral Boscawen's expedition to India in 1748. He had come across a report saying that Boscawen had lost 3,000 men from eating poisonous fish near Rodriguez—was it true? As it happened, I had got interested in that expedition for another reason: One of the recommended sailing ship routes from South Africa to India is still called the "Boscawen passage." I wondered why he chose that route, which would seem hazardous if one's longitude were doubtful, as it might well have been in 1748. Regrettably, I didn't find the answer to either question in time to write to Henry. In fact, I still don't know, though I'm still interested.

Both Mary and I miss his letters very much. ❖

Bruce Warren

Woods Hole Oceanographic Institution



In 1941 Albert Defant proposed his reference surface for computing absolute geostrophic currents in the Atlantic Ocean. It had no basis in physics, but it was used in a number of investigations—in part for lack of much else, in part by reason of authority of publication. It charmed Hank. When he visited Defant at his home in Innsbruck, Hank asked him how he had thought up the construction. Defant dismissed it with a wave, calling it, as Hank understood him to say, a *kosmischer Schwank* (cosmic prank).

Hank relished the phrase. His first diagram illustrating his revolutionary ideas about interior flows and western-boundary currents in the deep ocean circulation was published in the famous Letter to the Editors of *Deep-Sea Research* in 1958. Although this figure has been reproduced many times, it incorrectly represents the theory in high latitudes by turning the interior streamlines westward there. One afternoon Hank was in such a relaxed, expansive mood that I thought he might not be offended if I asked him what he had had in mind when he drew the streamlines that way. Only halfway into my question, he guffawed, and said, “Oh, that was a bungled *kosmischer Schwank!*”

After Hank died, Fritz Schott told me that probably what Defant had really said to Hank was *kosmischer Schwung* (spirit, play, verve). It had been a favorite term of his, as in recommending to anyone trying to contour a small number of data points that he get a *kosmischer Schwung* in his hand.

Whatever Defant said, Hank loved pranks, and he carried them off with verve. At the time when Buddhist monks were incinerating themselves in Saigon by way of objecting to the Diem government, Hank brought around a printed “news bulletin” that read something like: “Another Walsh Cottage Mathematician has burned himself to death on Water Street in protest against the harsh, dictatorial policies of President Poo Foo. When asked for comment, Poo Foo said, ‘I’m running a democracy here, and I don’t want any interference.’” Hank wouldn’t let this out of his hands, for fear that Paul Fye might see it.

A prank of a different sort was *The Indian Ocean Bubble*, a very informal newsletter whose five issues were published in 1959 and 1960. The Scientific Committee on Oceanic Research was then planning the International Indian Ocean Expedition, without paying a great deal of attention to possible seagoing participants, and its discussions tended toward showing the flag(s), international cooperation, “socio-economic” puffery, and the like. *The Bubble* sought through editorials and correspondence to chide these folks, to stimulate interest in exploring specific oceanographic phenomena, and in devising practical means for doing so. Even years afterwards Hank was reluctant to admit being the editor, although the typography and literary style were clearly his, and the mischievous twinkle as he evaded questioning was a giveaway. *The Bubble* remains fun to read, but its success was modest, limited perhaps to fostering reconnaissance of the Somali Current and the Equatorial Undercurrent. In concluding the series, the editor explained: “The editor

Hank
loved pranks,
and he
carried
them off
with verve.

has maintained a gently pejorative tone throughout the brief lifetime of this journal—it would have become a clergyman. But when one criticizes too often it tends to take on a petulant ring. Since your editor has essentially a sunny disposition, he has elected to retire from the field—secure in his cloak of anonymity—some day, perhaps, to appear again in a dramatic way (as a Black Knight?) to do battle with Sin.”

Would that he could. ❖

Redwood Wright

Woods Hole, Massachusetts



Like others, I have warm memories of Hank’s playful sense of the ridiculous and his ready willingness to share with a colleague or student his knowledge, insight, and understanding. But I remember best his personal kindness and compassion when I sorely needed both.

Hank was a member of my thesis committee and had expressed admiration for my dissertation. Nevertheless, when we drove to the University of Rhode Island together for my thesis defense, I was nervous, and he knew it. I don’t remember details of our conversation, but it was pleasant and nonscientific to keep my nerves in hand.

Coming back was far different. I had passed, but barely, and had come pretty well unglued in the process, despite the gentle questioning of the committee. Hank knew I was devastated. In the nicest possible way, he reminded me that we can’t all be world-class fluid dynamicists, and that significant contributions can be made by a conscientious observer. He identified several such individuals who had helped build the foundations of physical oceanography and were still working to open up new areas of understanding. He made me realize that sound intuition and honest, careful acquisition and analysis of data are essential in any worthwhile enterprise. He helped restore my spirits, and my perspective. And ever since, in crises not necessarily professional, I have drawn strength and confidence from those wise words of Henry Stommel. ❖

Walter H. Munk

Scripps Institution of Oceanography



Hank has had a greater influence on physical oceanography than any other person of my generation. I met him in 1946 at the New York Academy of Sciences at a symposium on convection, sponsored, I believe, by Carl-Gustav Rossby. Hank had just started his talk when all the lights went out. He was stopped for about two seconds. Then he said, “We can do this just as well in the dark,” and so he did. This paper is the first in the list of Stommel publications collected in *Evolution of Physical Oceanography* (Cambridge: MIT Press, 1981).

Continued on page 27

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LETTER TO THE EDITORS

The abyssal circulation

(Received 18 February, 1958)

In my survey of the theories of ocean currents (*Deep-Sea Res.*, 1957, 4, 149-184) several schematic interpretations of ocean circulatory patterns are presented. In this letter I wish to show how, using the same principles, it is possible to sketch in broad outline the flow pattern for the abyssal circulation of the world ocean.

It seems likely that the low temperature of deep waters in the world ocean is maintained in the face of downward diffusion of heat from the warm surface layers by a very slow upward component of velocity in the deep water. An adequate theory of the thermocline would, presumably, deduce this upward velocity as a function of surface heating, turbulence parameters, etc. We might regard the thermocline as a "pumping mechanism" which slowly draws up deep water and hence actually determines the rate of flow of the abyssal circulation. An estimate of the maximum upward component of velocity under the thermocline, w_{\max} , is given in terms of the depth of the thermocline, z_t , by the equation.

$$w_{\max} = \frac{\sqrt{2}}{4} \frac{\beta g \alpha l}{f^2} z_t^2 \theta_0.$$

This equation is obtained from a very primitive model* of convection on a beta-plane and contains neither the turbulence parameter nor the artificial basic stability explicitly. Both enter implicitly, of

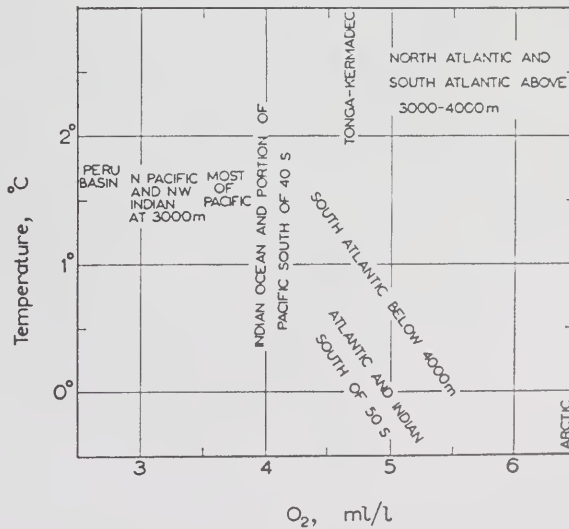


Fig. 1.

course, through the very structure of the model; both are basically objectionable, are introduced to circumvent our lack of physical knowledge about turbulence and our inability to handle the non-

*HENRY STOMMEL and GEORGE VERONIS (1957) *Tellus* 9, 401-407, to which the reader is referred for definitions of quantities: the above relation being derived by substituting $Gz_t = 1$ into equation (20).

linearity of the energy equation respectively. Consequently our conclusions are limited, at best, to order of magnitude considerations. If we take as representative oceanic averages

$$\beta = 2 \times 10^{-13} \text{ cm}^{-1} \text{ sec}^{-1}$$

$$g = 10^3 \text{ cm sec}^{-2}$$

$$\alpha = 2 \times 10^{-4} \text{ } ^\circ\text{C}^{-1}$$

$$\theta_0 = 10^\circ\text{C}$$

$$f = 10^{-4} \text{ sec}^{-1}$$

$$l = 5 \times 10^{-9} \text{ cm}^{-1}$$

$$z_l = 2 \cdot 10^4 \text{ cm}$$

we find that w_{max} is of the order of $3 \times 10^{-5} \text{ cm sec}^{-1}$. Since the area of the ocean at 2000 m is about $3 \times 10^{14} \text{ m}^2$, the total upward flux of water across the 2,000 m level over the entire world ocean is of the order $90 \times 10^6 \text{ m}^3 \text{ sec}^{-1}$, about twice the maximum estimated by analysis of hydrographic data. The slow upward flow over most of the ocean must be compensated for somewhere by downward flowing sources of deep water. I envisage these deep-water sources as occurring in very limited areas of weak gravitational stability where the thermocline "springs a leak." The location of these sources is apparently very sensitive to climatic factors; we determine it by consideration of the distribution of dissolved oxygen in the ocean below 3,000 m (Fig. 1). Evidently the two sources are in the North Atlantic, and in the Weddell Sea (heavy black circles in Fig. 2); there is no source



Fig. 2.

in the Arctic regions of the Pacific Ocean. The simple beta-plane convective model also assures us that the wind has no effect on the water below 2,000 m. Therefore, we have a distributed "sink" more or less over the entire ocean below 2,000 m acting on the water below; and two point "sources." To complete the picture of the thermal circulation in the ocean we must connect the distributed sink

to the sources in a way consistent with the dynamics of the fluid on a beta-plane or rotating sphere.

The streamlines in Fig. 2 are sketched following the modified form of Goldsbrough's method for the rotating sphere as described in my survey article. Because of the distributed "sink" the flow below 2,000 m is everywhere horizontally convergent, hence the meridional component of velocity below 2,000 m must everywhere in the interior of the ocean be directed away from the equator ($\beta v = -f \operatorname{div}_h \mathbf{v}$): except at the equator itself where it must vanish. Having fixed the meridional component and vertical component, the zonal component is determined by the continuity equation working westward from the eastern coasts. At the western coasts intense boundary currents are introduced as necessary to connect with the sources. The chief ambiguity is how much water to admit through Drake Passage; the Antarctic Circumpolar Current Transport is fixed however. Summarizing the features of flow deduced in Fig. 2: If the two sources each have strength $20 (\times 10^6 \text{ m}^3/\text{sec})$, then the western boundary currents are: western North Atlantic, 24 southward; western South Indian, 14 northward; western South Pacific, 30 northward; western North Pacific, less than 10, directed toward 30°N . latitude circle. The zonal transport of the Antarctic Circumpolar Current (below 2,000 m) is 50 across 55°S . latitude circle. In the interior of all oceans at mid-latitudes below 2,000 m the mean meridional component of velocity is of the order of 0.03 cm sec^{-1} . The value of w_{\max} is $1.5 \times 10^{-5} \text{ cm sec}^{-1}$.

The above presentation is in the nature of a *tour-de-force*. One cannot pretend that it describes the the abyssal circulation accurately in detail.

Woods Hole, Mass., U.S.A.

HENRY STOMMEL

Contribution No. 953 from The Woods Hole Oceanographic Institution.

❖ —Walter H. Munk Continued from page 24—❖

I had by then read Hank's book *Science of the Seven Seas*, which he disowned, but which I rather liked. Evidently Hank at one time thought of enrolling at Scripps, but was discouraged from doing so because he believed that Sverdrup thought the book was awful; I am not aware of any evidence this was so. In fact, when Hank visited Scripps in the summer of 1949, he was offered a job by Roger Revelle (who had just been brought back by Sverdrup as Associate Director under Carl Eckart), Scripps thus becoming one of the long list of places that have tried to recruit Hank. I don't believe Hank ever took seriously any offer from farther west than Massachusetts.

Roger's admiration for Hank goes back to this visit. Some years later Hank was to write Roger a wonderful warm letter which Roger recalled to me with great pleasure just before his death. (I have searched in vain for this letter.) In March 1988, Roger wrote Hank shortly after Paul Fye's death: "One by one the armies fade away. I hope you last for a long time, and I am going to try to do likewise." On June 10, 1991, Hank wrote Roger about the planned Revelle visit to Woods Hole from July 14 to 19: "Let's get together for a pleasant dinner or two at our house when you are here. We'd love to see you," to which Roger replied on June 21: "One of the good reasons for visiting Woods Hole is to see the two of you." Roger died on July 15, and now they are both gone.

The special relationship between Roger and Henry is a curious one, since Roger accepted authority with ease and took pleasure in organizing things. Hank, on the other hand, avoided responsibility and anything

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and dynamics
than any
other scientist.*

that smacked of organization. But not quite. I remember sitting on the lawn at the Bermuda Biological Station listening to a discussion of how to proceed with the Mid-Ocean Dynamics Experiment (MODE). The talk was going nowhere until Henry took the chalk and outlined on an outdoor blackboard which vessel was to go where and when.

During Hank's 1949 visit in La Jolla, he wrote "The Gulf Stream" for *The Scientific Monthly*. We climbed Mount San Gorgonio together. From then on our paths crossed on so many occasions that I cannot possibly sort them out.

Hank and I collaborated (with Bernard Haurwitz) on a spectral analysis of 10 years of Bermuda offshore temperature data for the *Rossby Memorial Volume* (1959). Hank sent one of his better MIT students, Carl Wunsch, to work with me during winter of 1965 to 1966. And we both made sage comments about the future of numerical ocean models ("Where Do We Go From Here?") at a symposium in Durham, New Hampshire, in 1975. I attended Hank's 60th celebration in Woods Hole in 1980, and he attended my 65th in La Jolla in 1982, where he told a hilarious story about a beautiful girl in Venice standing alone under a Tintoretto (see opposite page).

I had the pleasure of presenting to Hank the citation for the 1982 Bowie Award. As is the custom, I had submitted the citation to the American Geophysical Union (AGU) prior to the ceremony, and the AGU had forwarded a copy to Hank. We sat next to each other on the podium. When I returned from reading the citation and it was Hank's turn to respond, he whispered to me that he had not read the citation ahead of time, as he wanted to be uninfluenced in his remarks.

On the many subjects Hank wrote about, he certainly did not follow any footsteps but his own. If there is a connecting theme in Hank's broad interests, it is the concept of conservation of potential vorticity. Robert Stewart (the Canadian) once wrote: "...Stommel is the first person working in Geophysical Fluid Dynamics to really understand vorticity well enough that it became a reliable part of his intuition." And this intuition has determined the flavor of physical oceanography over the last few decades.

Although I knew Hank for many years, I do not consider myself as one of his closest friends. But I admire him tremendously. There was something magical about his person, and I looked forward to every occasion of being with him with deep pleasure. ❖

Allan Robinson

Harvard University



As a young theoretical physicist in the late 1950s, I began to read and think about ocean dynamics. I first came into contact with Henry Stommel's intellect through a preprint of his "A Survey of Ocean Current Theory." It was immediately apparent that the work and inherent ideas reviewed represented a stimulating, profound, and novel dynamical basis for ocean circulation studies. Soon thereafter, when I first met Hank himself, he informed me that it was unlikely that the field could use a person of my background.

An Excerpt from Hank Stommel's Remarks in Celebration of Walter Munk's 65th Birthday

Walter has been number one for all these years, and whether we want to or not all of us compare ourselves to him, at one time or another. When I was called to the chair of oceanography at Harvard, it was only after Walter had declined it.

Of course, I am not the only victim of Walter's preeminence. It has happened to others before, and will happen to more of us in the future.

Let us imagine the following scene: You have been invited to an ambassadorial reception in Venice. Arriving in a gondola by torchlight you step into a grand palazzo between the striped posts at its watergate. Perhaps you are the guest of honor, and an oceanographer of great distinction. Perhaps you are Klaus Hasselmann. You make your way amongst the elegant guests, shaking hands, introducing yourself, with assurance and grace. The main salon is crowded with the elite of the Italian academic world; the crystal chandeliers are ablaze, the champagne flows. Your eye catches the sight of a beautiful girl standing alone under a Tintoretto—a vision of loveliness. Unconsciously you slowly make your way in her direction, smiling, and exchanging greetings with old acquaintances all the while. The orchestra strikes up a waltz by Strauss. You approach her, bow politely, ask her for this dance. As she extends her hand, her eyes melt and she says: "You're Walter Munk aren't you?" ❖

—from *A Celebration in Geophysics and Oceanography—1982*
Scripps Institution of Oceanography Reference Series 84-5

However, several months later we were deeply involved in a collaboration on one of the first theories of the ocean's main thermocline. Typically, Hank had identified thermocline theory as an essential and critical element required for an understanding of the thermohaline circulation and was relentlessly pursuing it, although the research issues then stood at the edge of feasibility. This collaboration initiated a period of over 30 years of scientific and personal interaction with Henry that I value most highly.

In my opinion, Henry Stommel has contributed more to our conceptual understanding of ocean circulation and dynamics than any other scientist. He had the ability and interest to explore phenomena ranging from microstructure to global scales. He had a unique and canny skill of focusing on, exposing, and simply modeling essential processes. As a complete scientist, he still would remain frustrated at his inability to have deeper and further insights and to develop ideas more quickly. Hank's mind was extremely active, his energy legendary, and his interests very broad. The breadth of his interests and his ability to cut to the quick of matters made him a fascinating conversationalist and a most stimulating friend. Evening discussions of current affairs, politics, history, aesthetics, and morals, perhaps with Havana cigars, provided priceless memories.

Hank's personality had its dramatic side, which could range from the ridiculous to the sublime. While explaining to me at his home about a book review he was preparing, he invited me to accompany him to the town dump, where he tossed the volume as his final comment. On the

starting day of an important founding meeting for the Mid-Ocean Dynamics Experiment in London in 1970, he awakened me very early in the morning and without explanation we walked to Westminster Abbey and stood silently at Newton's crypt.

I remember Henry Stommel as a truly exceptional human being and an extraordinary scientist. ❖

Editor's Note: Allan Robinson and Henry Stommel co-directed the Mid-Ocean Dynamics Experiment and were co-chairmen of the US POLYMODE Organizing Committee.

Alan J. Faller

University of Maryland (retired)

❖ ❖ ❖

What an extraordinary time it was in Woods Hole—a time when WHOI was in the forefront of the newly developing field of Geophysical Fluid Dynamics. And at the nucleus of the group of independent scientists in the endeavor, although not always the most conspicuous, was Henry Stommel. My tenure at WHOI was from 1954 to 1963, with a year off at the University of Chicago and a year in Stockholm. Here I will attempt to relate some of my experiences and impressions of those years that are relevant to this issue of *Oceanus*. My recollections may differ slightly from those of others, and I beg your indulgence in this respect.

Although one may argue for earlier origins (in the works of Bjerknes, Rossby, and many others), Geophysical Fluid Dynamics began to become a somewhat organized and sustained discipline when it was recognized in the early 1950s that the independent laboratory studies of Raymond Hide at Newcastle (originally studies of the interior of the earth), Fultz and Long at the University of Chicago (atmospheric simulation), and William von Arx at WHOI (oceanic simulation) had many common features. Von Arx's experiments at WHOI had been preceded by rotating tank experiments by Spilhaus in the mid-1930s and, indeed, I was told (but have no record) that Henry Stommel had worked with rotating models of the ocean circulation prior to those of von Arx, in the same lab and perhaps with the apparatus developed by Spilhaus. Those early rotating tank studies, including some of my own (through 1960), took place in the basement of the Bigelow Building, in a northwest corner lab that was shared with biologists growing phytoplankton and lobsters, across the hall from the carpenter's shop, and beneath the director's first-floor office.

But the development of GFD began in earnest in the mid-1950s when Stommel and Malkus spearheaded a joint seminar between WHOI and MIT called the "Fortnightly Seminar in Thermal Convection." As a sample of the attendees, regular participants from MIT included Jule Charney, Norm Phillips, Lou Howard, and others, with occasional participation from Alar Toomre, Ed Lorenz, and other meteorologists and mathematicians. Regulars from WHOI in addition to Stommel and Malkus included George Veronis, Melvin Stern, myself, Peter Saunders, Joanne Malkus, Eric Kraus, and others, as well as visitors. Allan Robinson came from Harvard, while Bill Reed and others came from Brown. This seminar eventually expanded with visits to

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Trial By Gutenberg

by Starbuck

YEARS AGO IN A TIME before the flood of junk mail, when I was a bachelor living at the Episcopal Rectory in Woods Hole, I decided to answer all the classified ads in *Popular Mechanics* magazine. For months afterwards there was a gratifying deluge of sales material offering all sorts of bargains. The most persistent of all was the Paw Paw Chemical Toilet Company—which I finally managed to cut off by returning one of their missives marked “deceased.”

I did, however, succumb to the blandishments of the Kelsey Printing Company, Meriden, Connecticut, whose ad titled “Make Money, Do Your Own Printing” was so alluring that I ordered their 6 by 10 Excelsior press, several fonts of 8 point Bodoni, a small font of 24 point Clarendon and enough Old English for a masthead. This truly venerable firm has used the same enticing advertisement since at least 1876; it can be seen in the program of the Philadelphia Exposition in the possession of the Sturgis Library.

At first my job printing activity was confined to calling cards, Christmas cards and the programs for Mary Fassett’s Falmouth Friends of Music. The latter was the cause of my press’s demise, since it exhausted my supply of upper case “M’s” in setting up the list of patrons with their repetitive Mr. and Mrs.

The most ambitious project using the Excelsior was the weekly publication of *The Woods Hole Adjudicator*. It was motivated by differences of editorial opinion with *The Enterprise*. The labor of setting up type and then distributing it by hand was very discouraging and led me to curse Mr. Gutenberg’s invention of moveable type. The profits at 1 cent a copy were disappointing. After a few issues—none seem to survive—*The Adjudicator* collapsed, bankrupt, so to speak.

Nearly forty years have passed—years of repressed commentarial ambition. The rollers on the old Excelsior sag with mold. The tub of printers’ ink is hard as a rock. There is a better way. If you can’t beat them, join them. ❖

The Enterprise
October 31, 1984

...but in Stommel's office the pace was markedly accelerated.

Harvard, Brown, and the University of Rhode Island, and the GFD summer program at WHOI is in part an outgrowth of the collaborations developed in that seminar series.

The freedom of exchange of views among this group was remarkable. In particular I call your attention to the number of scientists with whom Henry Stommel collaborated, as shown in part by his list of 70-odd joint publications. I suspect that in most cases Stommel was the prime mover, but of this I cannot be certain. In the paper of Stommel, Arons, and Faller (1958), however, I can speak with authority, for the text was written in three sections by the three authors. The beautiful introduction giving the motivation and philosophy came from Stommel himself; the detailed analysis and theory was provided by Arnold Arons; and I was enlisted to develop, analyze, and write about the experiments, guided by Stommel and Arons.

On the other hand, there were many cases in which Stommel played a subsidiary but catalytic role, and these are well exemplified in the story of double diffusion and salt fingers. My narrative may differ in detail from the recollections of others who were present at the occasion, but there should be little difference in substance.

In the spring of 1960, Stommel had an office on the third floor of Bigelow that he was sharing with Bert Bolin, visiting from Stockholm. I believe they were collaborating on box models of the ocean to understand the movement of radioactive wastes and carbon dioxide. Willem Malkus had a nearby office, my room was a garret shared with Ferris Webster at one end of the Bigelow attic, and George Veronis was at the other end. Melvin Stern was on the second floor, close to the offices and lab of Al Woodcock and Duncan Blanchard. It was a Friday morning and we were all scheduled to leave that afternoon for a Thermal Convection seminar at MIT.

I was across the hall from Stommel and Bolin, waiting for the 10 o'clock coffee that we all shared in Andy Bunker's lab, when Melvin Stern breezed into Stommel's office in an excited state that was unusual for the pipe-smoking, contemplative Mel. Animated voices drifted across the hall, Willem Malkus dropped in, the decibel level rose, and I went across the hall to investigate the excitement of Stern, Stommel, Bolin, and Malkus. As Stern talked, I quickly grasped that he had quite independently developed a new theory, that of double-diffusive convection. He had then realized the relation of this presumed natural phenomenon to the "Perpetual Salt Fountain" of Stommel, Arons, and Blanchard, and had come to get Stommel's opinion.

You may well ask how one would come to conceive of the double-diffusive phenomenon in the first place, but this is not hard to understand in the context of the related studies then taking place within our group. Veronis and Malkus, in particular, were deeply concerned with theories of the onset of convection in which thermal diffusivity and kinematic viscosity, differing by an order of magnitude, play leading

roles. It is not surprising then that some inquisitive scientist concerned with oceanography should ask about the additional effect of the relatively low diffusivity of salt when both thermal and salinity gradients are present. But it is one thing to inquire and quite another to carry through all of the detailed mathematical manipulations, covering all possible physical circumstances, to discover the various types of double-diffusive phenomena that may occur, as did Melvin Stern.

Based on Stern's concepts and equations, Malkus went to the chalkboard and in about two minutes produced an estimate of the scale of the proposed phenomenon: less than 1 centimeter. Stommel suggested that we do an experiment, whereupon I produced a 1,000-milliliter graduated cylinder and some potassium permanganate (as both a salt and a colorful tracer). We prepared a warm solution of the salt, of lesser specific gravity than the cool tap water that now half filled the graduate, and we carefully poured the solution on top. Voila! In a few seconds, descending fingers of salty solution appeared as Stern had predicted, about 5 millimeters in diameter. These estimates and experiments, of course, would have been done by Stern himself in due time, but in Stommel's office the pace was markedly accelerated.

Arriving at MIT that afternoon enthusiastic over the new discovery, we all discussed Stern's theory and its potential with our colleagues at the seminar. What would be the consequences of thermohaline convection and salt fingering? In those major ocean basins where the salinity was higher near the surface, what was holding the salt up against this new convective phenomenon, etc., etc.?

But that evening during the two-hour return trip to Woods Hole, we gradually became aware that we had all spoken out of turn. Stern was visibly upset by the publicity afforded his as-yet unpublished, unwritten, and, indeed, incomplete theory, and rightly so. When we fully realized the seriousness of this breach of professional etiquette, it was suggested that we should all cease and desist from any further work on the subject or public discussion of the theory until the originator of the concept and the theory, Melvin Stern, could complete his work and publish the results. To this we all somewhat shamefacedly but enthusiastically agreed. All's well than ends well. The rest is history ["The 'Salt Fountain' and Thermohaline Convection," *Tellus*, 1960, pages 172–175]. But our experience suggested some pitfalls in the free interchange of ideas that we so cherished.

Upon returning to WHOI in the fall of 1991 as a guest investigator (after 28 years absence), I was astonished to find the same Henry Stommel. It was as though nothing had changed. Each Tuesday when I saw him he invited me into his office to tell me about his new theory of the temperature-salinity relation in the North Atlantic, or to show me his Coriolis machine, or to find out what I was doing.

And when he passed away my immediate thought was—but here was a man who should never have died. ❖

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Edward Spiegel
Columbia University



My most respected teacher, G.E. Uhlenbeck, used to say that to understand a thing in physics you had to feel it with your fingers and your toes. It was only after I finished my thesis and came to spend a month in Woods Hole in the summer of 1958 that I really learned what he meant. This time, my teacher was Henry Stommel.

I was assigned to share an office next to Henry's in the Smith Building with three other visitors. All of them were senior to me, so I had to use the windowsill for a desk. My office mates did not seem unhappy with the arrangement, and neither was I. It was wonderful to look out on the water while working, and things went well for me there.

One day, Henry came by carrying a large cylindrical container and, with a hint of a grin, invited me to accompany him down the hall. I fol-

lowed him into the men's room where he emptied the contents of the cylinder into the toilet and flushed it. Then he led me back to my "desk"

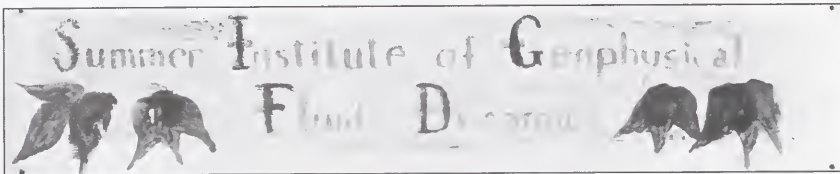
where he stood expectantly for a couple of moments. Suddenly, almost right under us, a lurid green stain appeared at the water surface and began slowly spreading. Henry took great pleasure in the look of astonishment that came over my face and rewarded me with one of his infectious laughs. For my part, after that I was quite choosy about where I went swimming.

During that month, Henry spent a lot of time standing over his drawing board working on a pastel picture of the earth as seen from space. He had become absorbed with the importance of bottom topography for ocean circulation, so he set about making a picture that would give him a good feel for what the ocean floor is like. I would go into his office frequently to watch the portrait evolve.

The picture showed the eastern Atlantic basin and the coast of Europe. Henry left the water out of the Atlantic so that the seafloor could be seen clearly. To emphasize its contours, he drew the topography of the seamounts and even of the mountains visible in southern Europe with a vertical exaggeration of five. Those were some pretty spiky mountains, and you could almost feel the roughness of the earth's surface as you looked at this picture. For an added bit of realism, Henry had added a large moon hanging over Europe.

A couple of days before I left for my new job in Berkeley, I passed by Henry's open door and saw him dismantling his topography project. He said he was done with it. In fact, the process of drawing it with loving care was all he had needed to get the bottom topography hardwired into his system. I was startled at the thought that he was going to throw the picture out and said so. I thought he was a little pleased by my reaction, but as he always seemed pleased with things, I could not be sure.

Terr Corbett



Hank Stommel lettered and decorated this sign for Walsh Cottage, the home of the summer Geophysical Fluid Dynamics program at Woods Hole. The foliage is poison ivy!

Henry told me I could have the picture if I wanted, so I asked him to sign it and happily carried off this memento of my summer working next door to him. I got the picture framed at what was then a big price for a young instructor. Since then, it has always occupied a place of honor on our living room wall.

Whenever I look at Henry's creation, I am reminded of some lectures that the painter Ben Shahn gave at Harvard about the meaning of art. What he was talking about, I think, is how you try to render something you know, or at least feel, in art, and mainly painting. It was an articulate, even moving, set of remarks that was published in a book called *The Shape of Content*. I have always been interested in the analogous problem of how we can turn the renderings of Nature into something that we can know—or at least feel. But when I look at the picture Henry gave me all those years ago, I realize that there is not much one can add to that simple statement about scientific understanding. ❖

Willem Malkus

Massachusetts Institute of Technology



Hank Stommel designed the dragon seal for the just-established Geophysical Fluid Dynamics (GFD) summer program in 1960. Although Hank usually was focused on his current oceanographic challenge, he lent his invaluable support to the idea that Woods Hole could be a seasonal center of studies for all fluid dynamics in rotating systems. His breadth of interest included the interrelated theoretical progress in meteorology, astrophysical flows, and dynamo theory. At the end of the 1950s and in the early 1960s, changing emphasis at the Institution led Hank to report (in a cartoon form) the self-immolation of two visiting GFD professors to protest a reduced concern for scientific matters. Yet, his poisonous sign, still hanging by the door of Walsh Cottage, reflected an alternate concern that the scientific debates were often too acidic. His wit and his wisdom saw the dragon through its early years, and Hank will be sorely missed. ❖

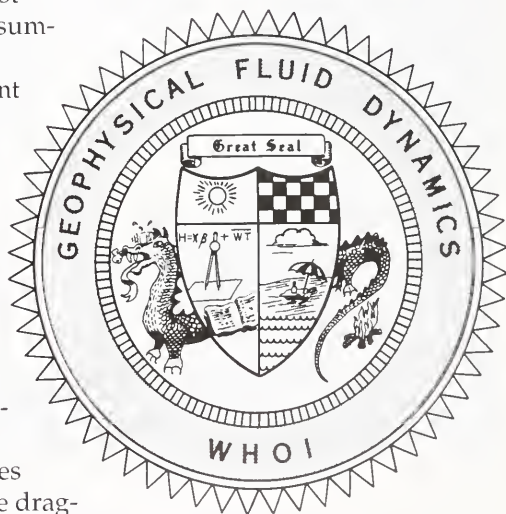
Lou Howard

Florida State University



While I met Henry Stommel about 1956 and knew him fairly well thereafter, I never had occasion to collaborate with him on a scientific paper. Perhaps this reflects Hank's clear perception of the really quite limited role of mathematics in oceanography; what mathematics was appropriate to most of his studies he could readily supply himself. Indeed, I have the impression that for him mathematics was somewhat

Hank Stommel applied his sense of humor to the design of a logo for the Geophysical Fluid Dynamics summer program. Note the cold head and warm tail of the dragon, depicting the driving forces for convective currents.



like a foreign language that he knew rather well: He didn't really think in mathematical terms, but could express himself mathematically if he thought it desirable to fit in with contemporary styles. It is salutary for mathematicians (like me) to recognize that a penetrating physical insight such as Stommel had makes it unimportant whether or not the mathematics is even completely correct—a slip in translation does not necessarily invalidate the original thought.

However, we did have some collaborations, such as our joint effort to keep the rotating table on the floor (see photograph). Another occurred aboard *Atlantis II* on a January trip from Woods Hole to Barbados—this was not the only occasion on which one of my oceanographer friends has attempted to show me that $z = 0$ is not a realistic description of the sea surface, but it was a memorable one, in part because of a severe storm encountered just after leaving Woods Hole. It was on this trip that we attempted to demonstrate the “perpetual salt fountain,” in the oceanic context envisioned in Stommel and Arons's original theoretical paper.

About a kilometer of plastic tubing with a couple of sash weights on

the bottom end was lowered into the sea not too far from Barbados. On the top was a float supporting a fountainhead constructed (by me—mathematicians can be of some use in oceanography) from a length of copper pipe with a perforated penny soldered over one end. To speed up the starting process, the tube was initially filled with fresh water from the ship's supply. This, of course, produced a tremendous fountain, the head difference at the start being some tens of meters; when the fresh water had been expelled, the fountainhead was attached, and set loose on the surface. This experiment was not entirely con-

clusive. There was indeed a persistent fountain, but it pulsed with the period of the surface waves, probably because longitudinal stretching of the tube was accompanied by a contraction in its diameter with a net decrease in volume. However, we convinced ourselves that we were observing not just a wave pump, but also a true salt fountain. In this case, the “perpetual” salt fountain lasted a couple of hours before the experiment was terminated.

A more serious experiment planned for this voyage was one of the early attempts to observe salt fingers in the ocean. A somewhat elaborate apparatus, constructed at MIT, had an intake probe in front and a large (and very elegant) aluminum propeller in back. This was to be towed by the ship—at a considerable depth—on a cable. The propeller was to drive a pump that sucked water into the probe to have its salinity measured electromechanically (thus avoiding the need to supply power for the



Hank Stommel and Lou Howard engage in some close-up rotating-table work in the basement of Walsh Cottage.

pump via the cable). As this device was put over the side, the sea surface again refused to stay at $z = 0$, and a large wave smashed the propeller, the probe, and some other delicate parts. This sort of thing is, I suppose, a normal part of oceanographic observation, but at the time it was pretty depressing, especially to the graduate student whose apparatus it was. Hank and I spent that evening in the ship's lab constructing a new propeller out of plywood and other odds and ends we found, and as I recall stayed up much of the night encouraging the graduate student, who was the only capable glass blower and the only one who really understood the instrument, as he repaired the rest of it. It was successfully launched the next day—but of course, there is still considerable doubt about the real nature of the salt finger process in the ocean. ❖

James Crease

IOS Deacon Laboratory (UK) and University of Delaware



What is most vivid to me about Hank is probably shared with everyone else—his generosity of spirit. I had come up from the *Aries* in Bermuda for a few weeks in summer 1959, I guess, and was sitting in on some early GFD lectures and ones by Kraichnan. It was a pleasant change from going to sea. Hank welcomed me into what at the time seemed to be his big office, full of people yet with still room for one more. Arnold Arons was at one desk. I am told now that it wasn't that big a room—it just seemed that way. ❖

Sloat Hodgson

Woods Hole Oceanographic Institution (retired)



Henry was a bit of a pyromaniac, along with the rest of his many talents, and as creative in this field as all others.

One day he sauntered into a lab at the Bermuda Biological Station. "Did you ever do this?" he asked, whereupon he opened one of the gas jets on the wall and put a match to it. A raw jet of fire shot 6 or 8 feet into the room. Henry viewed it with delight before turning it off.

On another day he came into the lab smoking a cigar. His eyes fell on a nearby bottle of oxygen. "Look," he said, and held the cigar butt against the bottle's outlet, then turned on the valve. A high-quality Roman candle could not have produced a better display.

I had a large Bunsen burner in a box to protect it from the wind on the porch of the lab overlooking Bermuda's Ferry Reach. A length of tubing led from inside the lab out through a window. The burner was adjusted to a low flame. I would turn on the valve inside the lab and walk out and light the burner. It accepted the flame from the match peacefully. One day I went through the lighting process and got rather more flame than I had bargained for. In fact I went over backwards as flame erupted from the box. Henry had opened the valve in the burner fully. Though startled out of my wits, I was uninjured. But you just couldn't get mad at Henry.

*Henry was
a bit of a
pyromaniac,
along with
the rest of his
many talents,
and as
creative in
this field as
all others.*

My Friend John Moore

by Starbuck

ONE FOURTH OF JULY, some years ago, I invited the neighbors to a homemade fireworks display: Giff and Win Ewing, Sue Sisson and Claude Ronne. John C. Moore came, too, from across the street, although, as he said afterward, he didn't think he was going to see much.

The pyrotechnical cardboard tubes were nailed to boards and sawhorses in the old apple orchard—all ready to go. When the first fountain bloomed it spread to the quickmatch on all the other devices, and the entire display erupted in one simultaneous convulsion of strontium-tinted fire. A dense magnesium-oxide smoke drifted across Sippewissett Corner. Giff fell over backwards in his chair. Globules of molten saltpeter ignited the grass. The garden hose was brought into play, and John Moore exclaimed that it was the best fireworks show he had ever seen.

Descendant of one of the first settlers in Sippewissett, John Moore was born in 1884. Qualifying as a marine engineer, he was employed first in the harbor works at Galveston that followed the great flood of 1900 and then on the two-mile-long west breakwater of the Panama Canal at Limón.

Malaria brought him back to the family homestead to recuperate, but shortly he found himself working for an insurance company inspecting factory boilers all over New England. Soon he was back on the Cape again, this time with the water department, of which he was superintendent from 1927 to 1946. When his brother Ed died he inherited the old family place; when his wife Laura died in 1961 he moved there.

John was one of the best-known men in town, and one of the best liked. Although he lived alone, his days were lightened by the visits of many an elderly lady friend from West Falmouth. Most afternoons there was an automobile in his driveway. He had an open, friendly, boyish manner, an impish sense of humor, already apparent in the family photos of the mid 1890's.

You might find him out in the backyard toolshed, by the wood stove from the Davisville school, deep in conversation with Charlie White, Clarence Anderson or Robert Kahler. His house still shows traces of his eccentric experiments in plumbing and heating arrangements. Some of his old pipe wrenches and home-made power tools that escaped the auctioneer's hammer are still there.

At 81, John was too frail to survive his colostomy. He was fretful over the tubes that tied him to his hospital bed and anxious to be home again. And I could see in his youthful eyes the utter disbelief of that inner boy of 10 that he was about to die. ❖

The Enterprise
March 29, 1985

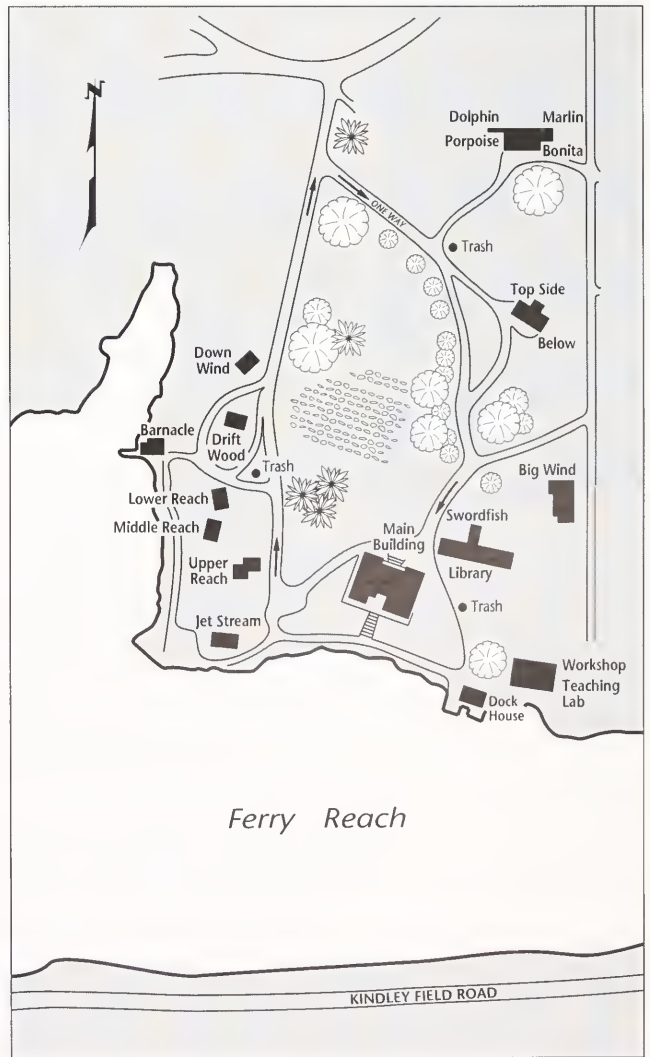
One night in Bermuda during a storm, the electricity in the area went off and all was black. But, lo, after a while a single reading lamp burned brightly. By its light, with great nonchalance, Henry was reading, obviously enjoying our puzzlement. He had gone down to the lab and hauled back 110 volts worth of dry batteries used in our buoy project, and wired them to the lamp.

We went to Bermuda to study surface currents near the island with free-floating buoys. At timed intervals they reported to the lab via their radio transmitters. We located them with cross bearings gained from portable radio direction finders. One day I was sent in a beaten-up Jeep to Wreck Beach, not far from the US naval station. My first job was to zero in on a broadcast from the lab. Shortly before, I had received a package addressed to "Slot Hangsow." This name amused Henry enormously. At the appointed time I tuned the radio direction finder into the lab transmitter to get my reference point. Much to my amazement, floating over the airwaves came a repetitive phrase in a faked Chinese accent: "Calling Slot Hangsow! Calling Slot Hangsow! Calling Slot Hangsow!"

Henry had made a tape loop and was feeding it into the transmitter. All I could think of was being found by a member of the US Navy shore patrol and being put in the brig as a spy with a dubious explanation for his activity!

Henry had a magical effect on others that made people in all walks accept, like, and respect him. I saw it happen with cool, smooth, tough Captain Beaden, skipper of the cable repair ship *Lord Kelvin*. (The Captain looked like a Virginia country squire, had copies of James Stephen's *Crock of Gold* and Lewis Carroll's *Alice in Wonderland* in his bookcase, and refused to answer sensitive questions. His steward turned pale and shook when the captain was displeased with him.) The cable and wireless people in Bermuda gathered around Henry to discuss problems with him and gave him every courtesy, including the use of their property in Tuckerstown. In Long Island, New York, the cable people were fascinated with him. They plunged into an experiment on their transatlantic cable system that ended in knocking the cable temporarily out of commission. When Henry apologized, they shrugged it off, saying "It's only the line from the Pentagon to Berlin. All they ever use it for is to send the baseball scores." ❖

Plan of the Bermuda Biological Station about 1960. The Stommels were staying in Upper Reach cottage during a hurricane in 1953 (see Nick Fofonoff's reminiscence).



Nick Fofonoff

Woods Hole Oceanographic Institution (retired)



One of my recurring memories of Henry Stommel dates back to my graduate student days while I was at Brown University studying under Professors George Carrier and Raymond Montgomery. During the summer of 1953, Henry invited me and another summer student at WHOI, Gunther Wertheim, to accompany him on an R/V *Atlantis* cruise to Bermuda. He planned to track drifting buoys around Bermuda by triangulation with radio direction finders. We were to carry out a hydrographic survey around the island and return to WHOI.

While the ship was docked in St. George's harbor, Gunther and I rented motorbikes for a tour of Bermuda. We spent a pleasant day cruising the length of the island, paying no heed to a hurricane warning issued that morning. Upon returning to St. George we discovered that the ship had been moved from the dock and was tied to a buoy in the harbor several hundred meters from shore. By this time the wind was blowing sufficiently strongly to prevent us reaching the ship by boat. We departed to seek shelter at the Bermuda Biological Station. Henry and his wife, Elizabeth, were staying in a nearby cottage.

During the peak of the hurricane that evening, we went from room to room in the main building checking rooms and windows for damage. It took two of us to push open some of the doors on the upwind side of the building because of the air pressure inside. We were surprised to see Henry appear. He had walked in the dark from his cottage to check on us. After we reassured him that we were safe and sound, he went back to his cottage. We were somewhat disturbed that he had ventured out into the hurricane.

The next morning Henry told us that he had been puzzled by thumping noises around him as he walked to and from the main building the previous night. In daylight he was shocked to find out that the thumps were caused by heavy brick tiles being blown off the building's roof and hitting the ground with such force as to be heard above the roar of the wind. The tiles were scattered on and around the path to the cottage that he had walked the night before.

Now, nearly four decades later as we mourn his passing, I still think of what might have happened on that frightful night and am thankful. ❖

Robert Walden

Woods Hole Oceanographic Institution (retired)



The Henry Stommel I remember was witty, always cheerful, and ever so keen. Hank's infectious humor and zest for life and work touched all of us who worked with him.

One particular incident stands out in my memory. We were at the Bermuda Biological Station in 1954 to measure ocean circulation around the island. Water-temperature and current-speed measurements were obtained from drifting telemetering buoys, probably one of

During the peak of the hurricane, he had walked in the dark from his cottage to check on us.

the first successful uses of radio telemetry of oceanographic data. Many weeks were spent plotting the buoy positions, rescuing those that were stranded, preparing and launching new buoys, and monitoring their transmissions. After long days (and nights) of this schedule, Hank would invariably boost our morale with his humor and pranks, like the evening he built a huge slingshot and strung it between two posts at the lower lab facing Ferry Reach. The first rock projectile soared into the air and landed near the road on the other side of the reach, much to everyone's surprise. Needless to say, the slingshot was quickly dismantled.

I have many fond memories of Henry during our associations at sea. ❖

Robert Knox

Scripps Institution of Oceanography



One summer in graduate school, about 1967 or 1968, I spent a few weeks in Bermuda working with Hank on early S (not C!) TD measurements of finestructure, making several-day trips to sea on Columbia's old *Sir Horace Lamb*. Ashore, we were staying at the Biological Station, and Hank had brought his son Matthew along. Matthew was then just shy of legal driving age, and there had been considerable father-son discussion about renting a motorbike. Matthew prevailed on the rental issue, but Hank's misgivings were real, so the condition was that Matthew had to do some satisfactory practice laps on the Station's entrance road before being allowed on the public roads. The entrance road was a loop, usually driven in only one direction, and partly screened from view of the Station by shrubbery. Hank and I sat down on the Station steps to observe the trials.

Everything went well at first. Matthew would move slowly away, keeping firmly to the left and observing all rules. As soon as he was obscured by the shrubs, we could hear a significant increase in RPM, but as he came back into view his speed was proper, even sedate, and his control seemed adequate. This process went through several repetitions. At the end of each lap, Matthew advanced the view that he should now be let out onto the public roads, while Hank, careful observer that he was (not to mention careful driver—see Fuglister or Veronis in *The Evolution of Physical Oceanography*, 1981) required another lap to be certain that the driving ability was real, not a statistical fluke. I think that on the final lap Hank was about to trust the statistics and slip Matthew's leash, but just then one of the Biological Station residents came zooming up the road, in the opposite and unexpected direction, counter to Matthew's course. Matthew immediately responded reflexively with excellent drive-on-the-right instincts, thus shooting directly across the path of the oncoming student and ending up in the bushes.

As we hurried to the scene, Hank's marvelously expressive face seemed to be trying to do three things all at once: show concern for his son (who in fact suffered no serious harm), preserve an appropriately solemn demeanor in order to bolster his point that perhaps more practice really was needed, and simply burst out laughing at the Chaplin-esque scene. Somehow he managed to do all three in a very short space

*Hank's
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do three
things all at
once....*

of time. I don't recall how much this incident may have set back Matthew's release onto the Bermuda roads. I remember it for capturing in a few seconds images of Hank, the kind and concerned human being—a truly gentle man, and of Hank, the possessor of one of the world's great senses of humor, with a laugh to match. I suspect he may have touched at least as many lives with these gifts of his spirit as he did with the unparalleled oceanographic accomplishments of his mind. ❖

Curtis Collins

Naval Postgraduate School



Probably the most useful thing that I can contribute is a description of Hank's dealings with me as a National Science Foundation (NSF) program manager. Dealing with funding agencies is a necessity for most academic oceanographers, and Hank had a unique approach, as he did for most things.

It is strange that I can't remember when I first met Hank. It was probably at a Mid-Ocean Dynamics Experiment (MODE) meeting where more loquacious oceanographers stood out. (The first encounter that I can remember with Hank was at the Bermuda Biological Station. I arrived with my wife and two very small children for duty at the MODE Hot Line Center, only to discover Hank and John Swallow firmly ensconced in the cottage that I understood had been reserved for us. While we were trying to find alternate quarters, Hank and John quickly packed and moved to the dormitory, solving my problem in a very gracious manner.) But I do remember my first encounter with a Stommel proposal. It was 1970 and I had been assigned two weeks of reserve training duty at the Office of Naval Research (ONR). A (nonphysical) oceanographer colleague, who was working at NSF, asked my opinion of a "problem" proposal. He explained that it had no hypothesis, no work plan, but had been reviewed extremely favorably. Of course, it was Stommel's proposal.

I don't think that Hank spent a lot of time writing proposals. He described clearly the problems that he was working on and the approaches that he was going to try to use, always honestly and usually optimistically. He picked difficult but important problems and innovative approaches to them. Some of the physics was clear to me. Even after 15 years, I looked forward to his proposals, finding them fun to read, and always learning something about ocean circulation from them.

Hank had tremendous faith in the judgment of his peers. He preferred to submit one-year proposals, despite our admonishment that multiyear proposals were easier to handle. (With understanding supervisors, we usually could convert his one-year proposal to two years.)

During the time that I was in Washington, it was usual for an oceanographer to write multiple proposals, one for base support and

I looked forward to his proposals, finding them fun to read, and always learning something about ocean circulation from them.

the other for field work, especially if it was expensive. The record shows that NSF declined three of Hank's experimental proposals and I know of one proposal that the ONR declined as well, dealing with expendable bathythermograph (XBT) development. Unless the principal investigator has clearly overlooked something, proposal declination was my least favorite duty, and, of course, Hank would not make critical technical omissions.

The last proposal that I declined dealt with a North Atlantic hydrographic section—one that I am sure that no one else wanted to do, yet Hank felt was important. I remember clearly the phone conversation: Hank readily understood how priorities had been set, put in a plug for Mike McCartney's proposal (for a related section) as being more important, and ended by consoling me on my onerous duty of calling with bad news! ❖

Arthur Voorhis

Woods Hole Oceanographic Institution (retired)



Random thoughts came to my mind when asked to reminisce about Henry Stommel. He was truly amazing, with a bold, creative, and original mind. I especially enjoyed his sense of humor and can still hear in my mind's ear his distinctive laugh. Anyone who stopped by his office in the Clark building was treated to all this and to stimulating talk on a variety of subjects, including his latest work and projects. One also usually got to see, as an additional bonus, ongoing work on his computer video screen.

Although Henry was a kind man and usually suffered fools gladly, he could get very annoyed. I found this out on the *Atlantis II* in the winter of 1969/1970 in the Mediterranean Sea, south of France. We were on a cruise to investigate deep convection and bottom-water formation caused by those outbreaks of frigid continental air known as mistrals. The ship had just departed from Gibraltar and we were proceeding eastward against a heavy sea when I was awakened during the midnight-to-four watch by a loud noise from the chief scientist's cabin. Holding on as best I could, I managed to get over to his room and open the door.

There stood Henry in his flannel pajamas in semidarkness, desperately trying to keep his balance and surrounded by hundreds of empty glass sample bottles that had broken loose from their cases and were cascading back and forth across the floor with each ship's roll.

I am ashamed to say I must have smiled slightly at his predicament before lending a helping hand. Although very angry, he was able to keep his temper and said, "I suppose you think this is all very funny."

Oh, Henry, what a wonderful understatement! ❖

*There stood
Henry in his
flannel
pajamas in
semidarkness,
desperately
trying to keep
his balance....*

Letters to the Children

INTERNATIONAL INDIAN OCEAN EXPEDITION and COOPERATIVE KUROSHIO INVESTIGATIONS



**** R.V. ATLANTIS II ****

CRUISE 15

WOODS HOLE OCEANOGRAPHIC INSTITUTION

WOODS HOLE, MASSACHUSETTS U.S.A.

Dear Matthew:—

Our trip is going along very well and we have had good sailing weather so far—no storms or rough weather. It is very hot here in Manila, though.

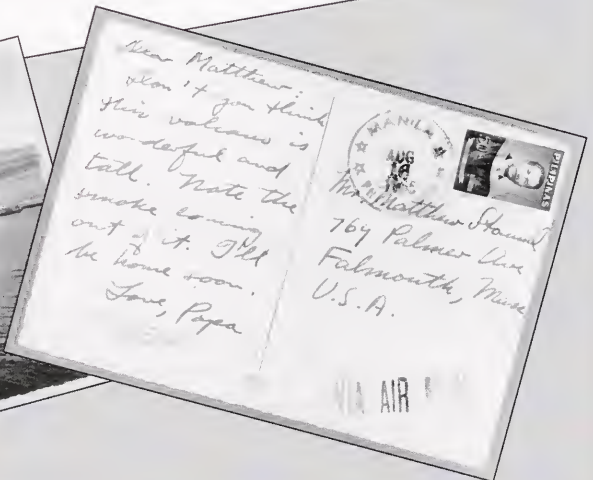
We have a computer on board the Atlantis much like the one Mr. Fredkin has. We are making it do much of the calculations and data reduction for the observations. We have a device which permits the PDP to draw graphs and drawings—and it now draws most of our graphs and diagrams automatically. There is lots of time on a ship to work with the computer and I am finding it fun.

We also have a lot of electronic gear that Mr. Walden helps with. I think you would like it aboard this ship. We have movies twice a week.

I hope you are getting along well and sailing a lot on our little boat. You are a good boy and I love you.

Your Papa

Mailed at Manila, Philippines
August 14, 1965



766 Palmer Ave., Falmouth, Mass 02540

Dear Lije:

It was really great fun to speak with you on the telephone last night, and to know that we are not really so far apart now as we were when you were in France. At least we can communicate by mail and telephone a little more easily.

I can sympathize with your predicament over continuing in music as a career. Most young people go all the way through college without making a firm decision for any particular kind of vocation. The liberal arts curriculum gives them plenty of leeway, and they can put off decisions. As you know I vacillated all sorts of ways when an undergraduate at Yale, and even afterwards for two years in graduate school was torn between the Ministry, Astronomy, and only by chance fell into Oceanography—at the age of 24. But Music is more demanding, I fear, and one has to decide much earlier. It is also rewarding in that even if you eventually decide not to go into Music professionally, it still remains a wonderful avocation, and a source of pleasure all your life. My old friend George Morgan was first a pianist (at Julliard) and later became a mathematician (and still later at age around 36 years became a sociologist). Whatever you decide, your years of practice, and your years in France, will always be a great experience and advantage to you. And you do not have to decide exactly what alternatives you must choose as a profession for quite a few more years. There are plenty of wonderful things to occupy oneself with in this world, and you will find out about them during the next few years at college.

Another thing: perhaps USC is too big and impersonal a place for you. I think it would be for me. A smaller, more intimate place, like Bowdoin, in pleasant rural surroundings on the Maine coast, would have been a happier place for me than Yale, I think. People like Bruce Warren and Robert Knox went to colleges like that. If you find the urban sprawl which surrounds you at USC unbearable, you can plan on switching...we can talk about that when I see you in mid-November.

Someday you should have a heart-to-heart talk with your violin teacher. Maybe he can judge from your performance so far what kind of violinist you would likely be. As painful as it is to look at oneself and one's abilities coldly and objectively, it is a healthy and necessary thing to do. Maybe he will encourage you to go on; or perhaps he won't. But I cannot judge myself, not being a musician.

And above all, don't undervalue your other attributes. You are a very special person, I think, in that you are more humane than most people; you have a deep sensitivity to other peoples' feelings and needs; you are basically kindly and loving; you want to help other people and want to be liked by them. These attributes are rather rare and they are desperately needed in a world torn by strife and greed and blind selfishness and unconcern. You are fundamentally civilized in personality and have a capacity for empathy with the needs and weaknesses of others. You could be a fine psychologist, an effective teacher or social worker, a good minister, a sympathetic physician...just to mention a few kinds of careers where human sensitivity can be made good use of.

Try to take it all in your stride, Buster, and phone us Monday night, Oct. 23, when I think Mother will be home again. Or before if you want to.

Your loving
Pa

Editor's Note: Elijah Stommel decided to become a physician.

SIPPEWISSETT FARM
 766 Palmer Avenue
 Falmouth, Mass. 02540



Dear Abby:

Here is a letter from me and another from Elijah.
 On Tuesday Elijah and I went over to Arnold
 Gifford's homestead and took our sheep with us. A man
 from South Carver sheared them all and they now look
 very skinny without any wool. But they are cooler now
 that they do not have such heavy coats.

We put the wool into grain bags and sent it to
 Maine where there is a factory that will make a blanket
 out of the wool. It will be nice I think to have a blanket
 made of our own sheeps' wool—don't you think so too?

Also there is enough wool to make some knitting
 yarn too—so if you want to you can even make a
 sweater.

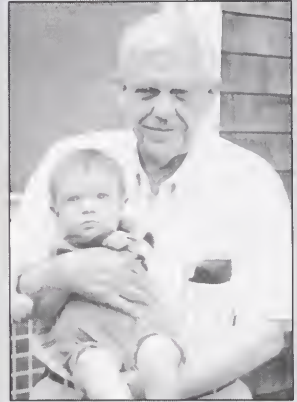
By next Christmas the sheep will grow new coats so
 they will be warm again for winter.

I do hope that you are enjoying the horseback rid-
 ing. We miss you a lot, and we all love you.

Your loving
 Pa

I am enclosing an envelope that you can use for answer-
 ing sometime.

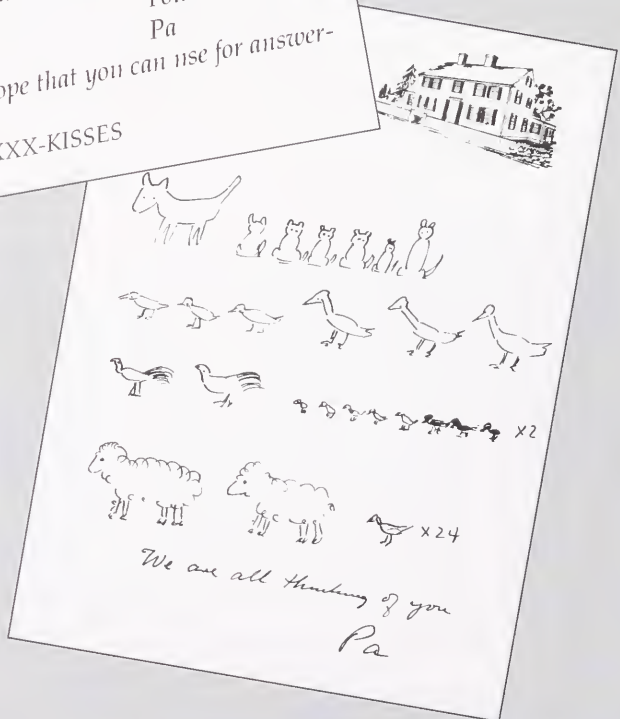
XXX-KISSES



Hank holds "Little Hank,"
 Matthew's son.



Hank Stommel clowns with a false
 moustache. Elijah is at right.



March 26, 1967

Dear Abby,

It is very lucky that the ship is going to stop for a little while in Wellington, New Zealand—just long enough for some of this mail to be sent out from the ship to you. So I have written some letters to each of you and to MaMa, just to let you know that I am thinking of you and love you all very much.

By April 7, which may be about the time that you get this letter, my trip will actually be half over. There was some wasted time at the beginning waiting for airplanes and the ship to leave, so that I will still have more than half of the ship distance to sail; but so far as time away from home is concerned April 7 is just exactly half-way. Isn't that fine?

Inside this letter is a map which I tore out of Time magazine. It shows the part of the world where I now am. I put an ink line to show where the ship is going to be at different times. That way you can know where I am. The ship left Melbourne, Australia, on March 10, and is now (while I write this letter) off New Zealand where I marked the line. The other places and dates are also marked for you to follow. If you put this up on the wall you will know how things are going.

We are having a pretty good trip although it is very long and lonely, and a ship is rather a small place to be cooped up for so long. But I think the work that we are trying to do is going well and the other people on the ship are very pleasant.

Just the same I am thinking of you and Mama and Elijah and Matthew all the time—and also of Binky and the cats, both of whom I hope are still well. Do you have any other new animals that will surprize me when I get back?

Love
Papa



Hank Stommel with Elijah and Abby

Lyze,

Your radiator seems to have stopped dripping. After a few days to make sure we can put some antefreeze into it again. Don't scald yourself with the radiator when taking off the hot cap. Use a rag and skip back.

Bob Beardsley

Woods Hole Oceanographic Institution



Around 1972, when I was on the faculty of MIT with Henry, a young graduate student named Brad Butman, now head of the US Geological Survey (USGS) branch in Woods Hole, asked about some formal course work in coastal physical oceanography. He had a summer job helping the USGS study bottom sediments and currents in Massachusetts Bay and wanted some training in this area. Pierre Welander (University of Gothenburg, Sweden) was about to visit for a semester, so Henry organized a reading course on estuarine and coastal dynamics involving the four of us and Ants Leetmaa (MIT). Out of this course came Henry's influential paper with Ants on the mean flow in the Mid-Atlantic Bight, and my own interest in switching fields from laboratory modeling to study shelf circulation. During this period, Henry also opened my eyes to the experimental side of oceanography, especially the need for good field experiments and the excitement and fun of learning from field measurements.

One vignette from this period is a picnic outing to Lexington where Henry, Pierre, Dave Niergard, and Erik Mollo-Christensen (all of MIT) sent up a long string of tethered balloons to see if we could see the velocity vector rotate with height in the atmospheric Ekman layer. I don't remember the results, except we had fun trying something new.

Thank you, Henry, for all your kindness and help along the way. ❖

D. James Baker

Joint Oceanographic Institutions Inc.



Enthusiasm, humor, gentleness, intensity, involvement, commitment, and fierceness are all characteristics I think of when I try to describe Henry Stommel. I first met Henry during the Indian Ocean Expedition in 1962. I was new to oceanography, having just completed my Ph.D. in physics from Cornell, and John Knauss had invited me to join him as his first postdoc at the University of Rhode Island. Henry stood out among the leaders of the field that John had invited on one of the one-month equatorial cruises; it was a fine introduction to oceanography and its people. Henry always seemed very friendly, but I remember best a different face.

I had read Henry's famous 1948 paper on ocean circulation, "The Westward Intensification of Wind-Driven Ocean Currents," and while we were taking water samples from Nansen bottles I asked him why he used the simple proportional-to-velocity dissipation term. He gave me a fierce look and clearly did not want to discuss it. I was startled, but realized I must have hit a raw nerve, or maybe was being too brash as a newcomer.

Later I moved to Harvard, and kept my personal contacts with Henry. We had long discussions about all facets of oceanography and also shared time at sea. His gentle good humor was always present; I will not forget his friendly chuckle when he saw something he liked or

*Enthusiasm,
humor,
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something that amused him. One evening we were sitting in the library of *Atlantis II* on a Gulf Stream cruise having a happy chat about something or other; I felt comfortable with the talk and the friendly chime of the ship's bell. I later bought a chiming ship's clock just to recapture those moments.

Henry was always focused on science as opposed to organization. MODE (Mid-Ocean Dynamics Experiment) was clearly an ambivalent activity for him; it involved new and exciting science, but also required a certain amount of organization and committee work. He did not like the organizational activity. However, I remember one sunny day in Bermuda at the end of a planning session where Henry rolled out a long—it must have been at least 20 or 30 feet—sheet that had all the MODE plans and cruises in a great schedule. He relished that!

Henry's intense involvement with whatever he did was contagious. He was an enthusiastic participant in discussions about how long-term global ocean measurements could be carried out. I remember a meeting in the 1970s in Victoria, British Columbia, on this topic that Henry had organized as part of a National Academy study. The meeting was in the Maritime Museum and Henry was in his glory, explaining the history of various artifacts to us. After the meeting, we agreed what we would each do. Henry then departed on a trip to Germany. Two or three weeks later I received a card from him in his best German: "I hope, dear colleague, that you have completed your work and sent it to me." I was so impressed that I finished my assignment immediately!

The Victoria meeting was the start of a long-term collaboration with Henry in the promotion of long-term ocean measurements. Later we traveled together to another meeting in London on the same topic, arriving on the usual early-morning plane from Boston. Henry's passion for railroading was evident that day. He told me he knew of a small store, somewhere in London, that had just the kind of train he wanted to buy. We spent several hours on the tube going to different parts of London to find this shop—the large group that started the search dwindled to just four. I was disappointed when we finally found the right one; it was small and didn't seem very interesting. But Henry knew this was the right one—he had been corresponding with the owner. The two of them had a happy conversation while we others politely waited and looked around. Henry finally finished and suggested that we have tea. It was at least a good way to stay awake all day on first arrival!

Henry encouraged me throughout the development of the deep-sea pressure gauge that I was working on at Harvard with Dick Wearn, and was supportive of the idea that the pressure gauges might be used in the Drake Passage to monitor transport. When Worth Nowlin (Texas A&M University) and I put together the first plans for the International Southern Ocean Study, which we hoped would be accepted as part of the International Decade of Ocean Exploration, we went to Henry to ask his advice about how to make this happen. By this time, Henry had moved back to Woods Hole from MIT. We met at his house, with a cheery fire in the background and Henry sitting beside the fireplace with all the air of an 18th-century squire giving his blessing to a new endeavor. His support was essential—I remember one NSF ocean sciences official saying that "whenever Henry comes down here and explains in his

*I later
bought a
chiming
ship's clock
just to
recapture
those
moments.*

friendly and polite way why whatever he has interest in now is essential for the progress of ocean science, we simply can't say no!"

I left Harvard in the early 1970s to move to the University of Washington. I talked to Henry then about research directions and general philosophy. He urged me to focus on science and to minimize committee work, a point he made in a nice way in his talk to the Oceanography Society at Monterey in 1989, where he used the example of Professor Airy, the Astronomer Royal of Great Britain, who missed the discovery of a planet because of committee busy work (see *Why We Are Oceanographers*, opposite). Henry was a strong supporter of the Oceanography Society, and wrote me several letters about what it might do. He was particularly interested in using the Society to get small groups of oceanographers together on a regional basis to discuss science. It was his way to continue to promote discussions on a personal basis. It's good advice, and I am sure that the Society will follow it. ❖

George Seaver
SeaLife Engineering



I first met Henry in 1973. I was just finishing graduate school and had been in the Navy, and Henry thought I could be useful at the Mid-Ocean Dynamics Experiment office at MIT. Well, soon after I arrived, he started talking about this "tramp steamer" idea: We would ride passenger freighters across the Southern Hemisphere's oceans and drop an expendable bathythermograph (XBT) every hour—cheap, worldwide coverage on a mesoscale. I would go on Orient-Oversea's Buenos Aires-

to-Capetown run, he would go on Royal InterOcean's Tonga-to-Auckland run, Chickie would go on one, and, I supposed, Lord Jim* would also take a leg. It was science on the *Patna**, science on both sides of the dynamic topography, and science with a dramatic flair. I thought so, anyway—but the Navy didn't when I presented them with the proposal. They thought it was capricious and not well thought out; I also came to see it as probably sub-professional.



Tramp steamer routes as a way to obtain cheap, worldwide, expendable-bathythermograph coverage were plotted for a proposal entitled "World Survey of Mesoscale Eddy Amplitudes and Scales."

It wasn't. I saw it again 16 years later, this time in the 1989 article in *Oceanus* entitled "The Slocum Mission" (Vol. 32, No. 4, Winter 1989/1990). It was all there, but now with "tramp drifters." People-sized science, playful episodes, sudden inspirations, fairness, rescue of a good idea, and—above all—innovative science. ❖

*References to Joseph Conrad's *Lord Jim*.

Why We Are Oceanographers

Henry Stommel presented this talk at the first meeting of The Oceanography Society in 1989, and it was included in the first issue of the society's magazine, *Oceanography*.

It is reprinted here with the society's permission.

WORKING BACKWARD in time, give or take a few years, the overlapping lifetimes of three scientists—Einstein, Darwin, and Maskelyne—take us back to an age when Isaac Newton still breathed. That is a measure of the speed at which scientific knowledge has grown.

The numbers employed in science today quite probably exceed 90 percent of all the scientists that ever were in all human history. The proportion of living oceanographers to the all-time total is, I think, even larger than 90 percent.

This expansion of job opportunities has its roots in the accelerated research effort during World War II. Many of my contemporaries—Andrew Bunker, Robert Reid, Don Pritchard, to mention but a few—came into oceanography because they had first been introduced to it in military meteorology training programs. The need for a permanent post-war national effort in oceanography was first clearly enunciated by the National Academy of Sciences' Committee on Oceanography under the chairmanship of Harrison Brown in the mid-1950s. Over the years this Committee, under varying names and guises, and with varying effectiveness, established a rationale for expanded national funding as we now know it. The heyday of the committee was, I think, during the years 1955 to 1965—the time of the two-martini lunch—when its members included lobbyists from the pet-food and chemical industries, a retired investment banker well acquainted with the Congress, and a sprinkling of charming promoters, as well as working scientists.

Depending upon the shifting focus of public concern, the committee espoused various causes: defense, food for the Third World, mining the ocean bottom, the Law of the Sea, preservation of the environment, and climate change. And Congress responded handsomely.

The success of the Committee also stimulated parallel efforts at promotion of oceanography in state governments, in international scientific unions, and in the United Nations Educational, Scientific and Cultural Organization (UNESCO). The universities responded by establishing new departments and schools of oceanography. And the ocean cinematographer Jacques Cousteau made the general public aware of the sea in a compellingly romantic way, with an immediate appeal reminiscent of the narratives of the great ocean explorers of past centuries.

OCEANOGRAPHY was made visible to two generations of students. It offered a free graduate education. And so here we all are—recipients of the opportunities opened to us by a few resourceful promoters with a convincing brief. That is the first good hard reason that we became oceanographers, or rather, that we could become oceanographers. The world has changed a lot since the day when Henry Bigelow advised Ray Montgomery not to enter oceanography because he did not have a private fortune. Those were the days when the Woods Hole Oceanographic Institution was known to its rivals from the Marine Biological Laboratory as the Harvard Yacht Club, and the *Atlantis* was encour-

aged to travel under sail because diesel fuel cost \$15 a day. So we really have a great deal to thank those promoters of 20 to 30 years ago for.

From the point of view of an individual faced with the decision of whether to enter upon a career in oceanography, the issues are more personal: Will it be congenial, will it be interesting, does it suit my talents?

Certainly work at sea is congenial. It is a special social experience. Life on a small ship means living with people with backgrounds different from those of academia, and broadens our human contacts.

Robert Munnis



Henry Stommel aboard *Atlantis II* in 1963.

Developing good instruments and getting good measurements at sea is challenging, and there are prospects of learning something new and unexpected. And there are foreign ports and remote islands to visit. For many, the regularity, the simplicity, of life at sea is therapeutic. Oceanographers like Nansen, who spent years living with others crowded in small ships in the Arctic, developed views of human relations that are different from those we learn on the freeway. George Deacon began his oceanographic career with four successive

antarctic cruises, each lasting 18 months. The *Discovery II* became a home to him: a regular station schedule (8 in the morning and 8 at night), a good drink before dinner, and a game of pinochle in the bar before retiring. You'd learn the social arts quickly enough. Deacon was one of the kindest, gentlest, and most persuasive men I have known. Work at sea rubs off the sharp edges, and makes us better people. The ship becomes a home away from home. That has changed a little for us at Woods Hole, now that our ships are "dry."

Oceanography is interesting because so much is still unknown and there is a great variety of activity—observational and theoretical—in which to submerge oneself. And if one tires of active research, there are jobs at management level in which one can find important, useful things to do.

DOES IT SUIT my talents? Those of us who entered oceanography from more highly developed sciences like astronomy, physical chemistry, or physics have thought so. Thinking that we were unable to make much impact in these highly sophisticated fields, some of us found areas within oceanography where elementary ideas, simple theoretical models, first-order descriptions, and techniques borrowed from better-developed fields could be useful. I hope that it does not offend anyone when I suggest that oceanography has been attractive to many of us because it is low-powered. It's just a different way of saying that we preferred the pioneer homesteading model of the scientific life to the glitter of the intellectually fashionable.

On the whole we have done pretty well. A new field of geophysical fluid dynamics (a term coined by Willem Malkus about 1953) has grown, with deep connections to meteorology and astrophysics. Leaf through the 1942 treatise by Sverdrup, Johnson, and Fleming and you

are struck by the absence of any dynamical theory beyond Ekman's 1902 spiral and the elder Bjerknes's practical method of doing dynamical current calculations. The most elementary problems had not been posed, nor the most primitive models constructed. Since then some of the vacuum has been filled—enough, anyway, to fill textbooks and, I fear, give new students the impression that the vacuum has been completely filled. However, as students begin to think and work, they will know better; they can still find an immense unexplored universe of ideas and phenomena to explore.

Many new sophisticated mathematical techniques that have increased our power to study the ocean have come from other disciplines. Fortunately there have been oceanographers with broad enough skills to translate these techniques into useful tools for oceanography. Singular perturbation theory and linear programming have come from applied mathematics. Most of the fundamental dynamical ideas, objective analysis, and numerical modeling with data assimilation have come from meteorology. Modern theory of time-series analysis and techniques for detecting signals have come from electrical engineering. Inverse theory has come via geophysics. These transplants illustrate the benefits of getting a good education in something besides oceanography alone.

THE CHIEF SOURCE of ideas in oceanography comes, I think, from new observations. Today we take much of ocean knowledge for granted. There was a time when eddies and meanders were only dimly perceived (1948); a time when we didn't know of the existence of the equatorial undercurrent (1952), or that the slope of the isotherms in the Gulf Stream extends to the bottom (1954), or that there was a deep recirculation, a time when the deep western boundary currents of Greenland Sea water had not been discovered flowing along the slope of Greenland around into

the Labrador Sea (1952). There was a time when we didn't have reliable estimates of the flow through the Florida Straits (1960), when the ubiquity of inertial motions in the deep sea was not suspected (1957), when it could be thought that the velocity in deep water was too small to measure by current meter (1958), and when we had no clear observational description of a deep, winter-time, bottom-water-formation event (1969).

More recently, we were surprised by multiple jets at the equator of the Indian Ocean (1976), by hot vents and the great helium plume in the Pacific, and by the red spectrum of the nine-year drift of SOFAR

**“Work at sea rubs
off the sharp edges,
and makes us
better people.”**

[Sound Fixing And Ranging] floats in the Atlantic. The geochemists persist in unsettling our mental equilibrium with new current patterns revealed by exotic tracers like Freons. Who would have foreseen “meddies” (1981), and who knows their role in deep-sea mixing? There is the wonderful unfolding development of our knowledge about El Niño. There are those subtle features of the equation of state that Trevor McDougall has uncovered. And there is that amazing large-scale horizontal coherence of persistent doubly diffusive layers revealed in the Caribbean Sheets and Layers Transects (1987) expedition that is pregnant with implications concerning deep-ocean mixing processes.

I have mentioned only a few of the unexpected phenomena that have been discovered in the past few decades—merely the ones that come easily to mind. There seems to be no end of new surprises. And, if oceanographers are permitted in the



Hank draws samples from water bottles with Bob Stanley and Mindy Hall (*Oceanus*, 1980).

future the freedom to follow their own noses, to scent out their own problems, and to formulate their own goals, this flow of new results will doubtless continue.

On the whole, when it comes to the phenomenology of the ocean, there are more discoveries than predictions. Most theories are about observations that have already been made. It is therefore particularly exciting when a theorist comes up with an idea about a feature of the ocean that he is willing to go to sea to look for. I urge those entering the field to take the risk.

SO WHEN WE SURVEY the personal reasons why we entered oceanography—that it be congenial, that it be suited to our talents, and that it be interesting—I think our choice of career was justified. And if some of us somehow can manage to avoid getting entangled in the “Big Science” part of our field, then perhaps we can preserve an innocent, simple approach to our

tasks. Our work can seem like a pleasant hobby to us, it can sustain a sense of wonder, and bring us joy and fulfillment.

There is a wonderful story about the excellent Astronomer Royal, George Airy, that may serve as a warning of the perils of too much committee work and public service. Airy was very accomplished. He also was extremely fastidious, so much so that all his papers are preserved—every check book, account, letter, memo. Even his scrap paper was sewed together and saved. He regularly updated an autobiography.

Airy is known to oceanographers as the author of an early tidal treatise and for the Airy function familiar to those who work with the equatorial beta-plane. He took on a huge amount of committee work. In 1845 (at the age of 43), he was president of the Royal Astronomical Society. He served on the Tidal Harbor Commission and did extensive studies of breakwaters of the Dover Pier. He lectured on Irish

tides and the design of saw mills. On the Standards Commission, he contributed theoretical studies of the flexure of uniform bars; he helped determine the longitude of Valencia Island, Ireland; he visited tin mines. He oversaw the planning and execution of the survey of the international boundary between Maine and Quebec. He studied rotary engines. He made himself busy devising schemes for compensating magnetic compasses on iron-hulled ships. He served on the Railway Gauge Commission—whose purpose was to choose a British standard gauge—which, in his own words, he characterized as “an important employment.”

UNFORTUNATELY HE was so busy that he was not at home in September and October when the 24-year-old astronomer John C. Adams twice came down from Cambridge to visit him, in the hope of discussing his new prediction of the existence and location of a trans-Uranian planet—the one we now call Neptune. Adams had devised a way to work backward from the observed irregularities of the orbit of Uranus to the orbit of the disturbing planet. It was an early success of inverse theory. It was destined to become one of the most celebrated astronomical achievements.

Although they did not meet, Airy sent Adams a set of observations of Uranus that had been collected at Greenwich. Autumn turned to winter, and still Adams did not publish his work. Meanwhile, in Paris, independently, Leverrier had commenced his own attempt at explaining the irregularities of the Uranian orbit. By November 16, 1845, he had published his first results, and on June 2, 1846, the second part appeared. Airy corresponded with Leverrier. He was now aware that both predicted locations agreed to within a degree, and by July 16th Airy became somewhat alarmed for Adams’s priority. Ten months had passed since Adams had tried to visit him, but still

he did not urge Adams to publish. Instead he asked Challis at the Cambridge Observatory, where there was a new 12-inch refractor, to search for the new planet where Adams had indicated it to be. But Challis was preoccupied by his own comet program, and Airy, as he put it, “my nerves shaken by the work on the Railway Gauge Commission,” traveled from August 10 to October 11 on the continent with his wife and her sister Elizabeth Smith to take the water at Wiesbaden and an excursion to the Swiss mountains. Meanwhile, on August 31 Leverrier’s third paper was published. Airy then junketed to visit his friend Professor Hansen at Gotha, where he heard the astonishing news that on September 16th Leverrier had mailed his predicted location for the new planet to the Berlin Observatory. And within five days, Dr. Galle, using Bremiker’s admirable map for Hora XXI as a reference, had observed the



George Knapp

A contemplative moment on *Oceanus* in 1980.

tiny eighth-magnitude disk of Neptune, with a retrograde motion in right ascension of six seconds a day. Praise and honors immediately showered upon Leverrier. Airy had a miserable five-day sea passage home from Hamburg to London (the crankpin of the steamer broke and had to be repaired). And he was seasick.

When he got home and put forward Adams's claim, he was shouted down by both angry French and British, the French for sullyng Leverrier's just claim to fame, and the British for being so inattentive to Adams's interests.

BREAKING NEW GROUND in science is such a difficult process that it can only be done by an individual mind. For some of us, that is the main attraction of doing scientific work. In this respect it is like the art of painting or musical composition or poetry. The exhibitions, orchestral performances, and public readings come later, as do the art dealers, the recording companies, and the paperback publishers. But it all begins with an individual's choice of medium, choice of theme and style and subject. And if you try to impose themes or goals with a social purpose, you produce those grotesque travesties one sees in Peoples' Republics and commercial advertising.

Each of us has a finite supply of energy. We draw upon it when we think hard, supervise a technical group, or go to sea. Often it takes the last ounce of effort to break through to something new. So watch your Plimsoll mark, and don't become too heavily laden with other things to do. You need to be able to turn quickly, change plans, backtrack, and, when the moment comes, to drop everything else to pursue that flighty elusive new clue.

The creative scientist asks the question that has not been posed before. He is like a perpetual graduate student in quest of a thesis topic. He discovers how to engage his own potential most effectively. Considerations of social relevance do not

dominate his tactics. He embarks on a course as nearly orthogonal and independent of previously charted courses as he is capable of desecrating. And with luck, the grace of the peer review system, and the support of the Science Foundation, he will produce some significantly new fact or thought. And it will bring joy.

The prospect of being an independent investigator is one of the great attractions of oceanography, as contrasted to laboring within a preplanned program. Certainly there are drawbacks. You will be an employee, with a time-clock number. You will be employed by an impersonal corporation, owned by people you never see. They will monitor your performance, set your rank and salary, and decide whether you can stay on. They will not provide funds, however, for your research. Those you will have to seek, annually, by proposals to government funding agencies. The outcome will be decided by outside peer review. So you will be in double jeopardy.

But serving two separate masters is the key to the freedom you need to carve out your own research program. So if someone mentions block funding, or suggests a big project in which you are welcome to work, you might consider rolling over and playing dead.

YOU NEED NOT work entirely alone. From time to time you will find another investigator whose skills and equipment complement yours. This will be a collaboration for a single, well-defined scientific purpose, not collaboration for its own sake. It will be comfortably below the threshold of big science.

My own most pleasant past collaborations, with English, French, and German oceanographers, were of this transient variety. Even the Mediterranean Deep Ocean Convection experiment (MEDOC 69), an international program involving six ships, lasted only three months and was

organized completely in a single, half-day meeting. The funding agencies were in different countries and so were all independent of one another, and we could work at the individual proposal level. In a sense, the Genie of Big Planning was momentarily let out of the bottle...but it was easy to stuff back in again.

A few words about the Genie of Big Planning. It is an old acquaintance of scientists. When let out of the bottle it feeds on our attention and time. It requires constant attendance at its court. There are those who can look into its face without turning into pillars of salt. For others, like me, pro-

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longed exposure threatens brain death.

In the early days of the Committee on Oceanography, when it came to putting some substance into a promotional report, one of the best and most effective statesmen of our science would commandeer the chalk and proceed to paraphrase the table of contents of Sverdrup's textbook as an outline. He repeated this performance over the years, just changing the words around a little. It provided an overall statement of the scope of oceanography at a time when few were acquainted with the word. It saved the rest of us a lot of work. It saw us through the 1950s. And it did not involve formulating detailed national plans. For the individual investigator it did not threaten the integrity of the peer-review system.

By 1958, it became clear that we needed to augment the research fleet, and that launching something big would help justify the expense. The International

Indian Ocean Expedition of the early 1960s was the adventitious child of this need. It was a last-minute inspiration of Columbus Iselin, who, during a coffee break at an international meeting in Woods Hole, happened to glance at a chart of the positions of deep hydrographic stations in the world ocean that was lying on the table in Fritz Fuglister's office. Iselin noticed the paucity of deep data in the Indian Ocean. A small idea expanded to fill a vacuum. He rejoined the meeting and suggested that an international expedition to the Indian Ocean might be the ticket. And soon we had the *Atlantis II*—the “new *Discovery*”—and had begun the last great nineteenth-century cruises of geographical discovery. There was no master plan; each cruise was the work of an individual scientist. A significant data gap was filled. And it became possible to visit such features as the Somali Current. Our interest in the monsoonal circulations of the Indian Ocean date from that time. The Genie's bottle had been shaken, but the stopper was not loosened.

YOU WILL RECALL HOW, as a school child, when the geometry teacher had you stand in front of the class and asked you to prove an unfamiliar proposition of Euclid, your mind went blank. You might have responded, “Should I drop a perpendicular, sir?” although you hadn't thought of the next step. Oceanographers, when confronted by a need for immediate action, respond by dropping a CTD and making a hydrographic section. It has been a fruitful reflex, often leading to important useful results.

If the West had too few ships, some science administrators in the Soviet Union were embarrassed by having too much ship time to justify. They acted reflexively. In 1962, they placed before UNESCO a proposal to ask its members to make regular, quarterly, hydrographic sections on a number of standard lines. If adopted, it would soak up much of the world's research-vessel time.

Advocates could argue on behalf of climate monitoring. We of the opposition feared that all our ships would hit the fan. The stopper in the bottle had been loosened, but before it popped out, a UNESCO delegation was sent to Moscow to offer a more scientifically interesting plan and ram the stopper hard home. The engineer of this countermove was a Russian, Konstantin Federov. Some major changes amongst the oceanographic and hydro-meteorological administrators in Moscow followed, and a first rate scientist was appointed director of the Institute of Oceanography. Federov was a brave and skillful dragon-slayer. He was a good friend and an excellent marksman with a rifle.

LOOKING BACK to this time, one can now see how weak a sword that counterproposal was. It was technologically impossible in 1962 to set out and recover a field of 100 moorings, each with a string of reliable current meters. It was not exactly a matter of opposing the undesirable by the impossible, but more a matter of introducing a new language into the debate: the language of time series, aliasing, power spectra, separation of time-and-space scales, etc.—things those sturdy old polar-explorer types who ran the Hydrometeorological Service didn't know about.

Seven years went by; I gave up promoting mooring experiments. It was in 1969 that Andrei Monin so stung my pride that I got involved in the Mid-Ocean Dynamics Experiment (MODE). It was at a meeting in Dublin; Ozmidov gave a paper on a Russian current-meter mooring experiment. Monin and I were sitting at a white enamel table in the college cafeteria. In a mildly sardonic tone he asked, "Henry, what ever happened to the US 100-mooring experiment?"

By 1969, Sverdrup's table of contents was overtaken by the growth of oceanography, and a new technique had been developed to stimulate oceanography: The International Decade of Oceanography

(IDOE). It became possible for the first time for oceanographers to organize sizable collaborative field projects and experiments, on a scale hitherto out of our reach. Under the wise leadership of Feenan Jennings it was possible to carry out MODE and the GEOchemical SECTIONS Study (GEOSECS). These projects involved important developments in measurement technology, for example, acquisition of significant numbers of moored current meters and Sound Fixing and Ranging floats. The projects were a form of Big Science, but they were not permanently established. They were formed for a particular job and dissolved in a few years when that job was done. Some future historian reviewing science policy may decide that it was at this point that the Genie got out of the bottle. I don't think so. I hope not. But there was a steep increase in the amount of administrative work and committee meetings. At one time someone suggested that we manage our projects with "PERT" or organizational diagrams, something big industries were presumed to do. To comply, Allan Robinson and I actually hired an expert from the Sloane School of Management to do it for us. MODE and the less successful POLYMODE that followed exhausted four successive executive officers.

I REALLY CAN'T JUDGE the full impact that these projects had on the field in the long run. In the short run, we got data available in no other way, we acquired numbers of new instruments, learned about objective analysis, and, I think, did worthwhile science. MODE may have been a model for future process-oriented experiments. It was my first experience with the socio-administrative aspects of joint scientific planning with the funding agencies and the setting of priorities that seemed to imply a preference for planned efforts over individual proposals. In the long run, one could reasonably be uneasy about whether this priority for the planned program was good for the future



The scientific party aboard *Oceanus* in 1980 for a "beta spiral" cruise included, top row, from left, Richard Tench, Mindy Hall, and George Knapp; Middle row, Affonso Mascarenhas, Bob Stanley, Ron Kroll, and Cindy Chandler; Front row, Larry Armi and Henry Stommel.

health of oceanography. I was certain that it stifled my own scientific productivity. And I realized that I wasn't shrewd enough to play poker with the professionals on the Potomac River sidewheeler.

Long ago at a meeting of the Harvard Oceanography Committee, the chairman announced that the committee had accumulated \$30,000 in funds of its own, and that the dean had asked to see him. He asked us for advice as to how to keep the dean from taking the money away. A distinguished chemistry professor said, "Remember that Dean Bundy is a lot smarter than you, and put all your cards on the table." I took that advice to heart.

It took a few years to find my way to the egress. In 1973, I chaired a National Academy of Sciences (NAS) report entitled "The Ocean's Role in Climate Prediction" for something called the National Climate Plan. The meteorologists were embarking

upon the First Global Atmospheric Research Program (GARP) and had invited us to join them. And, they had some funds to offer.

WE HAD TWO MEETINGS with at least 100 attendees. The substance of the rather slender report was a tabulation of assorted ongoing oceanographic projects, a time table, and some general remarks—not much of a report, really. It was something that one of us and a program officer could have produced over a weekend. Perhaps some of you will remember those hot September days in the parlor of the Brandigee Estate in Brookline, with the windows wide open. We were sweating over the great central table, surrounded by fading opulence, and, like Pharaoh, plagued by flying insects.

The atmosphere of the meeting was oppressive. As we pursued one tedious



Hank helps Dave Bitterman manhandle a velocity-sensing tow body aboard *Atlantis II* in 1979.

topic after another, the sense of having been there before and the enormity of what I was subjecting all my friends to overcame me. I went into some kind of emotional overdrive and spent the rest of the afternoon—with Claes Rooth—swatting flies. There was little joy in that meeting.

DURING THE PAST DECADE, I have renounced the Genie and all its works and haven't kept up with big planning. One does notice that there is a lot of coming and going over the surface of the earth—an unusually large number of meetings. The final World Ocean Circulation Experiment (WOCE) plan seems to be a fair balance between traditional and novel, between geographical and process-oriented programs. Participation in the planning has been widespread. Perhaps it really isn't Big Science at all—just an assemblage of the miscellaneous smaller projects that people would have wanted to do anyway. Surely

there is a Dean Bundy who will see to it that the outsider, the little guy, doesn't get pushed off the edge of the earth.

One is encouraged by the success of some of the other planned projects, such as Tropical Oceans–Global Atmosphere. This is one instance where repeated standard Equatorial Pacific Ocean Current Study sections made by National Oceanic and Atmospheric Administration vessels with the encouragement of Joe Fletcher have paid off in a handsome scientific fashion. This is a marvellous example of a natural system in which the sampling intervals and signal strengths are suitable for monitoring by standard sections. Further, the Office of Naval Research still manifests its old high skill in identifying and assembling little groups of oceanographers to work fruitfully together on special process-oriented projects such as Topo and Subduction.

If my little story about Sir George Airy has any prophetic value, and if our

science is a healthy one, wonderful and unplanned things will happen, unrelated to the large-scale planning. Young, unknown Leverriers will appear on the scene. They will be beginning postdocs unknown to our steering committees. And they will confound our cautious planning by important new insights that we had overlooked and by risking predictions that the rest of us will then be forced to confirm. New ideas have a dynamics of their own; they don't need to be promoted. If a simple theory can lead to a discovery in the real world, it commands attention, and is bound to shake up carefully planned programs.

LOOKING INTO THE future beyond 20 years of WOCE, I think that we will see establishment of a regular oceanic data network, using remotely controlled vehicles to make routine subsurface measurements on a global scale, like that of the meteorological network. Presumably, such regular data-collecting systems will eventually be taken over by responsible government agencies, and the research community will be relieved of taking much of these climate-motivated data. They will feed the hungry computers. But certainly we will always need ships to do our own work in the ocean.

Looking ahead, I think there may be a transformation in what we mean by "scientific understanding." When we use computers to process large amounts of data, it is a convenience that sharpens the data analysis. When we use complex numerical models to replace simple analytical ones, we may be doing something different. When we couple these numerical models to the inflow of data, in the so-called assimilation mode, we are doing something very different. And yet we must take this step if we are ever to have useful forecasting models for social purposes. Computers have also led us to accept the limits of predictability and have had a chastening effect on the arrogance of the exact sciences. Suppose that we concoct a model that actu-

ally forecasts climate with significant success. We know how the model is built because we programmed it. Once the program takes over it follows such intricate tortuous internal paths that we cannot understand them. It could be socially useful, maybe even a great triumph of sanitary engineering, but it will present us with a problem of understanding.

I wonder how our concept of understanding will evolve to accommodate itself to the complexity of these models. You all know that the notion of understanding is a rather slippery thing. For example, we can understand the interaction of discrete vortices most easily in terms of vorticity interactions, whereas resorting to the primitive inviscid equations would be a mess. We invoke ideas of normal modes in explaining vibrations. Rossby waves are easier to think of in terms of the vorticity equation rather than the momentum equations. These are old acquaintances, so we are comfortable with them. To some extent, analytical con-

"New ideas have a dynamics of their own; they don't need to be promoted."

cepts will be recognizable in the first data-assimilating models. For example, in Moore and Anderson's recent assimilation of expendable bathythermograph data into a layer model of the tropical Pacific Ocean (1989), the authors are able to interpret computed features as variants of familiar Kelvin and Rossby waves moving through the computed field and adjusting to the updated data. In more complicated models, features resembling analytically familiar ones may not dominate the action, and we will want new definitions of what we mean by understanding.

I have been asked by the two other surviving members of SOSO, the Society of Subprofessional Oceanographers, to convey our best wishes and felicitations to The Oceanography Society, and to wish it success. It will be an important forum in which oceanographers can consider their needs and can have an independent voice in organizing their affairs. In the past we have derived great benefit from our association with other societies, but they inevitably had an agenda additional to our own. Now we have a new start.

I hope that this is a good forum in which to make a statement of the main reason for being a scientist, as I see it. The president of the leading scientific honor society in this country has recently stated that students should be interested in science because it is fun. I think it is somewhat deeper than plain fun: It is a voyage of intellectual exploration, and an expression of the human spirit.

THE CONFLICTING tension between following one's own sense of direction and dutifully serving a social purpose is a strong one, especially when government funding is involved. I sense that so far I have given only one side of the story, so here are a few instances where the embattled pure scientist was unable to maintain a balance.

The history of science is strewn with melancholy wreckage from struggles to maintain some balance. When Ferdinand Hassler was appointed first director of the Coast and Geodetic Survey in 1816, he tried to begin with a general triangulation grid along the eastern seaboard. It was

slow, meticulous work, and to many congressmen it seemed much too academic. He was insensitive to the impatient commercial interests who wanted immediate surveys of their harbors. Consequently, the survey was disbanded in 1819. Thirteen years later it was resurrected under the superintendency of the more worldly Alexander Dallas Bache. Under Bache, the Coast Survey made many fine charts and maintained high professional standards. It even served as a refuge for a few scientists.

JOSEPH HENRY HAD great hopes that the Smithsonian Institution would be a national center for pure science. He was frustrated by the federal government's differing view: that it was the nation's attic.

There is the case of Josiah Whitney (for whom the mountain is named) who in 1861 was appointed head of the Geological Survey of California, on Louis Agassiz's recommendation. He embarked upon a serious scientific survey and had to appear before the state legislature each year to ask for a continuing appropriation. This was a time scarcely 11 years after the gold rush, and the lawmakers were anxious to exploit mineral resources. Whitney fed them a diet of paleontology, and undiplomatically lectured them on merits of science for its own sake and the evils of crass commercialism. The survey was discontinued after four years. Whitney was so outraged that he lost his sense of equilibrium and began to behave in a demented fashion; he lashed out against innocent bystanders, accused his colleagues of improprieties, and tried to destroy the reputation of his old acquaintance, Benjamin Silliman, Jr., of Yale because Silliman had publicized the opinion that there were useful oil reserves in Southern California. Whitney was on record that there were none.

In the nineteenth century, American scientists were very much on the defensive against the popularity of the unschooled Yankee inventor. Thomas Edison estab-

lished the first industrial laboratory in 1872. By 1876, he demonstrated his phonograph and carbon microphone before the National Academy of Sciences, but it was not impressed. Edison went on to invent the electric light, the motion picture camera—and founded whole industries. During World War I, the Navy had to consult him outside the National Research Council. Edison liked to make fun of pure scientists and loved to play the role of the common-sense practical engineer. In 1926, efforts were made belatedly to elect him to the engineering section of the academy. The members of long-standing who had resolutely opposed his election for 50 years had mostly passed away. R.A. Millikan made an impassioned speech of nomination, in which he asked, “Is there any physicist here who will deny that Edison has made great contributions to science?”—and A.A. Michelson rose from his seat to say, “I am that physicist.” He was the president of the Academy. This is a measure of how much pure scientists sometimes feel on the defensive.

With growing appreciation of approaching ecological disaster, oceanography has now been swept up in the effort to stave it off. There are important jobs to be done—and perhaps scientists like Hassler, Henry, Whitney, and Michelson would not be psychologically equipped to implement them. Perhaps oceanography has come of age in this respect, and in the future will inevitably be increasingly organized. So you see—in all honesty—there is another side to the question of pure science and scientific planning for public service. I’m trying to give the Genie his due, and to clarify the nature of the tension between the two sides. This new Oceanography Society can serve both pure and applied, the little and the big, the individual and the programmed.

However, in my heart I believe that, for a scientist, it is his personal mental wrestling match with some aspect of the universe that is his central activity and

reward. All alone, one confronts the unknown and divines some meaning from it. We sort the pieces and arrange them in new patterns.

When we stand before the tomb of Isaac Newton in Westminster Abbey, our sense of reverence stems not from his eminence as President of the Royal Society, or because as Master of the Mint he was so good at catching counterfeiters. We worship his memory because of that golden year in 1666 when, as a youth, exiled to the Lincolnshire countryside on account of the plague in Cambridge, he laid down, with the help of his own home-made calculus, the principles of theoretical mechanics. His

“Science is a voyage of intellectual exploration, and an expression of the human spirit.”

overweening sense of self-importance and his government service came afterwards. We have recently celebrated the twentieth anniversary of NASA’s Apollo Mission, one of the largest and most expensive planned technological feats of all time—yet I think it no exaggeration to assert that, in a basic sense, it actually was the blazing fire in the mind of the boy Newton that put those men on the moon.

Members of the Society, we are putting the fate of oceanography into your hands. We trust you will be faithful keepers of that flame. ❖

Barbara Gaffron

Woods Hole Oceanographic Institution



C hug up the rise with short strokes, there's the sedate red sedan. Smooth cruise down the hall and around the corner (it's a Rollerblade day). Up in the elevator with lingering cigar smoke: Henry has come this way.

What would there be this morning? A new trick puzzle whose solution would be announced with enthusiastic guffaws? An issue of *Soundings* or the *National Fisherman* advertising ships useful for short-lead-time research cruises, or a *National Enquirer* with discreetly indistinct photographs of the Loch Ness Monster (why didn't we see this on televised news last night)? Perhaps a colorful catalogue of fireworks available at a not-too-distant store in New Hampshire: I would park my car (definitely not a sedate red) a couple of blocks away (not a straight line) and be sure I had a "plain brown bag," get a nice supply of Roman candles, then continue upstate for weekend hiking. Upon returning to Falmouth, I'd go directly to Sippewissett Farm and deliver the cache. And listen on clear nights.

Or, would we be treated to an impromptu (for us at 0800) accordion recital? Henry would have been practicing for months in the sunroom or outside the house. We would see a strolling musician with that "unseeing" look of concentration and hear Hungarian polkas, Viennese waltzes, and English or French folk tunes, all played by ear.

Perhaps he'd bustle down the hallway, unfolding a map of Newfoundland that we'd pore over on the counter. He'd draw my attention to how much land was not crisscrossed by roads and how some coastal villages were connected by roads but gaps between others indicated passage by water.

Or, might I find a big bunch of giant zinnias, a bouquet of fragrant sweet peas, or a quart basket of red and yellow plums?

Something new every morning. ❖

Editor's Note: Barbara Gaffron served as secretary to Henry Stommel from 1985 to 1992.

Peter Niiler

Scripps Institution of Oceanography



I n early December of 1965, my postdoctoral advisor at Harvard told me that there was not a position for me on the Harvard faculty. I was crestfallen and called Hank Stommel at MIT about my predicament of being unemployed six months hence. He told me to immediately take a bus down Mass Ave. and meet him for lunch at the MIT faculty club. Hank arrived at the table with a motley, dog-eared manila file under his arm. After hearing my tale of woe over several glasses of wine, he handed the file to me with a comment that Bill Richardson and some graduate student named Bill Schmitz had sent it to him, but he really did not know what they were getting at in the enclosed material. He

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then added with a wink that, as a theoretician, perhaps I would understand better what they were doing than he had and that I should pay them a visit at Key Biscayne Oceanographic Laboratory of the University of Miami.

I was now further confused in my predicament, having fully expected the guru of oceanography to help me with a long list of famous university departments to which he would then give me personal introductions for a position which I thought I fully deserved. Instead, I had gotten an unsavory manila folder containing one piece of coffee-stained, blue-checked, graph paper. On it was a pencil-drawn abscissa depicting distance in kilometers east of Fowley Rocks and the ordinate was in units of Sv/km. A hand-drawn pencil line connected a series of black dots, first increasing and then decreasing with distance toward Bimini. On the bottom lefthand corner was a scribble that, upon closer scrutiny, could be taken as 32.4 Sv.

Hank had not waxed over the fact that this graph was of the first direct measurement of the surface-to-bottom transport of any major ocean current system in the world. The number 32.4 SV was the mass flux (in Sverdrup units) of the Florida Current between Miami and Bimini that Bill Schmitz had measured, knee deep in the bilge of a decomposing 32-foot motorboat using dropsondes invented by Bill Richardson. Over the next few weeks, while preparing for my trip to Miami, the facts on the coffee-stained paper dawned for me with the full force that Hank knew revolutionary facts of science would. I had been handed a jewel of scientific data in disguise and the possibility of joining a most exciting, but very unconventional, research group in the tropics.

That was his way; to be seemingly irreverent in the presence of what he knew was considered sacred by others and to be unimpressed by the normal academic ways of doing the business of oceanography. With this simple act of showing me a jewel of scientific data in disguise, he also sent me to live for eight years in the swamps of Florida and work on a small houseboat laboratory in Ft. Lauderdale. He could not have given a better gift from the real world to a young theoretician. ❖

Valery Lee

World Meteorological Association



What do you say to an aspiring oceanographer (female) who goes to sea in dresses?

Become a lawyer.

That's what Hank's advice to me was back in 1973 as we mused over my future, drinking gin and tonics on the fantail of an old cable layer sailing around the Bermuda Triangle in search of eddies.

From the first day he interviewed me as an MIT undergrad applying for a job during the January Independent Activities Period, Hank and I hit it off. We both liked to laugh about the same sort of things, and I was always game for one of Hank's pranks. One of my favorites was the light-bulb trick. I was often invited to the Stommels' cozy old farmhouse in the evening or on the weekend for a beer and a friendly chat. Usually we sat around the kitchen table, but on this particular day

He could not have given a better gift from the real world to a young theoretician.

Hank had another guest, and he ushered me into the parlor. He said, "It's a little dark in here, Valery. Could you please turn on the lights?" So I proceeded dutifully to switch on the many lamps. However, each time that I turned another one on, the room seemed to get darker! Hank had screwed light bulbs into the fuse sockets, which meant that as each additional lamp was lit, the output of all the others dimmed.

On another visit, we were sitting around the kitchen table and the doorbell rang. Hank came back all excited—there was a very distinguished-looking gentleman with a beautiful antique car asking after me out front. Hank was always concerned about my personal life as well as

His Trophy Presented Problem In Logistics

Oceanographer Henry M. Stommel came back from the 11th Pacific Scientific Congress at Tokyo recently with a stuffed albatross in a cage that accompanied his citation by the American Miscellaneous Society.

It is quite a trophy.

The Albatross Award of the American Miscellaneous Society was first awarded in 1959, the year the society was founded. The award went to the three founders "for conceiving the idea."

These charter members were Dr. Arthur E. Maxwell, assistant director of the Oceanographic Institution at Woods Hole; John Knauss, dean of oceanography at the University of Rhode Island; and Gordon Lill of the Office of Naval Research, director of the late Mohole project.

Someone dug up the stuffed albatross out at the Scripps Institution at La Jolla, Calif. It is a rather handsome bird at that, twice as good as a husky sea gull, with a puffy white chest, a cocked head and a beady eye. The cage he came back from Japan in is lightly built of wood and wire.

After that first year, the founders broadened their view and gave the Albatross Award out of the society to Walter Munk of Scripps, to Harrison Brown, professor of geochemistry at California Tech and former foreign secretary to the National Academy, to Victor Vacquier of Scripps, discoverer of a fault in the floor of the Pacific, to John Swallow, the distinguished Briton.

Mr. Stommel, the latest recipient, had received international attention for his discoveries and theories regarding circulation of the oceans.

Mr. Stommel received the award, according to his citation, for "abandoning the cherished chairs of oceanography," which perhaps reflects oceanographers' envy of Mr. Stommel's independence.

A tradition growing up about the seven-year-old Albatross Award of the American Miscellaneous Society is that it be presented in an out-of-the-way place where the recipient will have the maximum difficulty bringing his prize home.

This is the reason the sponsors waited for the occasion to present the award to Mr. Stommel in Tokyo. Dr. Vacquier had crossed the Pacific on one of the Scripps vessels, bringing the albatross with him. It was up to Mr. Stommel to get it back.

my professional one, and he would do his best to make encouraging remarks if someone came to the office to invite me to the movies or dinner. This time, however, it turned out that the gentleman in question was interested only in my vintage Mercedes, which he had seen parked in the Stommels' driveway.

Hank had a myriad of projects all going at once, and he always found something interesting for me to do. After graduation, I stayed in his employ to produce an atlas of data from one of the first "big science" physical oceanography experiments, called Mid-Ocean Dynamics Experiment (MODE), which Hank had been instrumental in conceiving

Mr. Stommel, who had reservations by air, might have been apprehensive at the very start of the trip. At Tokyo airport he was stopped from carrying the bird on the plane.

"It's an animal," the official said.

"It's not alive," added Mr. Stommel defensively. A superior had to be summoned. It ended up that the bird was given special handling in the plane's baggage compartment, which still left the problem of getting it through the Customs at San Francisco.

But at San Francisco, it couldn't have gone more smoothly. "Hey, Bert, we don't want to see this, do we?" the examining Customs officer called to a colleague.

"No," said Bert. The bird went through.

Nor was there any problem on the TWA plane that brought Mr. Stommel and his prize across the country. The albatross in its cage is, said Mr. Stommel, about "seat size." The bird sat beside him all the way.

The albatross now decorates the living room of Mr. Stommel's house in Sippewissett. Mr. Stommel travels a great deal. Next recipient of the Albatross Award may find it being handed to him on the far side of the Gobi Desert or some equally remote place.

At the 11th Pacific Science Congress in Tokyo were several from Woods Hole. L.V. Worthington read a paper on the Kuroshio Current, which is the Pacific's Gulf Stream. Jonathan Leiby and Frank J. Mather 3d of the Oceanographic Institution were there. ❖



Oceanographer Stommel and the Albatross

The Enterprise
September 9, 1966

Not Afraid Of Life

by Starbuck

ON THE AFTERNOON of Jan. 7, 1975, the wind came up in the Gulf of Maine. By evening it was from the northwest at 40 knots. The water temperature was 40 degrees. The little research vessel *Gulfstream* was unable to radio for help and foundered with all on board. During the next few days an extensive search recovered the body of one of the crew of five and a few bits of flotsam. The cause of the tragedy remains unknown.

Among those lost was William Richardson, a good friend of many at the Oceanographic since the early 1950s when he came to Woods Hole with a fresh Ph.D. in physical chemistry from Harvard. Bill first worked on the development of infrared sensing of ocean surface temperature from aircraft and later on the early stages of moored current meter instrumentation. His unceremonious and informal ways earned him the friendship and respect of those who worked with him. He was also independent and outspoken, by no means a tame spirit. In the early 60s he set up a new lab at Nova University where he could be his own boss and undertook an outstanding series of measurements of the fluctuations in strength of the Florida Current.

As a naval aviator at Midway, Bill had experienced the anguish of seeing his entire squadron wiped out in battle. He claimed that from that time he could never again take seriously the petty worries that bedevil most of us: financial security, job advancement, health, academic prestige. He tended to discount his own personal safety. When a crew member fell overboard on one oceanographic cruise, instead of calling to the helm or throwing over a lifeline, Bill plunged instantly into the sea to rescue him: rare courage.

Bill found people endlessly entertaining; he liked adventure and working at sea and in aircraft and the kind of person one finds there. He had seen death so close that he was not afraid of life. ❖

The Enterprise
January 7, 1985

and designing. At the same time, Hank had a National Science Foundation grant (with Ants Leetmaa of the Atlantic Oceanographic and Meteorological Laboratories) to take current profiles across the equatorial Indian Ocean from a locally built and operated schooner in the Seychelles. By the time the project was in its second year, Hank had exhausted his entire stock of students, technicians, and scientific collaborators willing to go on this romantic venture. He still needed someone for the last three months, and there I was, willing!

Hank was always encouraging, and willing to give people a chance, especially if they were inclined to take a lot of initiative and free him up to tackle the problems he found truly interesting, which did not include the nitty gritty of managing a field experiment on the other side of the globe. He warned me about driving in the Seychelles (his own experiences having resulted in unscheduled encounters with Arabs behind beaded curtains) and sent me on my way. I got to the island to find that the boat had just left, the captain had quit, and I had no place to live. From there started a wonderful adventure, and everything worked out, even the data gathering.

Hank continued to contemplate what would be the best use of my particular talents, and one of his ideas was that I should work for a United Nations organization, and start up an oceanographic equivalent of the World Meteorological Organization's World Weather Watch. Now (many, many years later) I work for the World Meteorological Organization, and it is a measure of Hank's prescience that the first steps toward a Global Ocean Observing System have recently been taken.

Hank was many things to many people. I will always remember him as the first oceanographer I ever met, a good friend and guide, and the one who infused me with an enthusiasm for oceanography I have never been able to shake. ❖

Robert Heinmiller

OMNET, Inc.



I came to WHOI in 1962, right out of MIT with my BS in hand, knowing nothing of oceanography. It seemed like a nice place to spend a year learning to sail. Hank Stommel was one of the people who introduced me to the wonders of oceanography and was responsible for the fact that, 14 years later, I was still there.

In late 1976, I was commuting from Boston to Woods Hole every day, getting ready to move to a position back at MIT. Knowing that I was driving to Boston every evening, people would send graduate students and other itinerants who needed rides to me.

One day in the fall, I was driving up Route 3 with a student who had graduated from a midwestern university and had arrived to enroll in the WHOI/MIT Joint Program. He had just been to Mecca for the first time. He went on at length about how thrilling it was to see the ships and the famous labs.

And, then, he said, breathlessly, "And...and do you know who I met?"

"No," I replied warily, wondering what eminent notable had been in town, "Who?"

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We're Over The Ridge!

by Starbuck

IN THE EARLY 50s the IRS was trying to decide whether the National Geographic Society with its ten million members could really qualify as a non-profit scientific society. "Doc" Ewing thought it a favorable moment to ask them for some research funds to sample the ocean bottom near the Azores. Shortly thereafter the magazine carried a colorful article about his success in scooping up some rocks there, and a salty photo bearing the legend "We're over the Ridge!" Recovering anything at all from the ocean bottom impressed us greatly.

Some of us who were wondering whether we could place instruments on the ocean bottom and recover them by dragging thought it might be worthwhile to consult a cableship captain. Our friend, the Marine Superintendent for Undersea Cables at Western Union, Mr. C. S. Lawton, arranged a visit to the cableship *Lord Kelvin*. Three of us from Woods Hole flew to Halifax, were duly impressed by her size, which dwarfed anything we knew, and ushered into Captain Beady's (as I will call him) cabin.

A small, delicate martinet of a man with, as I remember, manicured nails, Captain Beady politely had us seated and introduced the subject of golf. He must have run a taut ship, because as we sipped his sherry, the seamen, scrubbing down the bulkhead outside his open door, kept out of view: all we could see were their hands with the sponges.

We were interested in the type of grapnel he used, ones that could cut a cable and clamp onto the end desired, even if they tumbled over while being dragged along the bottom. Instead of taking us down to the weldeck he had the crew manhandle them up to his cabin and lay them out for our inspection on an immaculate canvas. It was a lesson in command.

Before we departed Captain Beady enquired whether we were associated with the place that operated the *Atlantis*. He had read the *National Geographic's* article. And he had been ordered out to the mid-Atlantic in winter to repair one of the transatlantic cables. Sending the mate to copy out the position of the splice, he said the cable had been probably cut by a rock dredge.

Upon our return to Woods Hole we found in the *Atlantis's* log that she had lost a dredge at that very position. It caused us to reflect upon the professional precision of his repair as contrasted to the amateur nature of our random scooping up of rocks. It might have been polite to write to him and confirm his guess, but somehow we didn't think the redoubtable Captain Beady really needed to know. ❖

The Enterprise
March 25, 1985

"Professor Henry Stommel," he said, in tones usually reserved for the beatified.

I stared blankly at him for a moment, and then blurted out, "Oh, you mean Hank!"

I am sure that that graduate student eventually became more comfortable around Hank. I am not sure whether Hank would have been amused or pained at the idea that anyone stood in that kind of awe of him. I don't think I ever took Hank's stature, intellect, and importance to science for granted. But it was perhaps useful to be reminded that this wonderful, accessible, friendly person wandering around Woods Hole was also the famous oceanographer "Professor Henry Stommel."

The world was fortunate to have had Hank. Those of us who worked with him and knew him were incredibly lucky. ❖

William Schmitz

Woods Hole Oceanographic Institution



Hank was a terrific person, colleague, and friend as well as a great oceanographer. I came to Woods Hole to a large extent to be around such an original and free spirit, and especially enjoyed our early-morning coffee breaks together throughout the years. All topics were fair game, although matters related to physical oceanography were most likely to come up. Hank was a mentor for a lot of us, a warm and brilliant light in our life. He will be deeply missed. ❖

Harry Bryden

Woods Hole Oceanographic Institution



In the early 1970s, Henry Stommel was organizing the Mid-Ocean Dynamics Experiment (MODE) program and commuting to MIT nearly every day. At the same time, the WHOI-based Joint Program graduate students organized a station-wagon commuter run to MIT for courses every Monday, Wednesday, and Friday. Each day we would pick up Hank at his house at 6:30 a.m. and head for Boston. Many of the students slept all the way there, but those of us who were wide awake got an oceanographic education unequalled in any curriculum. We learned how the National Science Foundation and Office of Naval Research worked, what was going on in MODE, what the upcoming International Decade of Ocean Exploration programs like International Southern Ocean Studies and Geochemical Ocean Sections Studies would be doing, how to get tenure. Hank discussed them all not in specific or personal terms but in a general philosophical approach that taught one how to frame practical solutions and approaches within one's own philosophy of science.

We had traffic problems, of course, on the Southeast Expressway. We utilized breakdown-lane driving before it was fashionable (legal); we developed a route through the neighborhoods of Boston so copied

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that the city eventually eliminated it by reversing the one-way nature of a critical street to make it impossible. One day, Hank showed up with a sign reading “This is not our car” for the car window; he was convinced it would keep the Boston drivers away from us as we repeatedly changed lanes on the expressway. The WHOI car managers, however, removed the sign before we could adequately test its effectiveness.

Hank always brought scientific problems along. Why does the sun set earliest on December 8th instead of on December 21st, he asked as he was reading the *Farmer’s Almanac* one afternoon. I would wonder for days about the answers, and it was more fun to wonder about the questions than to look up the solutions. One day, Hank began talking about traffic flow as being similar to water flow: When the traffic density becomes too great, disturbances propagate upstream against the flow and the transport of cars decreases. Until the end of the term, we tabulated traffic statistics, estimating density and speed under various conditions, trying to identify the critical density for maximum transport. I thought it was an interesting problem—until it showed up as one of my general examination questions, and then I felt foolish for not looking into the theory in more detail.

Commuting with Henry Stommel weekly over two years for three hours a day was one of the most remarkable experiences of my graduate education. I learned much about what was going on in oceanography, about the fun in asking and trying to answer interesting questions, and about the importance of maintaining a philosophy of science.

I once tried to explain to a Woods Hole faculty member about how much I had learned from Hank in the course of commuting, and he decided Hank should give a formal course so he and others could learn as well. But it proved impossible to bottle what Hank had to teach. You just had to be there with him to absorb the curiosity, the deep philosophy, and the joy of doing oceanographic research. ❖

Joseph Reid

Scripps Institution of Oceanography



Leg 1 of the SCORPIO expedition (*Eltanin* cruise 28), along 43°S from Melbourne to Valparaiso, was 54 days long, from March 12 to May 5, 1967, though there was a half-day stopover in New Zealand to let Manik Talwani (Lamont-Doherty Geological Observatory) off. This was to be one of the longer lines of stations ever made. With one set of gear, techniques, reagents, and people all the way, the Aristotelian unities of time, personnel, and place would be preserved: The data would have the virtue of internal consistency. It occurred to us that the geochemists, who had been collecting their own particular data in bits and pieces here, and using a range of different techniques and sampling schemes, might profit by getting together and carrying out such a line of stations in a more coherent fashion. We (Henry Stommel, Bruce Warren, and I) thought of perhaps a single long line from the north to the south end of one of the oceans, and decided that Henry should suggest this to the proper people. He did, and the result was the Geochemical Ocean Sections Study expeditions.

The *Eltanin* had brought along a lot of movies. Not all of them were prize winners, but they were accepted with some wry good humor. One B-grade horror movie had as its subject a human hand, severed but still active, that committed various crimes. The concept intrigued Henry. He found a large heavy-duty glove and put into it a device of his own creation that made it contract and jump at irregular intervals. He put it on a table in front of the movie screen and when the lights were darkened for the fourth showing of the horror movie it began to clench its fist and move around. After it was all explained, and the creator identified, the first mate, a rather proper fellow, looked at Henry and then turned to me and asked, "Is that the great Stommel?" It was, of course, as much the real Henry as anything else he had done. He was a grand shipmate. ❖

W. S. (Wally) Broecker

Lamont-Doherty Geological Observatory



A story I heard but can't vouch for is that during a GEOSECS isotope discussion held at WHOI's Carriage House circa 1971, Hank, dressed as a witch, crawled up the aisle, reached up a gnarled hand, and wrote on the blackboard

$$K = 0$$

then, without a word, he disappeared.

On a more serious note, it was Hank who launched GEOSECS. Ed Goldberg (Scripps Institution of Oceanography) and I were attending some sort of meeting at WHOI during the late 1960s. Hank came to us and said that radiocarbon measurements in the sea were of great importance. He went on to gently chastise us (the geochem community) for doing only scattered stations.

"What is needed," he said, "is a line of stations extending the length of the Atlantic."

"Gee," we said, "that would cost a million dollars, a sum greater than the entire NSF annual budget for ocean chemistry!"

Hank replied, "Well, it would be worth more than a million."

He spurred us to propose such a venture. Soon plans were being formulated not only to do carbon-14 but also a host of other chemical and isotopic properties along Hank's Atlantic line. Boosted by the appearance of Department of Energy monies, Hank's dream became a reality that ultimately covered the entire world ocean and cost NSF \$25 million! ❖

Peter Rhines

University of Washington



My most compelling memory is of gales of Hank's laughter echoing down the halls of the Oceanographic. He found fun in chasing and discovering ocean problems, and in all the mad adventures of life at Sippewissett Farm. That high state of whimsy came from a fresh view of life, just as fresh as was his approach to oceanography. ❖

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Henry Stommel—On the Light Side

by F.C. (Fritz) Fuglister

This is an excerpt from a contribution to *Evolution of Physical Oceanography: Scientific Surveys in Honor of Henry Stommel* (MIT Press) edited by Bruce Warren and Carl Wunsch and published in 1981 in celebration of Hank Stommel's 60th birthday. It is reprinted with permission.

DURING THE 1940s Stommel went to considerable effort to bring oceanographers together, using his own car to drive groups to Brown or Cambridge from Woods Hole. His car was old and he was a very careful driver, but the conversation was animated and the more spirited the discussion became, the slower the car moved. I remember, after one such trip to MIT, Carl Rossby, stepping out of the car smiling and stretching his arms wide, sighed, "It is hard to believe that only this morning we left Woods Hole."

It was on this same trip that we all had a thrilling experience. At one point, about halfway on our journey, we were amazed to find that we were behind a car that was traveling even slower than we were. The excitement ran high as Hank pulled over into the left lane and drew up abreast of the slow vehicle: He was going to pass him! Then we all saw who was driving the slow-moving car. It was George Veronis. He had a smile on his face as he waved to us, his thumb to his nose, and sped off for MIT. That is the closest we ever came to passing another car.

Fortunately, other cars and other drivers came along, and this regular movement of oceanographers back and forth, started by Stommel, continued and flourished; today, I understand that even going as far away as Yale is not considered unthinkable.

Over the years, Henry Stommel of course did more than drive cars....He was the first President of SOSO, and he is the Special Committee for The William Leighton Jordan, Esq., Award. Because of his retiring disposition, Hank has managed to keep these activities more or less secret.

In 1961, or thereabouts, L.V. Worthington (Val) showed me an item in an ONR newsletter about a new laboratory in Europe that was to be staffed by eight Ph.D.s and fifteen subprofessionals. "That is what we are," he said, "subprofessionals," and I had to agree with him. By the same token, we both realized that it made Stommel and others subprofessional, an astounding thought! That same evening I asked my young son to set up type for a letterhead for the Society of Subprofessional Oceanographers (SOSO). He asked me whether the society would have any administrators; I shuddered at the thought but I said, "Oh, make Hank President, me Vice President, and Val Ambassador to the Court of St. James"; so he did and that was the birth of SOSO. Since the society never holds meetings or keeps any records, we don't know what that birth date is.

We know that in 1964, when Henry was off in California receiving the Sverdrup Medal of the American Meteorological Society, I, in a bold coup d'état took over the presidency of SOSO.

Stommel joined his membership in SOSO to his fascination with the nineteenth-century English amateur, William Leighton Jordan, in the following announcement, issued some years ago:

**Society of Subprofessional Oceanographers
Special Committee
for
THE WILLIAM LEIGHTON JORDAN, ESQ., AWARD
Announcement**

The William Leighton Jordan, Esq., Award is given annually to the Oceanographer who makes the most misleading contribution to his field. Ignorance and utter incompetence do not automatically qualify. The work cited must be distinguished not only by being in error, but it must be outstandingly bad: wrong both in principle and fact, and revealing the most mistaken intuition and the most faulty insight. It should be over-ambitious, and exhibit egregious error—willfully artful, well and plausibly presented, and totally misleading and false. It is not expected that every calendar year will be graced by so grand and profoundly negative an achievement deserving of this award commemorating our most illustrious and our deadest member. The author of many theoretical works on ocean currents, a fearless critic as in the pamphlet entitled: “The Admiralty Falsification of the *Challenger* Record, exposed by William Leighton Jordan, Esq.,” has few peers indeed. But the members of this Special Committee will remain alert to commemorate the truly deserving oceanographer with this newly established award.

Stommel and SOSO have never quite had the nerve—despite several temptations—actually to bestow this award on anyone. ❖

Editor's Note: Fritz Fuglister (1909–1987) studied music and art at the Washington (DC) College of Music and the Corcoran Gallery of Art School. He continued painting studies in Provincetown, Massachusetts, did WPA commissioned paintings, and sold easel paintings. In 1940, of economic necessity, he was hired for a series of R/V *Atlantis* cruises to Georges Bank. He went on to become a highly respected oceanographer, winning the National Academy's Agassiz Prize in 1969.

Valentine Worthington

Woods Hole Oceanographic Institution (retired)



Many years ago, an article appeared in the European Scientific Note of the Office of Naval Research that described a laboratory, in Germany, I think, as being staffed by five Ph.D.'s and nine subprofessionals. (I am hazy about the actual numbers.) I brought the article in to Fritz Fuglister's office at coffee time, and told Hank Stommel and Fritz that the three of us were clearly "subprofessionals."

Hank immediately founded SOSO, the Society of Subprofessional Oceanographers. Hank got a lot of fun out of SOSO. I recall his amusement when Georg Wüst, an archetypical "Herr Doktor Professor," sent a preprint of his treatise on the results of the *Meteor* Expedition to Woods Hole. It was inscribed: To the Physical Oceanographers of Woods Hole—Stommel, Fuglister, and Worthington. Wüst had actually selected the members of SOSO to be the first American recipients of his life's work.

Wüst, of course, had put the members of SOSO in the correct order. Fritz's work was sometimes more aesthetically pleasing than Hank's; after all, Fritz was an artist. However, Hank's work had a scholarly dimension that was lacking in Fritz's and mine.

After Fritz died, I asked Hank if we should not invite a prominent unhooded, western oceanographer to join us.

Hank said, "No, let's keep it exclusive."

As the sole surviving member of SOSO, I hereby declare there is no further purpose for its existence and that the Society of Subprofessional Oceanographers is dissolved. ❖

Cecelia Fuglister

Woods Hole, Massachusetts



Influenced by his friendship with Fritz, Henry at one time became quite interested in trying his hand at painting.

Fritz gave him a fair number of tubes of oil paints, surplus from his large store, unused because Fritz had adopted tempera for his own medium. That year, a Christmas present arrived for the Fuglisters from Hank, beautifully wrapped in papers of the season. It was a small painting by Hank, very amusing because of its odd geometrical shape and intense colors.

A certain place on one of our walls has been enlivened by this little painting for a good many years. ❖



This painting is from Hank Stommel's "mouse period."

Shirley Webb

Sippewissett, Massachusetts



Sippewissett Corner is much quieter and sadder, now. Two good friends and neighbors, Hank Stommel and Bob Weeks (see editor's note, page 79), are both gone, and we miss them every day.

Around 7:15 a.m., I still expect to see Hank's red car trundling down "the back road" to work, and home again for lunch.

The evenings are more predictable without the exciting volley of firecracker bangs, with which Hank would often amuse himself—or the startling wail of the Canadian Pacific train whistle that he ran on a tank of compressed air.

One of the interesting sights of spring might be Hank climbing a ladder in his shorts (made by Chickie from an elderly suit) and wearing his "Peter Rabbit" slippers, which were fetchingly checkered and fuzzy, and most unsuitable for climbing ladders, or strolling over for a visit.

Some years ago, Hank appeared at the Stommel Christmas party in a rather alarming, pale-blue, 1940s "zoot suit," genially passing drinks out—and obviously happy and relaxed in his sartorial elegance, amongst neighbors and friends.

He and Chickie were always friendly and generous hosts, and to us, part of the fun was to admire Hank's latest works of art in the kitchen. Over the years, he went through various phases of expression—although the hand-painted tropical foliage on the Stommels' refrigerator remained the same. One year he had amused himself (and



Hank painted this sign for his Sippewissett Farm.

The Cape Codder Hotel was a landmark in Hank Stommel's neighborhood until it was torn down to make way for condominiums a few years ago. He imagines it on fire in this "disaster" painting.



everyone else) by imagining "Disasters" and producing dramatic murals of "Bourne Bridge Falling Down," "The Cape Codder Hotel on Fire," and "Woods Hole Flooded by Raging Seas."

After living in France for a time, the mural over the kitchen fireplace depicted a very French street and market scene, with houses, stalls, and people. He had ingeniously wired small lights in the win-

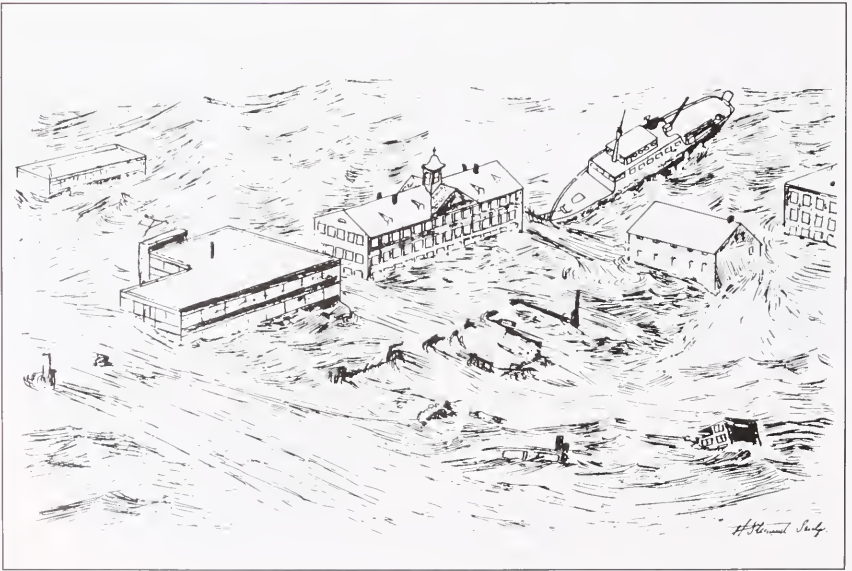


dows. The “mouse period” (perhaps influenced by Chickie’s beautiful hand-sewn toys) showed amusingly dressed mice in homelike surroundings.

Hank would often appear at our kitchen door at unlikely times, with a book, an idea, or a bag of fruit or vegetables—usually in his slippers or slip-on canvas shoes (often wet). He would sit and visit, then disappear abruptly, thinking a new thought, probably.

Sometimes he would request that we collect the mail, while he and Chickie went traveling (which he disliked, as

Hank Stommel's gallery of artwork includes a French street scene, a circus wagon from the “mouse period,” and Woods Hole inundated from the “disaster period.”



he preferred home and garden). One of these trips, we knew, was twofold. On a Monday, in London, he was to become a Fellow of the Royal Society, and on the following Wednesday, in Sweden, the King would present him with the Crafoord Prize.

I was very curious to know how on Earth he handled the possible need for very formal clothing at such important gatherings. I asked him if he had to pack an evening suit, and he replied happily, "Oh no! I just

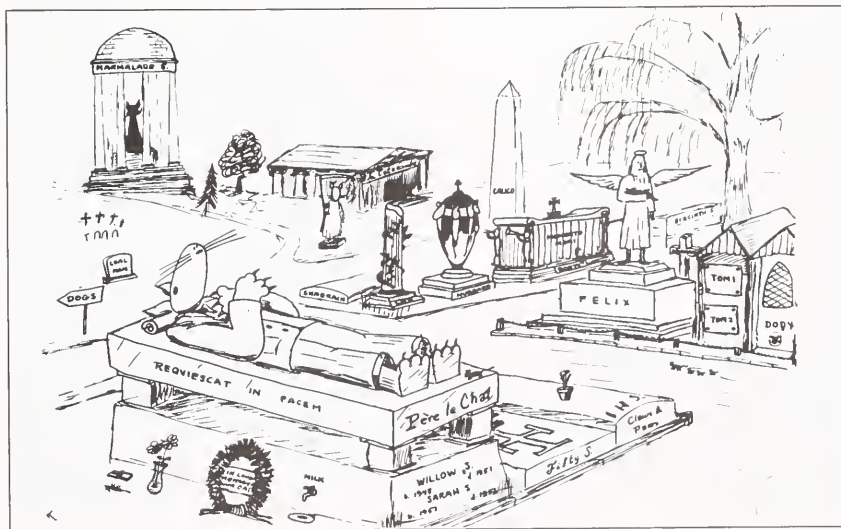


went down to the hospital thrift shop and found a nice, navy suit for 15 dollars. It will do very well!"

So, our memories of Hank often bring a smile, which he would consider a fine memorial, I know. ❖

Editor's Note: Shirley Webb is a Stommel neighbor and the wife of ocean engineer Doug Webb, who often worked with Hank. Bobby Weeks was a 40-year WHOI employee, the mechanical shop supervisor for many years. Many horses are boarded at Bobby's farm, adjacent to the Stommel property. Bobby died six months before Hank. It was Bobby's father-in-law who drove the ambulance that took Chickie Stommel and her brother to Boston in the mid-1930s (see "Counting Our Blessings," page 111).

Animals figured prominently in Hank Stommel's paintings! The names on the tombstones all belonged to Stommel cats.



The Ocean and William Leighton Jordan, Esq.

A closing note to this biography says: "This sketch is based upon research done during the years 1958-1962 by Luis Capurro, Henry Stommel, and Mary Swallow. In the spirit of Leighton Jordan's inflexible custom, it is privately printed. —October 1980"

DR. PEDLAR, THE CORONER who conducted the post-mortem examination of the body of William Leighton Jordan, reported at the inquest on May 23, 1922, that the cause of death had been heart failure, and since it appeared that the deceased had no friends, asked, through the medium of the Press, that any relatives communicate with him. *The Evening Standard*, which reported the inquest under a headline "Lonely Savant's Death," located a solicitor who had acted for members of the family, and a sister living in Bristol, too old to travel. A niece came to London to arrange the funeral. A literary friend of many years standing had little more to tell but that the 86-year-old Jordan had written scientific books about the ocean, about principles of natural philosophy, and the case for bimetalism. This friend remembered that one of the deceased's greatest sorrows was not having been appointed manager of a bank in Buenos Aires, and that one of his happiest memories was having, at the age of 15, heard Faraday lecture at the Royal Institution—an Institution to which he remained loyal to the very end, having attended a lecture there the very day before he was found dead in bed in his residence at 25 Hill Street, Mayfair.

From these few who knew him, it was clear that he had never said anything about his private life, he lived and dressed well, giving the impression of being comfortably off.

We confess to being peculiarly fascinated by Mr. Jordan. From time to time, students browsing through the stacks of reference libraries came upon mouldering copies of his books. Being the works of an eccentric, they are completely ignored in all

reputable literature. They were first written at a time of great interest in the deep ocean: telegraph cables were being laid across oceans, and strange living creatures fished up from depths which until recently had been generally thought to be without life. If Jordan's pre-Galilean physics was absurd, it cannot be claimed that his professional contemporaries in the 1870s were able to make much use of correct hydrodynamical theory in their thinking about ocean circulation. One need only read the acrimonious correspondence which raged in the columns of *Nature* during the two years previous to the sailing of the *Challenger* Expedition in late 1872 to appreciate that the professed experts themselves were quite confused. The overbearing Dr. William Carpenter engaged the erstwhile janitor of the Anderson Technical College, Mr. James Croll, in a heated exchange of letters debating whether the ocean circulation was due to density difference or to wind-driving—and it became so pusillanimous that even the protest of an Anonymous Eminence (whose identity must remain forever unknown because the magazine's files were destroyed in World War II) was to no avail. It is no wonder that poor Jordan was mercilessly cut down.

But he was a man of great resilience and energy, and though rebuffed countless times, managed to badger academic oceanographers, and carry on a campaign to enlighten the Admiralty for another 50 years. What kind of man was he to pursue his personal vision of how the ocean works for so long with so little encouragement—except at the very beginning it seems when A.G. Findlay did encourage him.

He was born in Rio de Janeiro on June 4th, 1836 to Swinfen Ker Jordan, merchant from Manchester, and Mary Louisa, daughter of William Pennell, H.M. Consul-General in Brazil. Until the age of 12 he lived there, and then was sent to a school near Manchester for four years, during which time he once listened to the great Faraday, and underwent the emotional experience which supported his life-long interest in natural philosophy. There followed two years' work under a tutor. At the age of 18 he entered his father's business, but evidently was so engrossed studying science, without any outside assistance or guidance, that he must have neglected his business duties so scandalously that his father suggested that he retire altogether from business at the age of 27 to devote his life to science.

AT ONCE HE embarked upon a world tour, in the spirit of von Humboldt. He crossed the Andes on foot, and traveled through much of California, and mountaineered in the Alps from Breuil and Zermatt. And all this time his brain was teeming with a theory about the causes of ocean currents and tides, built upon what to him was to become an *idée fixe*: that Newton's first law was erroneous, that momentum is not conserved, but always runs down.

Jordan's misconception about particle dynamics is well illustrated by his discussion of a thought-experiment proposed by Newton. Newton envisaged a mountain high above the atmosphere upon the summit of which a cannon was placed to shoot a ball horizontally. Common sense tells us that the greater the velocity, the further the ball will go before it strikes the surface. The genius of Newton recognized that at a certain muzzle velocity the ball would circle the earth forever. Jordan preferred common sense—but we should remember this was in the days before artificial satellites became familiar—and he thought Newton had made an illogical leap of

imagination. It was this idea that things must run down that Jordan tried to illustrate by deducing from it various features of the ocean currents and tides. He could hardly have chosen a more puzzling and controversial example.

In his view the fluid envelope of the earth was swept westward by its being unable to keep up with the earth's rotation. Since this tendency to fall behind is strongest at the surface and at the equator, and since the oceans are blocked from indefinite westward flow by continental land masses, the net result would be a series of cellular swirls, vertical and horizontal, similar to the patterns actually portrayed on charts. Where they were not similar, he contended, more careful surveys were called for. It was a theory in which the curl of his "vis-inertia" played the part of our present-day curl of the wind-stress.

Within a short time he had a paper on ocean currents ready for the Royal Geographical Society (in 1866) and another on tides for the Royal Society (in 1868) and he took up residence at 24 Eastbourne Terrace, London, to await the result. As was inevitable, they were both rejected. Sir George Stokes, who had the unpleasant task of refereeing the paper on tides, wrote: "We all recognize that your friend is just the man to be encouraged to stick to science, but his views are so directly at variance with what we know to be true that no serious society can have anything to do with them."

BEFORE THE YEAR was out Jordan had returned to South America, this time to Buenos Aires, where he lived for some years with occasional visits to England. In view of the rejection of both his papers he had them privately printed, and in 1874 Longmans published *The Ocean, its Tides and Currents and Their Causes*. It is this book, in one of its several editions, which one sometimes stumbles upon in a dusty library corner. When the reviews came out, they were disastrous—and because it was

a time when public interest was high, even South America was not a secure shelter from ridicule.

Just what he did with his time in Buenos Aires is not clear. At one time later in his life he claimed to have been managing director of a railway construction company, and hinted that he was in the banking business. The hydrographer of the Argentine Navy, in 1962, searched around Buenos Aires for information about Jordan. Families of that name still living there did not know of him, nor did the British Legation. The English Club had no knowledge of his existence, nor did the Argentine Engineering Society. There was nothing in the Central Police records. A large correspondence to the Editor of the English language newspaper *The Standard* in Jordan's name was located in the National Library, but the files of the newspaper itself were consumed in a fire before the first World War.

IT WOULD APPEAR from the correspondence in *The Standard* that Jordan carried his scientific controversy to South America with him, and engaged in relentless debate with the president of the Engineering Society and with a pseudonymous Dr. Zeb. When editors gave sign of tiring of the stream of letters from Jordan, an unknown Mr. "Y" implored them not to cease publishing them: (January 21, 1879) "His letters are most interesting and easily understood by a little study, but in this hot weather one must take his coat off while reading them." The reader today will see clear signs in the carping tone of Jordan's letters that he felt there was a conspiracy amongst scientists against him, and he must have felt that he should have a little recognition at a time when the world was celebrating the successful return of the *Challenger*.

Sometime shortly after 1874 Jordan wrote the article on Argentina in the Ninth Edition of the *Encyclopedia Britannica*—an article which is both surprisingly liberal

and sympathetic to the plight of the Indians and peasant gauchos, and for a man of Jordan's blunt diplomacy, uncharacteristically prudent in describing the regime in power at the time.

IN 1877 he made a short visit to London, and in November 1877, in Willis's rooms, delivered a challenge to all Fellows of the Royal Society and the staff of the *Challenger* Expedition. While on this visit he decided that the Hydrographer Evans had purloined an idea of his about tides, and when he called upon him at the Admiralty he found the Captain's conduct to be "somewhat mysterious." Back in Buenos Aires his correspondence with editors burgeoned, and much of this he had printed in a book entitled *Skirmishes in the Vanguard of Science*, which he seems to have distributed privately. One hopes that this acrimony was relieved a little in 1880 and again in 1881 when he was elected President of the English Literary Society (the Society seems to have been last heard of in 1922—the year Jordan himself died). At last he found, in the little expatriate British group, a receptive and appreciative, if uninformed, audience—he must have relished it.

But by 1887 Jordan was back in London, and it seems this time, for good. He moved about from 5 Gordon Street to the St. George's Club, Hanover Square, then to 25 Jermyn Street, the Thatched House Club, St. James's Street, and the Royal Societies Club. He made valiant attempts to join the establishment, was elected to the Institute of Civil Engineers in 1887, the Royal Statistical Society in 1888, and the Royal Meteorological Society in 1897—they thereupon received copies of his printed works for their libraries. He also became a Life Member of the Royal Institution in 1896, where he is remembered (1962) by Mr. Cory, the Librarian of the time, "as a very regular reader in the library. He had a very pleasant personality, rather quiet. He spent

Skirmishes
in
The Vanguard of Science

August 1880 to February 1881

by

William Leighton Jordan,

President of the English Literary Society
and

Fellow of the Royal Geographical Society

Buenos Aires

Imprenta a vapor de Juan H. Kidd, Calle Corrientes 117

1881

whole days consulting books and dealing with his pet subjects, ocean currents and tides. I think his views and theories were rather unorthodox and I recall he had a long correspondence with the Editor of *Engineering* who eventually had to say 'this correspondence must now cease!'

IT WAS AT THIS period of his career, when he had just returned to live in London for the remainder of his long life, that he made his most determined attempt to gain recognition of his views, by attacking his tormentors in a campaign to expose "The Admiralty Falsification of the *Challenger* Record." The 35-page printed version of correspondence between him and the authorities associated with the *Challenger* Expedition concerned what he claimed to believe was a deliberate falsification of the observed temperature data, but seems more likely was a device to procure attention.

In his first salvo of November 8, 1887, aimed at the Lords Commissioners, the Admiralty, Whitehall, Jordan explains how his interest in the vertical structure of temperature in the ocean led him to take a keen interest in the temperature measurements of the *Challenger* Expedition. His paper on ocean currents, rejected in 1866, had predicted that because the driving force of ocean circulation was unrelated to density convection there would be many thermal inversions and interleavings observed,

whereas conventional wisdom had predisposed other scientists to expect that the temperature would drop monotonically with depth. He described how the Preliminary Reports issued during 1876 by the Admiralty had disappointed his hopes, and caused his friends to accept that he was mistaken. Then, he says, upon returning to London, and examining the Final Report of the Expedition published in 1884, he found that there were more than 100 temperature inversions actually observed on the thermometers, some amounting to many degrees Fahrenheit. To him this meant that the inversions had been "suppressed" in the Preliminary Reports eight years previous. He insinuated that this was evidence of connivance of the scientific staff to rob him of his just recognition, and that the Admiralty owed him—and his country—a definite statement as to which reported temperatures were correct. Moreover he offered his own services as investigator of the matter, should they care to appoint him.

WITH APPROPRIATE bureaucratic delay the Admiralty replied that it was to the Lords of Treasury that he must apply, and in due course the Treasury itself informed him that they could not comply with his request to be appointed because the *Challenger* Staff "is already in the course of reduction."

No guns spoke for several months, but if the Lords Commissioners were under the impression that they had silenced William Leighton Jordan, Esq., they were mistaken, for shortly after receiving a further demand to explain certain wide discrepancies in reported temperature (one of 33½ ° F!), a question was directed to them at Parliament by Mr. Marum. According to *The Times* account of July 24th, 1888, the First Lord G. Hamilton replied: "The reports of the *Challenger* exploration, published at intervals by the Admiralty from 1873 to 1875, had not at that time been dealt with in the light of the investigations subsequently made, especially those on

deep-sea thermometers by Professor Tait. These reports, therefore, can only be looked upon as preliminary. Careful perusal of the official narratives of the *Challenger's* voyage, and also the report by Professor Tait published in connection with the temperatures, will satisfactorily explain the discrepancies between the preliminary and final reports."

Jordan immediately recognized that this reply was not to the point. He was familiar with Tait's report—it had to do with small corrections that should be made to the deep-sea thermometers because they were not completely protected from pressure (only the large bulb was). We know that the *Challenger* staff had trouble with their thermometers, that they could not be read to better than 0.1 °F and had a bad parallax of 0.25 °F. They were the so-called Miller-Casella variety of Six's maximum-minimum thermometer—the extremum temperatures being read from indices sliding on top of the mercury column. In principle these thermometers were incapable of registering temperature inversions, but because of the great thermal inertia of the large bulb, they could be hauled fast enough up through a cold layer without registering it, and all evidence of inversions not erased. Thus what looked like errors to the *Challenger* scientists might be the inversions looked for by Jordan. Jordan was aware of this, and that field experiments with varying types of insulation might have revealed, had they been made, what was accepted as errors to be the truth, but he could elicit no discussion of this from the Admiralty, or even later, as we shall see, from Murray. The curves in the final report are drawn through data points actually observed. The points show many inversions, the curves are all drawn with temperature decreasing. Murray simply stood on the record, and disdained to explain his justification for drawing the curves to Jordan (or to anyone else, so far as we know).

Following the First Lord's reply in Parliament, Jordan was referred to the Edinburgh office and the following correspondence ensued.

❖ ❖ ❖

25 Jermyn Street, London, SW

September 10, 1889

John Murray, Esq., F.R.S.E.

The *Challenger* Office

32 Queen Street, Edinburgh

Dear Sir,—At intervals between fourteen and ten years ago, the Lords of the Admiralty sent me various reports on the exploration then being made on board the *Challenger*.

Report No. 6, dated 1875, gives the following among the statements of temperature obtained by the thermometer:—

In lat. 34°7'S., long. 73°56'W.:

At 1,100 fathoms 35.9°

In lat. 4°19'N., long. 130°15'E.:

At 40 fathoms 74.5°

Nine years after the above, that is to say in 1884, vol. i. pt. iii., "Physics and Chemistry," part of the large work on the *Challenger* Expedition, was published, and in this new record the results of the above-mentioned observations are recorded as follows:—

In lat. 34°7'S., long. 73°56'W.:

At 1,100 fathoms 47.4°

instead of 35.9°

In lat. 4°19'N., long. 130°15'E.:

At 40 fathoms 41° instead of 74.5°

Will you kindly inform me (sic) which of those Reports is correct? You will notice that there is in one instance a difference of more than 11° Fahrenheit between them, and in the other more than 33°.

I address you in consequence of having been referred to the Edinburgh Office by the First Lord of the Admiralty, and I notice from one of the above-mentioned volumes that you appear to be in charge of that office.

I am, Sir,

Your obedient Servant,

WM. LEIGHTON JORDAN

❖ ❖ ❖
Challenger Expedition Office
32 Queen Street, Edinburgh
September 11, 1889

Dear Sir,—The temperature shown by the thermometer when hauled up from 1,100 fathoms in lat. $34^{\circ}7'S.$, long. $73^{\circ}56'W.$, was $47.4^{\circ}F.$

The temperature shown by the thermometer from 40 fathoms in lat. $4^{\circ}19'N.$, long. $130^{\circ}15'E.$, was $41^{\circ}F.$

It is evident that in these cases the thermometers had not worked properly. The temperatures you give from Report 6 are not the observed temperatures, but temperatures from the curve, as is fully explained in pt. iii of vol. i, "Physics and Chemistry" of the *Challenger* Reports.

Yours sincerely,
JOHN MURRAY

❖ ❖ ❖

London: September 13, 1889

John Murray, Esq., F.R.S.E.
Edinburgh

Dear Sir,—Your explicit and friendly reply to the inquiries I made on the 10th instant encourage me to ask you further to inform me what is the evidence that in the two cases about which I inquired the thermometer had not worked properly; and also as to whether in all the 163 cases in which there are important differences between the first and second Reports, of the same character as the two cases above alluded to, the observed temperatures were discarded for the same reason.

I am acquainted with the explanation, to which you refer, of the manner in which the temperatures have been obtained from curves, but can you explain why such exceptions were made in the first records as to give the temperatures of 33.7° at 500 fathoms and 41.4° at 1,100 fathoms in lat. $45^{\circ}57'S.$, long. $34^{\circ}39'E.$? Do you suppose that the thermometer was correct in those readings?

Yours sincerely,
WM. LEIGHTON JORDAN

❖ ❖ ❖
Edinburgh: September 17, 1889

Dear Sir,—The indices of some thermometers work much better in some instruments than in others, and if the index of a thermometer shifts easily there is always a suspicion if such a thermometer records a temperature widely different from the temperatures usually recorded in a certain depth or locality. I refer you to the Introductory note to "Deep Sea Temperatures" in vol. ii, "Narrative," for further information as to errors, curves, etc.

In lat. $45^{\circ}57'S.$, long. $34^{\circ}39'E.$, to which you refer, there is a note to say that "Probably owing to the rolling of the ship, a number of the thermometers were shaken."

The observed readings have never been discarded: they are published in the "Report on Deep Sea Temperatures" for your information and for all others interested.

It is evident that when observations are plotted on squared paper the curves may be drawn different through the points of observation, and of course the temperatures taken from the curves will be different. To show how we have drawn them, they have all been published in the Report just mentioned above.

Yours sincerely,
JOHN MURRAY

❖ ❖ ❖

London: September 21, 1889

John Murray, Esq.
Edinburgh

Dear Sir,—I have to thank you for your reply of 17th instant to my inquiries of 13th, from which I gather it to be your opinion that wherever a thermometer has recorded a temperature intermediate between the extremes above it cannot have worked properly. I shall be much obliged if you will kindly confirm or contradict this interpretation of your letter.

I also understand that in such records as that in lat. $32^{\circ}28'N.$, long. $154^{\circ}33'W.$, where the temperature is recorded as 35° at

1,300 fathoms, and also 35° at the bottom, 2,850 fathoms, the only positive evidence given is that from 1,300 fathoms to the bottom there is no water colder than 35°, but that it may be warmer to any extent not exceeding the surface temperature of 74°. I hope you will kindly confirm this also if correct, as I wish to be clear as to what may or may not be deduced from the records.

I must say that in the first record sent to me there was no such note as that you give in reference to lat. 45°57'S., long. 34°39'E. The temperatures appeared to be given as if there was no doubt as to their correctness; and when I used the word "discarded" I alluded to the observed readings having been discarded as regards publication for eight or nine years, whilst temperatures obtained from *curves* were published instead of those obtained direct from the thermometer, which gave a very different bearing to the records.

Yours sincerely,
WM. LEIGHTON JORDAN



Fife: September 26, 1889

Dear Sir,—Your letter of the 21st instant has been sent on to me here, where I have no books or papers on temperatures.

I must say I do not understand your letter, unless it be on the assumption that you do not know the kind of thermometers used on board the *Challenger*. If you will look at the narrative of the cruise you will find all the necessary explanations as well as my opinions on the temperatures.

The preliminary reports of the captain of the ship were prepared at sea, and never professed to give all details; these you will find in the official Reports, and I will always be glad if I can give you any information or further details about the observations recorded in these official Reports, but you must refer to date and page of the Reports, for my time is very much occupied.

Yours sincerely,
JOHN MURRAY



London: September 28, 1889

John Murray, Esq.,
Edinburgh

Dear Sir,—On receiving your letter of 26th I looked back at mine of 21st, and it seems to me that the two questions asked in that letter, as also the question at the end of my letter of 13th, were sufficiently clear and explicit to have enabled you to answer without more trouble than that of putting pen to paper: that, indeed, you might have answered by a shorter and less troublesome letter than the one you have written.

I well know the kind of thermometer used on board the *Challenger*, and was much disappointed when it was decided that the vessel was to go to sea with such thermometers only, as such decision showed a determination not to admit the possibility of such a distribution of temperature as the late Mr. A.G. Findlay declared to be required by the sections which I submitted to the Council of the Royal Geographical Society in 1866. I know what was then supposed to be the working of these thermometers, and that in the *Challenger* narrative you continued to be of the same opinion; but I wished to ascertain whether you had been led to change, or in any manner to doubt, the correctness of that opinion. Viewed by the light of the full details given in "Physics and Chemistry," vol. i, it certainly looks as if there had been, even during the voyage, some changes of opinion on that point.

For instance:

It appears from Plate XVIII, "Physics and Chemistry," 1884, vol. i part iii, that on May 2, 1873, in lat. 37°25'N., long.

71°40'W., the thermometer recorded:

35¼° Fahr. at 550 fathoms and

45° Fahr. at 800 fathoms;

but in the original Report, 1876, No. 7, page 17, the latter temperature is discarded, and instead of it the temperature is given as 37.6° as if obtained direct from the

thermometer, and is made to show in that report a constant fall of temperature, as that of $35\frac{1}{4}^{\circ}$ at 550 fathoms is not recorded. As both records broke the curve which it was supposed the thermometer must record if working properly, they were discarded as erroneous. But, later on, on December 24, 1873, as appears from Plate XCIII of the above 1884 volume, in lat. $45^{\circ}57'S.$, long. $34^{\circ}39'E.$, the thermometer recorded:

34°Fahr. at 500 fathoms, and

42°Fahr. at 1,100 fathoms;

and these are given in the original Report, 1874, No. 2, page 14, as

33.7° at 500 fathoms, and

41.4° at 1,100 fathoms;

though no reason appears for discarding the rise of temperature at 800 fathoms near the Gulf Stream if that at 1,100 fathoms near the Agulhas Current was to be retained.

When I received the latter record I supposed that some arrangement had been made to enable the thermometer to record correctly the temperature of its resting-place without being affected by the strata above.

It certainly appears from the above and other cases that there were either some changes of opinion on board the *Challenger* as regards the working of the Miller-Casella thermometer or carelessness as to misleading students of the subject who had not the advantage of being on board.

I now therefore venture to ask again if you still consider that in none of the 163 serial soundings in which the thermometer recorded a temperature intermediate between two super-incumbent temperatures it can have worked correctly? To give weight to this question I will point out (as is probably well known to you) that in experiments made by Commander Beardslee, U.S.N., with the Miller-Casella thermometer in an ice-bath of the temperature of $32\frac{1}{2}^{\circ}F.$, in which a standard thermometer fell from a temperature of 70 degrees to that of $32\frac{1}{2}^{\circ}$ in one case in 20

seconds, and in another case in 40 seconds, the Miller-Casella took in the latter case 25 minutes to fall only 35.1° , and in the former 14 minutes to fall to $35^{\circ}F.$ These records I take from page 114 of *Deep Sea Soundings*, by Charles D. Sigsbee, USN, Washington, 1880.

This seems to show that a Miller-Casella thermometer might *pass through* a cold stratum of water without recording it, whilst another higher up on the sounding-line, chancing to *rest* in the cold stratum, would record it.

Yours sincerely,

WM. LEIGHTON JORDAN

❖ ❖ ❖

Edinburgh: October 2, 1889

Dear Sir,—I refer you to pages 85 and 420 of the Narrative of the Cruise for an answer to your question as to the temperatures recorded by the Six's thermometers.

I don't think one of these thermometers could be drawn through a cold stratum of water without being affected by it.

Whether or not it would record the correct temperature of that stratum would depend on the time the thermometer was in the stratum.

I enclose you a paper which gives examples, I think, of alternate layers of water of different temperatures which could not have been recorded by the Six's thermometers.

Yours sincerely,

JOHN MURRAY

❖ ❖ ❖

London: October 7, 1889

John Murray, Esq.,

Edinburgh

Dear Sir,—I have to thank you for your note of 2nd instant and reference to pages 85 and 420 of the *Challenger* Narrative, with both of which I was, however, already acquainted. They express the same view of the action of the Miller-Casella thermometer as given by Sir Wyville Thomson in *The Depths of the Sea*.

I never for a moment supposed that those thermometers could be drawn through a cold stratum of water without being affected by it; and your present statement is the first made by anyone connected with the *Challenger Expedition* (as far as my knowledge goes) to the effect that a Miller-Casella thermometer might, whilst working quite correctly, pass through a cold stratum of water without recording it, though correctly recorded by another Miller-Casella thermometer higher up on the sounding-line and chancing to rest in that cold stratum. The action of the cold stratum on the thermometer passing through it to the lower stratum would of course prevent it from recording the full amount of the rise of temperature in that lower stratum. The evidence would be an increase of temperature not less than the difference between the minimum indexes of the two thermometers, nor greater than the temperature of the surface stratum unless recorded by the maximum index of the lower.

This action of the Miller-Casella thermometer gives increased importance to evidence which has been ignored throughout all the diagrams published to illustrate the distribution of temperature shown by the *Challenger* observations.

As an instance of the manner in which those diagrams have suppressed evidence given by the *Challenger* thermometer, I ask you kindly to refer to "Physics and Chemistry," vol. i, Plates CLXIII to CLXXII. Those ten Plates show that in ten consecutive serial soundings taken in 1875 from June 28 to July 14, and extending from

Lat. 35°22'N., long. 169°53'E., to
 " 38°9' " " 156°25'W.,

where the depth varies from a minimum of 2,550 fathoms to a maximum of 3,125 fathoms, the thermometers show in every one of those soundings a higher temperature at the bottom than at a depth between 1,300 and 1,500 fathoms.

The vertical distribution of temperature shown by the *Challenger* thermometer

in that part of the ocean is professed to be given in the Narrative, diagram 18, p. 756. But that diagram completely ignores the fact that each one of the serial soundings on which it is based shows a higher temperature at the bottom of the ocean than at a depth about halfway between the surface and the bottom. How in the face of this is the publication of that diagram to be justified? It in fact suppresses the most important evidence by far given by the *Challenger* thermometers in that region. And, according to what we have now agreed to be the action of those thermometers, the rise of temperatures thus shown indicates a still greater rise.

I am much obliged for your pamphlet, and intend, with your kind permission, to reproduce one or two of the diagrams in a new edition of my work on "The Ocean" which I have had for some years in preparation.

Yours sincerely,
 WM. LEIGHTON JORDAN

❖ ❖ ❖

London, October 29, 1889

John Murray, Esq.,
 Edinburgh

Dear Sir,—I notice that in my letter of 7th instant I said, when alluding to the ten consecutive serial soundings, that the rise of temperature was "at the bottom" instead of at deeper depths than 1,200 to 1,500 fathoms. The phrase "at the bottom" holds good literally only for the six consecutive soundings, Nos. 248 to 253, on which diagram No. 18 is constructed, and not for No. 247, which is repeated from diagram No. 17.

To state the case briefly: Through seventeen consecutive soundings extending east and west for 3,000 miles the thermometer showed a break in the fall of temperature which commences at a depth of about 800 fathoms in the warm Japan current, and falls gradually in a waving line until the rise of temperature is shown quite at the bottom of the ocean.

It seems to me quite incomprehensible why such a break in the fall of temperature, corroborated by seventeen consecutive soundings, has been ignored as it has been. This alternation of temperature starts from near the surface where the hot waters of the Japan current interlace on the surface with the cold Arctic current, and falls in an easterly direction, at first steadily, and then in a waving line, until, 3,000 miles to the east, it is spread along the very bottom of the ocean.

There is also a similar alternation at a lesser depth starting from the same locality, and shown by every sounding for 700 miles.

When my paper on Oceanic Circulation was submitted to the Council of the Royal Geographical Society in 1866, the late Mr. A.G. Findlay insisted that my diagrams required that the alternations of temperature found on the surface in the temperate zones should be carried downwards into the lower strata; and it is a mere matter of fact that the *Challenger* thermometers have superabundantly shown such an alternation extending from the meeting of the warm Japan and cold Arctic currents for 3,000 miles through the lower strata.

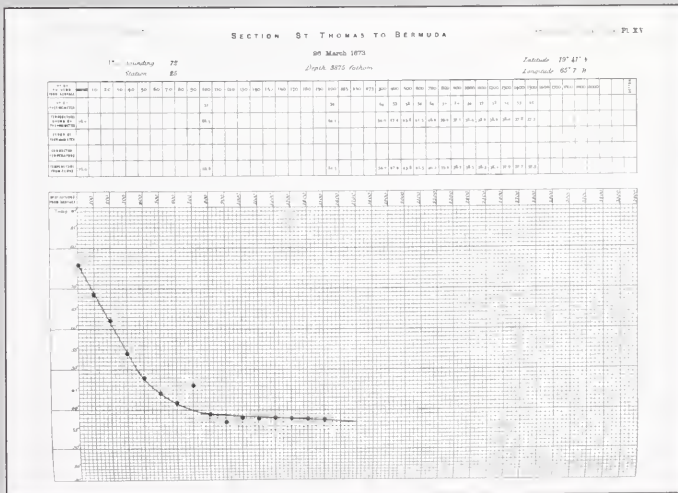
Yours sincerely,
WM. LEIGHTON JORDAN

❖ ❖ ❖

A sample sounding made in the North Atlantic from the *Challenger* 1884 report is shown in the figure below. The temperature curve is not drawn through all the observed points. It is likely that some of the points observed are errors. In reporting some of the furthest south stations in the Indian Ocean no curves or dots are shown, only tabulated values, and these appear to be consistent with what we now know to be true; there is a temperature inversion of several degrees actually there, and the *Challenger* staff not only observed it with thermometers which were not supposed to be able to do so, but they also mistrusted the inversion so much that they decided not to plot points or draw curves. We do agree with Murray, however, that much of the scatter at other stations was probably due to errors, and the Final Report, if not the Preliminary Report, is very clear to anyone without a monumental chip on his shoulders.

There seems to have been no further reply from John Murray. It is a pity he could not avow his predilection for an ocean that always gets colder as one goes down; and it is a pity that Jordan could not simply take the observations themselves as verification of his prediction (no matter

how absurd the way he thought them up). Jordan instead appealed again to the Admiralty for redress in the words: (November 14th) "...If, my Lords, you should think that I have not shown sufficient reason for requesting an official opinion as to whether the statements of 'observed temperatures' or the statements of temperature obtained by the above-mentioned alternations of those observed temperatures most faithfully represent the existing



THE SLING

Letters to the Royal Institution
The Institution of Civil Engineers
and the Admiralty
May 1905 to May 1912

by
WM. LEIGHTON JORDAN

Member of the Royal Institution; Associate of the Institution of
Civil Engineers; Fellow of the Royal Geographical Society, of the
Royal Society of Arts, of the Royal Statistical Society, and of the
Royal Meteorological Society; Past President of the English
Literary Society of Buenos Ayres

Second Edition

London: Simpkin, Marshall,
Hamilton, Kent and Co. Ltd.

distribution of temperature, I shall be quite ready, my Lords, to give you a perfect deluge of further evidence."

The brief note acknowledging receipt of this letter once more stated that the Admiralty would not in any circumstances offer an opinion on the scientific question raised, and that it was not responsible for the *Challenger* publications. Jordan closed the correspondence on July 14th, 1890, with a final blistering protest.

Now, so long after the battle, it is easy to appreciate both sides. Jordan was a crank, he had inexhaustible energy and time, he was a terrible pest. The *Challenger* Office was cautious, reserved, willing only to stand on the record as published, but not to explain it to Jordan for fear of becoming embroiled. And we cannot refrain from recognizing that the temperatures first published, taken from a smooth curve without warning or explanation, on the assumption that all inversions were simply errors, was misleading, to say the least. Poor Jordan.

During the last thirty years of his life Jordan loitered about the Royal Institution, to the dismay of Rayleigh, Crookes, Larmor, Love, and other distinguished scientists who found their paths crossed his.

Jordan published his side of these encounters in a volume entitled *The Sling*—a weapon to which he often refers, and the theory of which he tried to use to confound the experts. Perhaps it also cast him in the role of David.

And then a wonderful thing happened to him in 1916. His veracity was questioned and he was exonerated, and the apology was handsome indeed. It happened this way. The *Journal of the Alpine Club* reprinted an article entitled "The Second Ascent of the Matterhorn by the East Face" by J.M. Elliott in which an allusion was made to a previous attempt by Jordan in 1867, and J.P. Farrar had added a footnote: "It is probably that Mr. Elliott was misinformed." But Farrar soon discovered that Jordan's ascent was a fact and located and apologized to the old man. The interview was a great success, the young mountaineer much impressed, and he persuaded Jordan to write up the account of his experiences on the Matterhorn. Moreover, Jordan's 1867 report in the visitor's book at Breuil was found and published as well. Farrar was deeply impressed by the octogenarian Jordan. It even emerged that Jordan had risked his own life to rescue one of the guides who was about to be abandoned because judged to be in a hopeless position. Jordan was the first to traverse the Matterhorn from Italy to Switzerland, the second to climb the Matterhorn following Whymper's tragic accident; he left a rope which is still called the Echelle Jordan there—Farrar called him "a splendid veteran of the mountain." Jordan must have basked in the adulation of youth generously bestowed.

We find Jordan in retrospect to have been a sympathetic character. Today we have some inkling of what a long road there is yet to travel from Newton's laws of motion to a full elucidation of how these laws work in the actual ocean. The notions which Jordan on one hand, and Carpenter

and Croll on the other, put forward all seem to fall so short of the mark that the difference in starting point seems insignificant. Only the caution imbibed with proper academic training restrained Carpenter and Croll from trying to climb as high as Jordan did. One does not see that their deference to the laws of motion, nor their inability to apply them, led them to a better overall view. Like the rest of us, Jordan did not reach that summit from which someday we will be able to see the mechanism of the oceanic circulation laid out in detail before us. He chose the wrong route, and suffered the indignity of being judged a fool, but in his dogged way he did traverse the mountain. ❖

Publications of Wm. Leighton Jordan

1866 *The elements: an investigation of the forces which determine the position and movements of the ocean and atmosphere.* 2 vols., maps, charts, and plates. R.G.S.

1868 *A treatise on the action of Vis Inertiae in the ocean; with remarks on the abstract nature of the forces of Vis Inertiae and gravitation and a new theory of tides.* 212 pp., maps, and charts. London: Longmans, Green & Co. R.G.S.

–University of London Library. (copy held)

–Reviewed in *Nature*, London, 3, 505–506, 1871.

1871 *Remarks on recent oceanic exploration by the British Government and the supposed discovery of the law of oceanic circulation by Dr. W. B. Carpenter, F.R.S.* Buenos Ayres: Imprenta Inglesa, 56 pp. R.G.S.

1873 *The oceans, its tides and currents and their causes.* London: Longmans, Green & Co. R.G.S.

–2nd edition. Royal Society, Admiralty Library, 1885.

1877 *A lecture on the winds, ocean currents and tides and what they tell of the system of the world.* 2nd edition, map. R.G.S.

–Royal Institution; Lecture delivered at Willis's rooms "A public challenge to the Council of the Royal Society and all scientific staff of the *Challenger* Expedition."

1877 *The winds and their story of the world.* R.G.S. –3rd edition. 1885.

1877 *Remarks on the recent oceanic exploration and the current creating action of Vis Inertiae in the ocean.* Maps and plates. R.G.S.

1881 *Skirmishes in the vanguard of science, August 1880 to February 1881.* Buenos Aires. R.G.S.

1882 *The standard of values.* 150 pp. London. R.G.S. –5th edition. University of London Library, 1888.

1883 *The new principles of natural philosophy.* Royal Institution 2nd edition. 570 pp., plate. London: Longmans, Green & Co. R.G.S. A defence and extension of the principles established by the author's treatise on ocean currents.

1888 *The Challenger explorations.* 15 pp. London: Spottiswoode & Co. R.G.S.

1890 *The Admiralty falsification of the Challenger record.* 35 pp., 1 fig. London: Spottiswoode & Co. R.G.S.

1895 *The cardioid earth.* R.G.S.

1900 *Essays in illustration of the action of astral gravitation in natural phenomena.* pp. XIV + 192, diagram. London. R.G.S.

–University of London Library, Royal Institution Library. (Much on ocean currents, information that manuscript charts on "ocean cyclones" in Map Room of R.G.S.)

–Reviewed *Nature* June 13, 1901, No. 1650, Vol. 64, p. 155. (copy held)

1904 *Astronomical and historical chronology.* pp. 70.

London: Longmans, Green & Co. Royal Institution. (Aim reformation of historical chronology to bring it into accord with the method of numbering the years B.C. adopted by astronomers.

Astronomers 1 A.D. preceded by Ø.

Historical 1 A.D. preceded by 1 B.C.

Dedicated to "all librarians of the cities of Florence and Pisa.")

–Reviewed *Nature* July 14, 1904, No. 1811, Vol. 70, p. 243 by W.T.L. (copy held)

1907 *The Sling. Letters to the Royal Institution, the Institution of Civil Engineers and the Admiralty, May 1905 to May 1912.* 2nd edition, 1913, 431 pp. London: Simpkin, Marshall, Hamilton Kent & Co., Ltd. Royal Institution. (Also 1st edition 1907, letters 1905 to 1907.) [Contains also "Squaring of the Circle," 1918 submitted to *Nature* and inserted in all his books at the Royal Institution.] [*The Sling* refers to ocean cyclones, polar exploration, Bering Sea, building of the *Dreadnought*, and *Challenger* records. 1907 Japanese deep-sea thermometers.] See also comment in *Nature* April 2nd, 1914, No. 2318, Vol. 93, p. 121, referring to Belot, *Comptes Rendus*, Vol. CLVIII, p. 647. W.L.J. points out that "the motion of the earth through space tends to create high land in the Antarctic and deep water in the Arctic region;" "cardioid" a term he used in 1866 and in *The Sling*.

Carl Wunsch

Massachusetts Institute of Technology



Discussions of Hank's career have tended to downplay his role as a "professor," but that's what he was from 1960 to 1963 at Harvard, and then from 1963 to 1978 at MIT. I first met him the fall of 1963 when he came to MIT from the Harvard faculty. At the time, I was a Ph.D. student in geophysics, and Raymond Hide, who was then at MIT, suggested I should go see this new professor. Hide described him in terms of the highest praise, and only obliquely alluded to the oddity of someone's moving from Harvard to MIT, a change that in those days was almost unheard of (the pay at Harvard at the full professor level was much better). When I got to know Hank well later on, I heard several variations of his reasons for this move, which I will pass over here.

Hank deprecated his professorial years—I suspect for several reasons. He was not very comfortable with the inevitable formality arising from the gap between a senior professor and a very young, ignorant student; he remained uneasy about lacking an advanced degree himself while trying to advise others about how to get one; and he generally preferred the one-on-one approach to doing science—standing at the blackboard with a colleague who could hold his own (later he referred to the Harvard/MIT years as his 20 years of "exile").

Following Ray Hide's suggestion, I went to see this man of whom I had never heard (I was studying things like seismology and geomagnetism), and the meeting changed my life. I have never, before or since, encountered anyone like him. The then Department of Meteorology was jammed into one of the buildings left over from the World War II Radiation Laboratory. Hank had to share an office (something now unheard of for any faculty member, even in the most junior ranks), with a radar meteorologist whom Hank did not greatly admire. Their desks faced each other, with the telephone swinging on a pivot between them. In the midst of the resulting chaos, he treated me as a colleague, almost from day one—grabbing my arm to lead me to the department library to look up instantly something he was all excited about. Among the things I remember him being greatly excited about were the thermocline theory, Fredkin's computerized vortices, the long-period tides, double diffusion....

For many students, having Hank as an adviser must have been a considerable trial. If one went to him with a problem ("I don't see how to solve this differential equation"), he would listen patiently for as much as five minutes, before breaking in with something like "Yes, that looks hard. But let me show you what I've been doing." With that, he would launch into an extended discourse about something he had gotten obsessed with. The student who needed purely technical help was stuck, and often had to seek assistance from the several other eminent, but more conventionally disposed, faculty members; but the student who was really interested in physical oceanography was exposed to the most intense, enthusiastic encounter with the moving frontier of the subject that one could imagine.

But the student who was really interested was exposed to the most intense, enthusiastic encounter with the moving frontier of the subject.

Hank's lectures were never models of preparation and polish. He usually talked about whatever he was working on at the time—even in those classes advertised for those (like myself) who knew nothing about the ocean. Often he would start a sentence, get to the middle somewhere, hesitate, and come to a full stop. After a short break, a new sentence would start, typically in the middle, as the result of a thought that had occurred as he had been talking, probably infuriating most of his listeners. He was a lousy lecturer, but the most inspiring teacher, and later colleague, I have ever encountered. I think he never himself appreciated the effect he had on people. ❖

Myrl Hendershott

Scripps Institution of Oceanography



At some point when I was Hank's student at Harvard I asked him how he had visualized the difference between the mid-ocean flow and the western-boundary-layer flow in his famous 1948 paper on westward intensification of wind-driven ocean currents, since, in that paper, he hadn't used the boundary-layer theory that we students were learning to use to think about similar problems. The ingenuity of his answer has been striking to everyone I've ever told it to, scientist or not.

Imagine a pair of rollers side by side. Paper rolls off of one and onto the other, and the moving paper lies horizontally flat in between the rollers. Now suppose there is a rain of fine sand onto the moving, flat paper. The thickness of the sand layer is zero where the paper first leaves the offrolling roller, it gets bigger towards the onrolling roller—until very near the onrolling roller sand suddenly begins to fall off the paper onto the floor. The gradual rise of sand thickness from the offrolling to the onrolling roller corresponds to the feature of the theory representing gentle, mid-ocean currents feeding the Gulf Stream. The sudden decrease of sand thickness as sand near the onrolling roller begins to fall off the end of the paper and onto the floor corresponds to the rapid and narrow Gulf Stream.

Later on, when I was recalcitrant about getting my thesis into the literature, Hank printed up a notice on pink paper that said "From the Central Committee." Beneath it he wrote, "We think you should publish your thesis."

Not too long after I came to Scripps Institution of Oceanography there was a series of cruises in the Indian Ocean. I went along on one. Hank and Warren Wooster were co-chief scientists. There were a lot of stations to do, and the cruise began with a carefully thought-out station plan. But Hank and Bruce Warren often modified it during their watch to look into details they found. When the next watch came on, we'd have to scramble hard to catch up to the station plan. The weather was sometimes rough, so much so that the ship's physician feared for the life of one of the other students (who went on to a distinguished career in solar physics). With Hank, we conceived the idea that a deck chair could be rigged to hang from the I-beams of the lab ceiling so that its occupant would be shielded from the worst of the rolling. It worked pretty well,

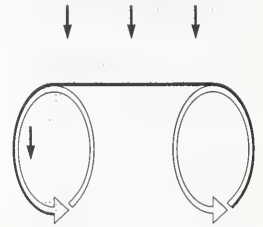


Diagram represents a paper-roller-and-sand concept Henry Stommel used to describe how gentle, mid-ocean currents feed the Gulf Stream.

When I began to work with graduate students, I more or less unthinkingly treated them the way Hank had treated me.

but the walls of the lab now came rushing at the person seated in the chair at a frightening rate. That was the summer that US ships were supposed to have been attacked in the Gulf of Tonkin, and the shortwave radio was filled with news of possible US retaliation. "Just think, Myrl," said Hank, "you may end up a prisoner of war in China." I recalled those days vividly when I was at sea in the Strait of Gibraltar as US planes flew overhead toward Tripoli, and again when I was at sea in the Gulf of California during the Iraqi invasion of Kuwait.

Some years later, I spent part of a summer at Woods Hole. I didn't feel I was making very good progress with my studies of tides. Hank offered what encouragement the work permitted him to give, talking about his own early interest in similar calculations. He also added that it was important to keep the physical problems foremost. Of course I knew this at some level, but the technical problems were the ones I thought I could do something about. So I had stayed with them, rather afraid to try to answer the simple physical questions I thought were important but didn't feel capable of answering. It took me a long time to work up the nerve to take his advice, but it was when I did temporarily abandon the technical concerns that I found the most significant results of the investigation.

When Carl Wunsch and Bruce Warren put together the 1981 book in his honor, Hank sent each of the contributors a letter. He included in mine some reprints on tides autographed by some of the greats in that field. On top of those reprints in the envelope was his biographical paper about William Leighton Jordan, Esq. When I opened the package—which I guessed was from him and was about the book—that name was the first to strike my eyes and I thought for a short, frightening moment that something I had written in my chapter had qualified me for the award (see page 75 for the award citation).

My association with Hank was *the* great experience for me when I was trying, as a graduate student, to think for myself about science. But of far greater importance to me has been the fact that, of the many people I've known, Hank was one of a handful about whom I consistently felt from first acquaintance "I want to be that kind of person." I can't say I succeeded, but I am ever more appreciative of his example and his friendship as I get older. The most visible result may have been that, when I began to work with graduate students, I more or less unthinkingly treated them the way Hank had treated me; the results have been my very best professional contributions. ❖



Warren Wooster took this photo of Hank Stommel "on the boat between Anzio and Ponza" in 1969.

David Halpern

Jet Propulsion Laboratory



Hank Stommel's lessons always extended far beyond the science of oceanography, making me feel fortunate to have been his student, for many reasons. In September 1964, Hank started my training at MIT by having me mark ocean temperatures on a large sheet of paper. Instead of assigning me the tedious plotting duties, Hank connected the values himself because, as he told me, drawing in the isolines encourages a variety of interpretations of the numbers on the paper. Even in our current era of voluminous data sets analyzed by supercomputers, I keep in mind Hank's emphasis on the human element, and the value of intuition.

In my second semester, Hank suggested that I look for internal waves in Massachusetts Bay as a research project. Although I had never been on a boat larger than a canoe, Hank told me to gather up equipment at WHOI, buy appropriate instrumentation, and charter a boat—and that he would back me with his research funds. Hank expressed confidence in a concrete way and motivated me to reach beyond what I thought I could do, in the same way that he encouraged so many others.

I learned from Hank that service to the scientific community is important, although he once quipped that bringing oceanographers together toward a common goal was akin to conducting an orchestra of elephants. In the middle of my training, Hank taught me the most meaningful lesson. While describing his view of the future of oceanography, he told me that we were fortunate to live in a time when someone would pay us to do our hobby. Hank made me realize how lucky we are to be able to spend our time doing something we love. ❖

Bob Munns

Woods Hole Oceanographic Institution (retired)



Henry Stommel was a wonderful shipmate, by which I mean full of wonder. It was on longish voyages that I was most associated with him. The work might be dull for those not directly involved with the stations and the data, but life was not. It is on such trips that nerves might wear thin from boredom or irritating personalities, but not with Henry aboard.

I was always amazed at the breadth of his interests. Astronomy, navigation, merchant shipping, shortwave radio, mathematics, geography, exploring seaports, weird chemicals, gardening, and using a printing press are among those that I recall. Whatever he took up, it was with great enthusiasm. He had the essence of life, in that everything good should be FUN to do.

An oceanographer should be international, and Henry was. That he worked closely with British, French, German, Russian, Spanish, Japanese, and Israeli oceanographers I have seen, although he was not a linguist.

*Even in our
current era
of super-
computers,
I keep in mind
Hank's
emphasis on
the human
element, and
the value of
intuition.*

Henry was most kind to those less fortunate than himself, as I have witnessed. He gave much credit to others who needed it.

In addition to the pleasure of sailing with him, I much enjoyed having dinners with Elizabeth and himself at their home and several others in Falmouth.

I do hope there is a sailor's "Fiddler's Green" where I may see him often. If there is not a special branch for oceanographers, I hereby nominate HENRY M. STOMMEL as an honorary member of that deep-sea sailor's society. ❖

James Luyten

Woods Hole Oceanographic Institution



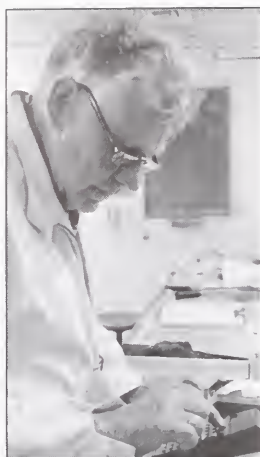
*The Pencil Is Mightier Than the Computer**: After spending a year at the National Center for Atmospheric Research in Colorado, where I became interested in personal computers, I tried to get Hank interested. It seemed hopeless—he said that it was too much trouble to learn something new; his old Texas Instruments calculator was just fine for his level of computing. He would show mild interest in the color graphics, but he felt intimidated by having to learn a computer language. I finally hit upon a solution. I was going to Italy for a week, and I told him that I was concerned about leaving the computer in my house while I was gone, suspecting that Hank would offer to babysit for the computer. I played up the risk of theft at my house, and he bit. I took it over to Hank's house, and offered to set it up, as long as it was going to be there. Hank agreed but relegated it to a rickety table in the corner of his study.

When I returned a week later, Hank couldn't wait to show me what he had been doing—he had learned BASIC and had found that he could solve nonlinear equations. He was enthralled because he could proceed on his own where he had always needed to get mathematical help before. I think that it also had the immediate feedback of working in real time that Hank needed. Soon he had a simple LPS (ventilated thermocline) model of the

thermocline, a planetary system, and a fireworks display. Hank also loved the graphics in spite of the primitive resolution at that time.

It took two weeks to get my computer back from Hank, and then only after the one he had ordered had arrived. He was thoroughly hooked, because the personal computer combined an interactive way to solve equations with a visual display. It gave him a way to visualize the solution and unravel the problem to get to its essence. Hank enjoyed using a computer greatly, writing more than 1,600 programs between 1985 and 1992, but it was just a tool to get where he wanted to go—to understand problems clearly enough to explain them without any mathematics. ❖

*from Henry Stommel's talk "The Goldsbrough Variations" upon accepting the Crafoord Prize; *Tellus*, 111-119, 1984.



Hank uses his trusty calculator in the top photo and plays with a computer in the lower photo.

Close Encounter

by Starbuck

WHEN HALLEY'S COMET visits the sun again this coming spring, it will herald the 300th anniversary of Newton's presentation of *Principia* to the Royal Society in April 1686. All during the summer of 1685 Newton labored upon the final draft, and the world of science was never the same again.

Halley became the Astronomer Royal and began to apply the theory of universal gravitation to comets. Assuming that the orbits of comets are parabolas (a geometrically simpler form than extremely elongated ellipses), Halley calculated the elements of some 24 comets observed back to 1337. He noticed that the elements of the comets of 1531, 1607, and 1682 were much alike and speculated that they might all be appearances of the same celestial body. He predicted that it would appear again in 1758-1759.

The reason for the indefiniteness of his predicted date of reappearance was that there was a 15-month discrepancy between the periods of historical appearances. The interval between 1531 and 1607 was 76 years and 62 days; that between 1607 and 1682 was 74 years and 323 days. He attributed this difference to the derangement of the comet's orbit by its encounter with Jupiter. However, the means of calculation of this derangement did not exist in Halley's time. He died in 1742, leaving it to the French mathematician Clairaut to attempt a more precise prediction. Clairaut managed to present it to the French Academy just one month before Halley's comet was seen again. His error was 22 days.

The advent of computers has made calculations of the motions of comets much easier than they were in Halley's time. It might make a good science project for some Cape Cod high school student to demonstrate with a program on the screen of his personal computer just how a massive planet might be expected to affect the orbit of a highly eccentric comet during a close encounter. As a starter, one could begin with both orbits in the same plane.

So I'm offering a prize of \$250 for the most interesting entry by a high school student, or younger, to be judged by two of my scientific friends. We will be looking for some physical understanding of the phenomenon and the discovery of some good questions. An actual prediction of Halley's Comet itself would be more intricate and is not expected.

Entries on a floppy disk, with an explanatory essay, should be at *The Enterprise* office by Thanksgiving. Entrants should put their names on both disk and essay. ❖

The Enterprise
August 2, 1985

William Young

Scripps Institution of Oceanography



Stommel's Thermohaline Regulator

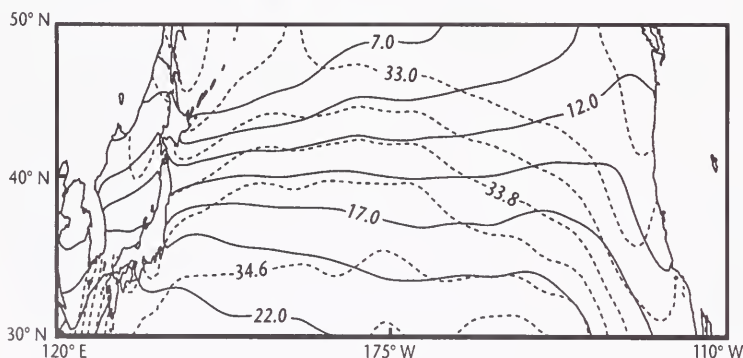
The annually averaged surface temperature and salinity in the North Pacific Ocean, drawn from data compiled by Levitus. Notice that if one starts on the 17°C isotherm and cruises northward, both the temperature and salinity decrease. This means that these two properties make antagonistic contributions to the density of the ocean. But the temperature wins, so that the density increases as one goes northward.

When personal computers became widely available, Hank Stommel was one of the first oceanographers to become an enthusiast. His adoption of this new toy was so complete that I feared his scientific career had been derailed. He displayed some of the classic symptoms of a graduate student hacker: large programs written in rambling, uncommented BASIC, a happy disregard for questions of numerical accuracy and stability, and an inability to stay focused on one problem for very long. This condition abated after a few months and he returned to science. But the personal computer made an enduring change in the way Hank attacked problems. One example of this is his last work on the mixed layer "thermohaline regulator."

During the summer of 1991, after an interval of over 30 years, Hank returned to a simple box model of the thermal and saline structure of the ocean. His original publication in 1961 used this model to describe the grand thermohaline circulation of the oceans. But in 1991 his interest was engaged by observations of the temperature and salinity in the ocean mixed layer. Hank suggested that some of the same processes that determine the temperature and salinity fields in the thermohaline circulation operate on the much smaller space-and-time scales of the mixed layer. He used his 1961 box model to illustrate this possibility.

The other difference was that in 1991 Hank had a personal computer and he solved this box model numerically.

Some of the observations of mixed-layer temperature and salinity that caught Hank's attention are shown in the figure at left. Notice how temperature and salinity make antagonistic contributions to the density. If one starts on the 17°C isotherm and heads north, both the temperature and the



salinity decrease. The fall in temperature makes the fluid denser, but this increase is partially offset by the decrease in salinity. Hank examined this more quantitatively and noticed that in all ocean basins roughly half of the density increase due to temperature in the range 7° to 17° C is cancelled by the fall in salinity.

Of course this partial compensation between temperature and salinity might be because all oceans are forced by the same atmosphere. In other words, the temperature-salinity relation in the mixed layer is a reflection of the patterns of heating, cooling, evaporation, and precipitation imposed by the atmosphere on the ocean. Hank did not dismiss this "imprinting hypothesis," but he did have a strong emotional bias against it. He remarked to me that it was demeaning to consider that

meteorology was solely responsible for determining something as important as the temperature-salinity relation in the ocean mixed layer.

The box model shown below right is Hank's alternative "regulator hypothesis," in which internal mixed-layer processes set the temperature-salinity relation. Box 1 is a control volume in low latitudes and its temperature is set at $T_1 = 1$, while Box 2 is a control volume in high latitudes with $T_2 = 0$. The respective salinities are S_1 and S_2 and y is identical with $S_1 - S_2$. The density difference between the two boxes is proportional to $(T_1 - T_2) - (S_1 - S_2) = 1 - y$. Conservation of salinity is

$$\dot{S}_1 = \frac{1}{2} E(1-y)(S_2 - S_1) + \frac{1}{2} p(t) \quad \text{and} \quad \dot{S}_2 = \frac{1}{2} E(1-y)(S_1 - S_2) - \frac{1}{2} p(t) \quad (1)$$

where the exchange of mass between the two boxes is modeled by the "exchange function," $E(1-y)$. The exchange function depends only on the density difference, $1-y$, between the two boxes. The imprinting hypothesis corresponds to a constant exchange such as $E = 1$. The regulator hypothesis corresponds to an exchange that becomes more vigorous as the density difference increases. An example of this second possibility is $E(1-y) = |1-y|$. In this case the exchange gets bigger when the density difference gets bigger.

Precipitation and evaporation are included with the forcing function $p(t)$. This term is the random effect of rain showers taking fresh water from one box and dropping it into the other. This process is uncorrelated with the salinity in the boxes, and in Hank's numerical calculations he created $p(t)$ with a random-number generator.

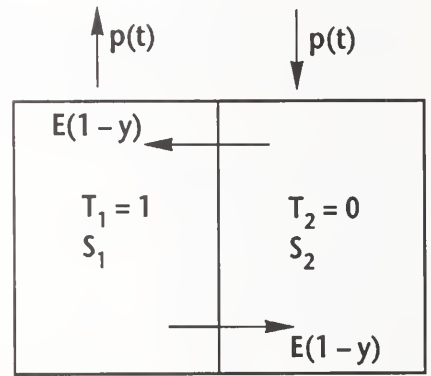
Subtracting the two equations in (1), we have a single equation for the salinity difference between the boxes:

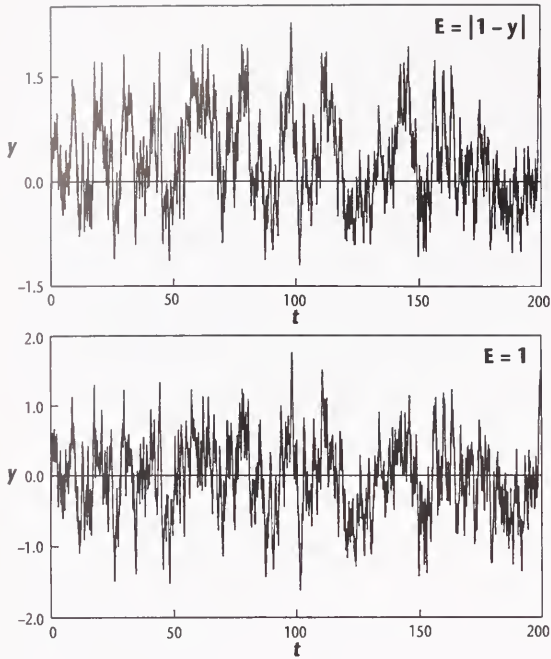
$$\dot{y} = -E(1-y)y + p(t). \quad (2)$$

Thus the state of the system is defined by $y(t) = S_1 - S_2$. This is the equation Hank solved on his personal computer and, as always, he used the very simplest time-stepping scheme: $y(t + \delta t) = y(t) + \delta t[-E(t)y(t) + p(t)]$ where $p(t)$ is a random number and δt is the time step. This method, sometimes called forward Euler, is usually discussed in numerical analysis texts as a precursor to more refined and accurate approaches. But, as far as I know, Hank always used this primitive technique. I am not sure why. His training in astronomy had included orbital calculations using the methods devised by Adams. In this problem it probably makes little difference since high accuracy is not required and computer time is not a constraint.

In the figure overleaf, I show typical results obtained by integrating equation (2) with forward Euler. This figure compares two integrations, one with $E = 1$ and the other with $E = |1-y|$. The difference Hank noticed is that in the second case the average salinity difference is not 0, even though the forcing, $p(t)$, is symmetric. By compiling the results of several calculations, Hank found that if $E = |1-y|$ and the random forcing is very strong, then the average value of y is close to $1/2$. This is the thermohaline regulator—the nonlinear exchange function rectifies the random forcing and creates an average salinity difference between the

The two-box model. The precipitation is $p(t)$ and the salinity difference is $y = S_1 - S_2$. The mass exchange, \bar{E} , is a function of the density difference.





A comparison of two time-stepping solutions showing the salinity difference y as a function of time, t . Both of these jagged curves are produced by randomly forcing the box model in equation (2). The upper panel is the thermohaline regulator. Note how the average salinity difference is nonzero (the curve spends more time above the $y = 0$ axis than below). The lower panel shows the imprinting case in which the average salinity difference is zero (the curve spends equal time on each side of the $y = 0$ axis).

boxes. In fact half of the density difference due to temperature is cancelled by the average salinity difference.

Of course, Hank wasn't content to just observe and publish the results of a calculation. He found an explanation of the computed average salinity difference and also started trying to interest his colleagues in this problem. I think that this was a strong characteristic of Hank's scientific personality—when he didn't understand something, he pounded the pavements looking for help from his colleagues. And it usually wasn't too hard to find someone who was interested in what he was doing. He told me about the thermohaline regulator on the porch at Walsh Cottage, and I suggested a statistical approach that enables one to compute various properties of the random functions in the figure at left. Hank was more impressed by this than he should have been—I simply recalled what I'd learned in a statistical mechanics course 12 years earlier. I had always hoped to apply these techniques to an oceanographic

problem and Hank delivered it on a platter!

The box model is really only a metaphor for the poorly understood processes that distribute heat and salt in the surface layers of the ocean. But, like many of Hank's ideas, it seems endlessly open and inviting. In the fall and winter of 1991, Hank spent much time playing with stochastically forced chains of boxes and speculating about the temperature-salinity correlation in the mixed layer. In a letter to me he remarked, "Whether any of this has anything to do with the real ocean doesn't seem to be known by anybody yet, but we do have a fresh lead to follow up. And someday maybe there will be a good observational test. In the meantime it is enjoyable material to puzzle over." ❖

Robert Weller

Woods Hole Oceanographic Institution



At 7:00 in the morning, the Clark Lab is quiet. Phones are not yet ringing; there is an hour or more before meetings and seminars begin. It is a good time to make a solid start on writing or computing. On a special subset of mornings, before I moved from the third to the second floor of Clark, it was also the time when Henry Stommel would stop by. Henry's visits were always fun. Waiting for the coffee to brew, perhaps not fully up to speed yet, Henry's infectious enthusiasm and energy drew you into whatever he wanted to talk about.

One morning was spent working out pressure loss in pipes, hydrostatic heads, and other technical details of a plumbing project for his yard. Many discussions were about his work with Jim Luyten and Joe Pedlosky on the ventilated thermocline. Another morning we searched

through catalogs and wondered how to install small differential pressure transducers in the laboratory apparatus he used to demonstrate Coriolis force. Sometimes we went back to his office for a demonstration of an oceanographic atlas or of his latest computer visualization of a geophysical fluid dynamics problem. We also talked about his previous observations of Langmuir circulation, and he encouraged every attempt to learn something more about the physics of the ocean.

These mornings, and Henry Stommel himself, were a source of inspiration. A discussion with him, his energy, and his enthusiasm blew away the silt of the daily routine that settles and tries to bury the fun and excitement of physical oceanography. ❖

Jean Claude Gascard

Université Pierre et Marie Curie (Paris, France)



Henry has been a constant source of inspiration for me. I met him for the first time during the Mediterranean Deep Ocean Convection experiment in 1969. The following year, Henry invited me to spend a year in Woods Hole. This was undoubtedly one of the greatest experiences in my life.

I had always been fascinated by his ability to set up powerful physical concepts using the simplest mathematics. The fascination was also due, I believe, to Henry's immense talent for surreptitiously introducing the mysterious dilemma between the simplicity and complexity of nature. In essence, I strongly believe this was a distinctive sign of his genius, which extended far beyond oceanography.

Henry was the kind of person with whom you don't feel the frontier between home and office. I remember well his cheerfulness in all details of social as well as professional life.

I visited Henry almost every year during the past 20 years, and I have so many remembrances of him that it is hard to pick a few! I will never forget Thanksgiving at Sippewissett Farm and all the MIT-WHOI students Henry loved to have with him on this occasion. There were a lot of happenings in his garden, ranging from hot air balloons to railway tracks (everybody knew of Henry's fascination with railways). In addition, there were serious discussions, dealing, for instance, with Mao Tse Tsung and the future of a billion human beings in China. Henry loved antiques, especially the music boxes he collected from all over the world; he found fantastic specimens in Paris.

I also remember well the Bastille Day we celebrated on July 14, 1971, at the house I rented for a year in front of Little Harbor in Woods Hole. All the French immigrants around Woods Hole gathered there with hundreds of American friends. Henry provided the fireworks, which he asked us to take care of since in the US they were forbidden (for Americans, I supposed!). We had a wonderful party that a lot of tourists, waiting overnight for the Nantucket ferry, enjoyed too, as well as the director of the institution and his guests.

I well remember the winter cruise that Henry led to Barbados on the *Atlantis II*. We left Woods Hole in a blizzard, crossed the steamy Gulf

*I had
always been
fascinated
by his
ability to set
up powerful
physical
concepts
using the
simplest
mathematics.*

*A week later,
back in Paris,
I received a
letter from
Henry with
a detailed
plan for
starting the
construction.*

Stream, and by the time we reached the hospitable Sargasso Sea, we realized that a cat had been trapped outside the boat from the very beginning. It seemed completely frozen. By the time we started the salt fountain experiment, plunging a long pipe in to the deep ocean, both the *Atlantis II* and the cat were melting. Amazingly, the cat recovered just enough to make up his mind to retire in Barbados. The experiment was a success, the pipe spouting salt spray like a whale's blowhole.

More recently, I visited Henry in Sippewissett and we had a big discussion about building a railway track in my garden in Gif sur Yvette just like his. A week later, back in Paris, I received a letter from Henry with a detailed plan for starting the construction. It was a transfer of technology, just as I understand the TGV (train de grand vitesse) has recently been transferred to Texas.

Henry was a master, a friend, and a great companion for me. No one has influenced my career as much as he did. Henry is still very much alive in my heart. The best memorial I can think of would be to rebuild the towed pipe that we both used successfully on the *Asterias* to measure internal waves on Stellwagen Bank north of Cape Cod. We had intended to use it to study the deep convection in the Mediterranean Sea. Perhaps we will do it one day as Henry suggested.

Henry, I miss you. ❖

William Simmons

Woods Hole, Massachusetts



Hank was one of the best organized people I have ever known. He did so many different kinds of things during his lifetime that planning and maneuvering became absolutely necessary to fit them all in.

Once, in 1975, he whisked into my office at MIT and announced proudly he had just finished planning all his major scientific projects for the next decade, roughly to his retirement!

He often made light of planning in conversation, but I suspect he actually came to enjoy it. He could become visibly disturbed when plans went awry, especially if he was feeling boxed in by them.

Hank and Chickie loved gardening, and named their Falmouth property Sippewissett Farm. I was first invited there for dinner in October of 1967, as a young, MIT postdoc. Having no idea what to expect, I showed up, wine bottle in hand, dressed in a suit and tie. Hank and Binks, their large, independent-minded, shepherd dog, gave me the penny tour, and I was mightily impressed. I wanted to ask about each and every thing I saw, but Hank seemed unwilling to dally. In fact, he seemed downright annoyed, which made me a little uneasy. The tour ended in the cold shed adjoining the house, where, next to the food freezer, stood six 3-foot saplings, still in wrapping paper. The pace of his speech quickened as he explained with annoyance they had just arrived from Miller's Nursery, two weeks late, that he was due to go to sea for a month the very next morning, and there would be no opportunity to plant them.

It seemed simple enough. "Why not do it now?" I asked. His face lit up with relief, almost disbelief that a theoretician would offer a practical and unconventional solution.

"Would you mind?" he asked softly.

I told him I loved gardening and he beamed. We dug six large holes parallel to the north wall at the eastern end, filled them half and half with Bobby Weeks's horse-farm product and soil, mixed well, and watered to soupiness. They were set by the waning rays of an autumn sun, just in time for our dinner call. By that time, we were both drenched in mud to the knees, well-splattered just about everywhere else, and generally quite sweaty. It was a feeling I would come to experience on many occasions at Sippewissett Farm (but never again in suit and tie). Hank's sense of relief was obvious, and we all had a wonderfully happy meal, with copious references (always the planner) to the tasty fruit pies we'd be enjoying in three to five years.



Hank was fond of things that happened as a result of serial processes. He liked to try to understand them, and seemed delighted when he could. He especially loved mechanical things, machines that performed tasks: clocks, music boxes, machine tools, scientific instruments, printing presses, and, particularly, trains.

In the 1940s, he installed an elaborate model train in the basement of the rectory of Woods Hole's Church of The Messiah, when it was a cooperative dormitory for Oceanographic bachelors. The train could go from room to room through tunnels Hank had blasted in the cement-work, and he was especially proud of its ability to run through the bathroom. Whilst there, it could be remotely stopped near the hopper, and a tiny spring-loaded flag could be tripped that read: "Passengers should please refrain from flushing toilets while the train is in the station or within its view." He liked to test its efficacy on unsuspecting female guests who were using the facility. His trickster side will be described, I trust, elsewhere in this issue.

He once invited me to listen to a new high-fidelity recording. I thought it would be a special piece of music, but it was authentic train-whistle sounds from famous routes around the country. We sat through the entire recording, while Hank knowledgeably annotated the commentary.

Years later, when Hank was a grandfather, he built, singlehandedly, and ostensibly for his grandchildren, his own private railroad. The track was angle iron and the ties were two-by-fours, all purchased with prize money. Visitors were often invited to ride it. It started at the shack behind his house. Hank donned his engineer's cap, and a copy of the train record played on a small portable cassette recorder he kept there. The engine was roughly handmade of wood and about the size of an orange crate. It was driven by a DC motor, pullies and belts, and an automobile battery. There was a lab-type, double-pole throw switch on the control panel, and a huge smile on his face when he encouraged me to engage it. I did, and sure enough the train pulled out slowly but determinedly to the south, turning eastward across the property along the garden. It did quite well on flat ground, but required assistance along uphill sections. Hank had actually flattened the hills to some

He especially loved mechanical things: clocks, music boxes, machine tools, ...printing presses, and, particularly, trains.

extent, using cinder blocks as standards. It pulled a child's weight quite well. The track ran the width of the property and made a huge U-turn back in the woods, finally heading westward along the north side of the garden. There, at the edge of the lawn, it confronted an impressive abutment straddling the track, and bearing, of course, a small sign that advised reversing the polarity of the switch to enjoy the return ride back to the station. I could see that devilish smile of satisfaction on his face as I arrived at the abutment, the same one that flashed whenever the "Passengers should please refrain..." flag was raised at the rectory. It was cleverly conceived. It worked, it pleased, and, except for the motor, it was mechanical.



Next to his infatuation with ideas, I like to remember Hank's yea saying. He pursued the right to try new things that interested him, protected it fiercely, and helped others, individuals and groups, do the same. He loathed negativism. His magnetic charm and limitless inventiveness brought people together, and often launched them in new directions.

Once, in the early days of POLYMODE, an unspecified delegation of American oceanographers was invited to a joint scientific planning meeting in Moscow. It was to run for a week in the middle of spring semester, mud season in Moscow, and a difficult period for American academicians to get away. I was dreading the cajoling that would be required, but, with Hank as a nucleus, Allan Robinson and I were readily able to assemble a working group of a dozen or so sterling young scientists.

We departed, en masse, at mid evening from Logan airport. On board, I noticed Hank carrying a small green plaid zipper-topped suitcase. It was about the size of a briefcase, and I thought he intended to use it as one.

With the bag on his lap, he opened a conversation with, "This time, I'm really traveling light." When I asked what that meant, he told me, matter-of-factly, the bag contained all his needs for the entire trip. Being new to international travel, I was mightily impressed. "How'd you possibly do it," I asked. For awhile, he dodged the question coyly, building suspense. When the stewardesses were a safe distance away, he agreed to show me. Unzipping the bag, he reached in without lifting the flap, groped around, and produced two tiny spent shot bottles, refilled with a very strong, reddish rum, and offered me one. "It takes awhile to get a drink on these long flights," he said, "and I want to turn in early."

Anticipating an interesting show-and-tell, I pressed on about the bag. Again, he groped under the flap, like a striptease artist, this time producing, with an enormous smile, two rolls of soft American toilet paper, and offered me one. Everyone had heard Soviet toilet paper stories, and we roared hilariously.

There was much excited chatter within our group that evening, but Hank had been up since five. He was yawny and had the seat back at the first opportunity, settling in for the night. I respected that, but I really wanted to know more about the contents of that bag, and pressed one last time. "Okay," he hissed, in a you-asked-for-it tone, and tossed back the flap so I could see for myself. Except for the one roll of

His magnetic charm and limitless inventiveness brought people together, and often launched them in new directions.

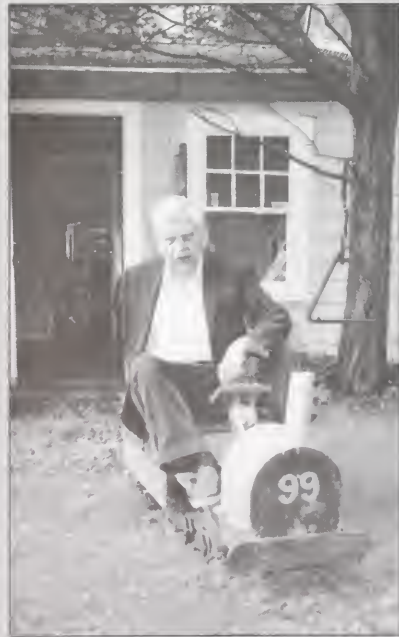
The West Falmouth, Sippewissett & Woods Hole Railroad



W.F.S. & W.H.R.R.

WEST FALMOUTH, SIPPEWISSETT, & WOODS HOLE RAILROAD.

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American toilet paper, a razor, a toothbrush, and a handful of under-clothing, it was absolutely empty! I wanted to press further but his smile was fading and his eyelids were beginning to droop. By the time the refreshment wagon arrived at our row, he was in a deep sleep.

We were met at Moscow airport and driven to our hotel. The first day of meetings all went smoothly. Late that evening, Hank took Allan and me aside to tell us he would be returning home the very next morning. He didn't say why and I never asked, but I like to think it was to coalesce some other group in some other city, to help them do what needed to be done—but they somehow couldn't quite start without his presence.

Throughout the 1970s and 1980s, many an oceanographic program functioned on the Stommel diesel principle: Hank provided the spark during the warm-up phase, after which internal heat assured self-ignition. ❖

Walter Zenk

Institut für Meereskunde (Kiel, Germany)



Train No. 99 to Sippewissett Farm: For me, the day that I first came to Woods Hole is almost like yesterday. It was the week after Labor Day 1973 that the young Zenk family approached the Cape in an Avis car and settled in Roslansky's old house down the street from the aquarium. Although well prepared for our American adventure ("Keep left after you leave the airport tunnel"), we encountered so many exciting new things that we will never forget.

One of these very personal experiences was my first meeting with Henry Stommel. It must have been in October that all of a sudden Hank dashed into my office. I sat in the uppermost northeast corner lab of the Smith Laboratory where I had just started to enjoy the hospitality of Chairman Ferris Webster's Physical Oceanography Department. Hank needed translation of a sentence in a will from a regional court in Idar-Oberstein, Germany. He and I had a funny discussion about—hopefully, I recall correctly—his late, great aunt and the little German village, Stommeln. With this rural town (mostly sugar beets) west of Cologne, the Stommels of Falmouth, Massachusetts, share their name in some complicated way.

As I have experienced so many times since, the resulting invitation to the Stommels' home was nearly

inevitable. How many young oceanographers' families may have been welcomed in the cozy kitchen of Chickie and Hank's home on Palmer Avenue? Hard to imagine they were more impressed than we were by the endless wonders of Sippewissett Farm: the big fireplace, which Hank later converted for a wood-burning stove, decorated with his painted vision of the burning Cape Codder Hotel, the king-size gramophone with the big crank, Hank's favorite video performance of the



Krissy Hogg and Steffi Zenk ride Train No. 99 at Sippewissett Farm in the summer of 1987.

cunning fox who needs to see a tiny mouse dentist, or the railroad network Hank built for his grandchildren.

Not only for them—when we visited the Stommels years later, together with Nelson Hogg and his family, Hank had not forgotten to charge the engine's battery well in advance: Our youngsters had a wonderful time riding the old train No. 99 around Sippewissett Farm all afternoon.

This short remembrance would not be complete without cordial thanks to Chickie. Just as the Zenks in Kiel keep Hank in memory as a genius oceanographer and a great friend, so do many colleagues and their families around all three oceans. ❖

Nelson Hogg

Woods Hole Oceanographic Institution



Over the years of knowing Hank I have been continually amazed at the variety of his interests beyond oceanography and science. One of those we shared was an amateur, but deep, interest in gardening—and when I think back, it is likely that interest that brought us together. At one of our first dinners together, I remember getting an amused laugh when I summarized my own gardening efforts as building up the soil with all the failed seeds that I had sown. With some seven acres to work at Sippewissett Farm, Hank could be quite ambitious and, at one time, I am told, took to selling surplus produce at the roadside. One of our usual subjects of the gardening season was the perennial problem of what to do about the woodchucks. I suspect that Hank grew so much food that they weren't a serious problem but, nevertheless, he did keep a live trap baited and waiting.

One summer, however, the problem was not woodchucks but several deer that had invaded his garden. With the continued encroachment of new housing developments, his land was part of a narrowing corridor linking Woods Hole with the more open lands to the north. The deer were a serious problem, eating vegetables, fruit, and shrubbery by night, so Hank took battle. He set up lights around the garden and had a radio playing loud music all night long, believing that the noise and lights would act as a deterrent. It did as far as the deer were concerned, but one night, apparently attracted by the prospect of a late-night party, someone came into the yard and made off with his woodchuck trap!

I've often thought of Hank as a fisherman as well—a fisherman of people. After returning to Woods Hole, he had the habit of roaming the halls with regular stopping places for chats about whatever new ideas were on his mind. At those infrequent times when he had run out of problems that interested him, he turned to some person, usually amongst the junior staff, that he found interesting, instead. He would then decide what bait to try—namely some problem he knew would interest the unsuspecting, cast it out, and then reel in the catch.

I was fortunate to be one of those caught, and the problem that hooked me concerned the physics of the Gulf Stream recirculation, my

*I've often
thought
of Hank
as a
fisherman
of people.*

main interest over the past decade. Once caught, we then went on to explore other problems of Hank's choosing, particularly the dynamics of the "Hetons" (a kind of vortex). It was a wonderful and invigorating experience from which I learned much more than how vortices work. ❖

Charles (Chip) Cox

Scripps Institution of Oceanography



For me, three memories of Hank stand out. In one he is questioning a graduate student during a departmental exam. He asks totally unexpected questions that delve into simple, yet fundamental, aspects of the physics underlying fluid dynamics. He is a deeply serious professor of immense intellectual power.

Another wonderful memory is a family dinner at his Sippewissett Farm. He is working in the garden putting up fence posts when we come. The tractor is inoperable but an irresistible attraction to my three-year-old son, the garden a treasure of delights to all. Then the evening in the wonderful house with its remarkably diverse artifacts! He is a devoted and deeply happy family man.

A few years later, he unexpectedly comes bounding down our driveway on a weekend, a joyous figure full of fun and ideas.

What I remember best about all my encounters with Hank is that ideas flowed out of him with no restraint; he was a totally generous man. ❖

Bernard Saint-Guily

Muséum National d'Histoire Naturelle (Paris, France)



Henry Stommel was ever wonderful with fresh and unexpected views. Indeed he was naturally looking at things "as never yet seen," and he was sensitive to the casual and unusual sides of facts and circumstances.

I remember one of his at the same time funny and romantic tricks. Once I was just arriving at his home when Henry dragged me off directly to the garden to hear, close-up, a few blasts of a genuine old locomotive whistle, which he had attached to a compressor. For an instant we remained motionless, delighted with this rich and evocative sound.

He was also enthusiastic, and pressured himself to clear up without delay all vagueness in ideas and notions. Very long ago, perhaps the first time I came to Woods Hole, we were speaking of chance in discoveries, and I was curious to know the etymology of serendipity. Immediately Henry took me to the wonderful Woods Hole library, urgently to consult the *Oxford English Dictionary* and illuminate the question. ❖

*Ideas
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man.*

High-Iron

by Starbuck

THIS YEAR IS THE 150th anniversary of the opening of the railroad from Boston to Worcester. The through route to New York City didn't open till a few years later, but already in 1833 ground was being broken for the new line at Stonington, cuts and embankments made across farmers' meadows toward Cranston, the levelling survey being carried forward by a 17-year-old lad, fresh from an apprenticeship at the construction of the Grand Western Canals in the southwest of England.

We happened to find his daybook among some family papers. It conveys the sense of adventure and excitement as the first railroads were being laid out: setting out the pegs, calculating the cut and fill, negotiating with the laborers about their 65-cents-a-day pay.

The chief engineer on the job was Major George Washington Whistler, age 33, who had resigned his professorship at West Point to take up a career of railroad construction. When the Tsar of Russia decided to build a railroad between St. Petersburg and Moscow, he engaged Whistler to do it for him. American companies took over the Alexandrovsky State Factory just outside of St. Petersburg to build locomotives and rolling stock, an early example of technology transfer.

Many of the transplanted American, Scottish and English families continued in business in Russia until they were expelled during the Bolshevik Revolution.

Three weeks ago I rode Amtrak from Wilmington to Boston. The cars seemed as crowded as during the War. It was a leisurely, comfortable ride, and a good chance to renew acquaintance with old familiar landmarks. And though we made our obligatory stops at Old Saybrook and Westerly, many of the stations on the old railroad maps seem to have disappeared. What has become of Slocum, Leete or Wood River Junction?

The railroad mileage in this country increased rapidly until the 1920s, when it first began to decline. At one time there was a mile of track for every four square miles of land area in Massachusetts. Amtrak is the gallant, though tired, survivor. Now, according to the TV news, David Stockman says it's got to go. He's probably right: it must be cheaper to send a Senator on a junket to outer space than a high-school girl for a train ride to Philadelphia. Whoever asserts otherwise will be suspected of high irony. ❖

The Enterprise
May 20, 1985

Michèle Fieux

Université Pierre et Marie Curie (Paris, France)



Hank was like a second father to me. When I arrived to work with him at Woods Hole in 1973, I was impressed with his stature, but also soon found another family in Hank and Chickie. I remember when Hank first showed me his nice garden, and discovered my interest in gardening (although I never had a garden), he said, simply, "Here is a row for you. You can grow what you like." Every weekend I had a marvelous time with him in his garden.



Michèle Fieux's photos show Hank Stommel in his Sippewissett Farm garden, holding one of the Stommel cats, and at his lathe.

One day, at Sippewissett Farm, Hank decided to harvest his wild grapes. I was a little surprised to see him with a chain saw in his hands. We went to his woods where he showed me the grapes growing on trees. All of a sudden Hank started the saw and cut down the tree to get the grapes. To my astonishment, Hank told me it was his way to get grapes and wood

for winter at the same time!

One night, Hank invited another friend of his and me to one of Chickie's famous dinners. We were in their marvelous kitchen at Sippewissett, surrounded by Hank's paintings. The fire hummed companionably and the cat and dog were stretched out, asleep. Hank was talking about the stars. He asked the visiting theoretician to calculate the distances between some stars, and he asked me to go outside and measure them with a sextant. Then we could compare the results. Hank always managed to make his guests feel comfortable and important in his home.



Hank loved to play pranks. One of his favorites was to wait until friends were visiting his garden, and then suddenly blow his loud train whistle!



Every time I traveled to Woods Hole, either Hank or Chickie came to fetch me at the airport and bring me home. One day I saw Hank, after working for hours, go outside to have a tour of the garden on the little home-made train.

Hank was able to make anything with his hands. He was

Counting Our Blessings

by Starbuck

OUR ATTENTION IS SO directed by the media towards the shortcomings of our society that we often forget to count our blessings. One that particularly comes to mind is the Falmouth Hospital, which but for the efforts and devotion and vision of some of our physicians and citizens might never have come to be.

Only a few short years have passed since we had to rush to Wareham or Hyannis in an emergency, and now, seemingly miraculously, there it stands—a secular temple of care and kindness and medical knowledge. There is something comforting and homely about a community hospital: faces known for half a lifetime, the nurses with whom your own children went to school. And all of this, grown out of the good intentions of our neighbors great and small.

It was not always so. About fifty years ago, in the mid-thirties, a little girl and her brother on Shore Street came down with what was diagnosed as infantile paralysis. Dr. Oscar Simpson, who used to live in a red-trimmed white house where Li'l Peach Grocerette now stands, tried to arrange for the town ambulance to take the pair to the Children's Hospital in Boston for further examination.

Polio was then viewed with the same fear that AIDS is now. It seemed that no one was going to volunteer to drive the two children down to Boston—but finally one fireman stepped forward to take the risk. It must have taken considerable resolve and courage for anyone to contemplate the possibilities of infection, especially for a father with children of his own.

It is one of the redeeming features of our human race that, when everyone else wrings his or her hands and turns away, there is always someone else who stands firm and does the needed job. We have our splendid hospital now, largely through the devoted efforts of a few. The town ambulance sped to Boston on its mission of mercy those long years ago, on account of one man's courage and generous heart. He is my neighbor Stanley Fisher. The little girl is my wife. ❖

The Enterprise
February 20, 1985

always trying to explain difficult theories in the simplest way possible. He was always passionate about something new and trying to communicate these passions. He was a wonderful friend, and I miss him very much. ❖

James Luyten

Woods Hole Oceanographic Institution



A *annealing*: Although I first met Hank during the summer of 1968 at Walsh Cottage, I didn't start working closely with him until he returned to Woods Hole in 1978. I was in an office next to Hank's, so we often met in the hall. It began by his just coming to ask me what I thought of some idea or problem. It was flattering to be asked and to have Hank's attention. Hank was always on the lookout for people who could complement him, particularly in mathematics, where he felt shaky. After several months of these sporadic conversations, we began talking about a specific model and said, "Well, let's work it out." We continued from that point for about 14 years. It has certainly been the most exciting and entertaining period of my life, both scientifically and personally.

What were the qualities of working with Hank? Fire, sweat, heat, out of control, laughter, despair, rebirth, cigar smoke.... The imagery is emotional because Hank was so passionate and emotional.

Most collaborations began with a free-ranging conversation, sparked by some new observation, a remark made at one of the seminars, a recent paper, or, as it turned out, one of the questions that Hank had been pondering for 40 years. (In 1955, Hank laid out a blueprint for himself and oceanography in an address to the staff of the Oceanographic, entitled "On the Present Status of Our Physical Knowledge of the Deep Ocean.") That pretty much covered most of oceanography, as well as most of our human experience. The next stage was what I call the conflagration, in which we tried to get a problem going. It was like trying to build a fire with damp wood, little kindling. We needed the spark of curiosity and a feeling that we might be able to make some progress, a problem where you could try to develop an explanation in simple terms (i.e., how does a gyroscope really work when thought of as point masses?). If the explanation made sense, and could not be further elaborated, that was nice, but there was no place to go with it. There was no rub. Time to get some more coffee or read the mail....

Sometimes, we couldn't get any purchase on the question at all, everything was just too wet to burn, even though it looked like great material. That was sort of depressing...

Then there were questions where the simple explanations lead us further afield, to larger or deeper questions. This is where the fire really got going, a real conflagration. It worked best if our initial guesses turned out to be wrong. It became very intense, all ideas were thrown into the fire, challenged, picked apart, discarded; all seemed lost and hopeless. It was very chaotic and all attribution was lost. We would calculate, dig out some atlases, analyze some data, plot things up, drive down to the MBL Library, whatever was needed. In the evening and on weekends, the

*What were
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Fire, sweat,
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out of control,
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smoke....*

phone would ring and Hank would start as if in midthought. It was like being at sea in a storm—you knew that you would just have to ride it out. Hank had such a deep intuitive feeling for how the ocean worked that he could keep his bearings where few others could.

Often this process led us to seeing what was essential for modeling the phenomenon at hand. Then began an annealing process, in which we could move ahead a little and see what could be thrown away as not being essential to the understanding. As we got closer to understanding, the pace increased. More phone calls, but this process was a bit more controlled—putting the ideas into the fire to strengthen them rather than transmute them involved talking them through again and again, with colleagues and visitors. For Hank, the continuity of the communication was part of the creative thinking. Everything was still up for revision. This was also the time to start writing and, here again, you could hardly keep up with Hank. His passion to express himself was given free reign. Every morning copies would be distributed to all who were interested, as we struggled to get it right.

In the end, we got the manuscript together, checked everything, made the figures. After the manuscript was sent off to the journal, Hank would come in and say, “Now what are we going to do next...” ❖

Philip Richardson

Woods Hole Oceanographic Institution



Dropping by to chat with Hank was like going on a roller coaster ride—a scary acceleration down a steep slope, a sudden swerve around a tight turn, a dive into a dark tunnel, and out again into sudden brightness. You were never sure where you were going or how long you would be gone. It might last days or weeks or even longer. These rides were always interesting and usually fun.

My favorite visit to Hank’s office began after a seminar he gave about the interaction of discrete eddies. When I showed him some new drifting float trajectories in eddies near the Gulf Stream, he became very excited and off we went on one of these rides. He quickly launched model floats into his computer-simulated eddies, and for hours we played wonderful games, trying to guess the number of eddies and their behavior by observing trajectories of model floats. Hank dashed back and forth between his two computers, changing model parameters and the number of floats and eddies to see what would happen. With quick side trips to the blackboard he worked out a mathematical scheme to calculate the properties and behavior of eddies using a minimum number of floats.

At one point he became so excited and spewed out ideas so fast that I was afraid he would collapse right there on the spot. We both hung on and survived. This particular ride lasted a few weeks, but that first day was the wildest and most fun. In a few hours he taught me a lot about the interpretation of float trajectories, and, more importantly, how he approached and solved a problem swiftly, directly, and playfully.

Hank had such a deep intuitive feeling for how the ocean worked that he could keep his bearings where few others could.

Once in awhile Hank and I would cross the street from his house to Doug Webb's garage to see the latest exciting technical developments of the Slocum vehicle. A Slocum is a mini submarine powered by the thermal stratification of the ocean; it is named after Joshua Slocum, the first person to sail single-handed around the world. Hank envisioned hundreds of Slocums porpoising through the ocean, monitoring it in the same way as weather balloons monitor the atmosphere.* Once again, we would be off on another ride. ❖

*Henry Stommel's "The Slocum Mission" can be found in *Oceanus*, Vol. 32, No. 4, Winter 1989/90.

John Van Leer

Rosenstiel School of Marine and Atmospheric Sciences



*Hank
envisioned
hundreds of
Slocums
porpoising
through the
ocean....*

Those of us who worked with Hank Stommel were all exposed to his amazingly diverse interests, infectious curiosity, and keen original mind. Many, however, may not be aware of his fascination with things mechanical, particularly those involving steam. As a person with an engineering background, he would regale me with his mechanical adventures. Hank loved to tell stories on himself, such as the following sample.

About 20 years ago, Munroe's Shipyard in East Boston decided to get rid of an ancient coal-fired steam crane. This prize was self-propelled on caterpillar treads and had a large clamshell shovel, which Hank could use to clear the brush out of his pond at Sippewissett Farm. Best of all, the boiler had enough capacity to blow a locomotive steam whistle Hank had previously acquired.

Hank and WHOI naval architect Jonathan Leiby somehow arranged the purchase of the crane and its transport to Woods Hole on one of Dan Clark's barges. The next question was how to unload it. The next Saturday morning Hank calculated that the tide would bring the deck of the barge to the same level as Dyers Dock, allowing them to drive it off the barge under its own power. Early on the appointed morning they set about making a fire under the boiler in the firebox to get up steam.

As they began to move the crane it became painfully evident that the steam pressure was not going to move the crane more than halfway off the barge. Meanwhile, the tide was falling and the crane began to tilt. They quickly decided to intensify the fire by adding a quick shot of drain oil to the blaze.

Unknown to them, the firebox had small rust holes in the bottom, which allowed the ignited oil to run out the bottom and onto the wooden dock and barge. Soon the dock and barge were starting to burn, leaving them with the interesting choice of whether to concentrate on fighting the fire or moving the crane. Moving the crane took precedence, and they succeeded in getting it completely onto Dyers Dock, whereupon they could concentrate on putting out the fire. ❖

Dennis Moore
University of Hawaii



Hank Stommel loved to pose and solve interesting problems, and hated to make errors. I once asked him about global sea-level rise, and he sent me a sketch of Woods Hole awash! He asked me to have a look at Compton's 1913 *Science* paper on a laboratory demonstration of the earth's rotation. That was much more interesting to Hank! Compton's sketchy theory was incorrect by a factor of two, but Hank spoke of Compton in heroic terms, and really wanted him to be right. In the appendix to our book on the Coriolis force [*An Introduction to the Coriolis Force*, Columbia University Press, 1989], Hank discusses Compton's work very politely, and never says outright that Compton made a mistake.

Hank was on the fourth draft of the Coriolis book when he asked me to help with it. Typical of Hank's generosity, he had Columbia University Press rewrite the contract, giving me an equal share. It was very demanding, but great fun, working closely with him. His enthusiasm was tremendous and his mirth was contagious. Hank was very meticulous. At the last minute we had to have a figure redrawn to correct a careless drafting error we had cautioned against in the text and then made ourselves. Hank said we would have been "hoist by our own petard" if we did not correct it. I added an acknowledgment to the draftsman who "created" the redrawn figure on the computer. Hank did not see this until the book appeared, and then gently suggested that if the book were ever reprinted we should change "created" to "drew." Hank said "created" should be used exclusively to describe what God has done. ❖

Gabe Csanady
Old Dominion University



Among my fondest memories are the dinner parties at the Stommels'. We never knew what to expect: a demonstration of black light, Hank reading aloud a passage from an old classic, or a ride on the narrow-gauge railway in the yard. There were, of course, lively discussions on topics ranging from Stephen Hawking's *A Brief History of Time* to new ideas in evolutionary theory. We also looked forward to the new or modified painting by Hank, hanging over the fireplace: Would it be a cat cemetery, a cat with mesmerizing eyes, or the Cape Codder Hotel burning?

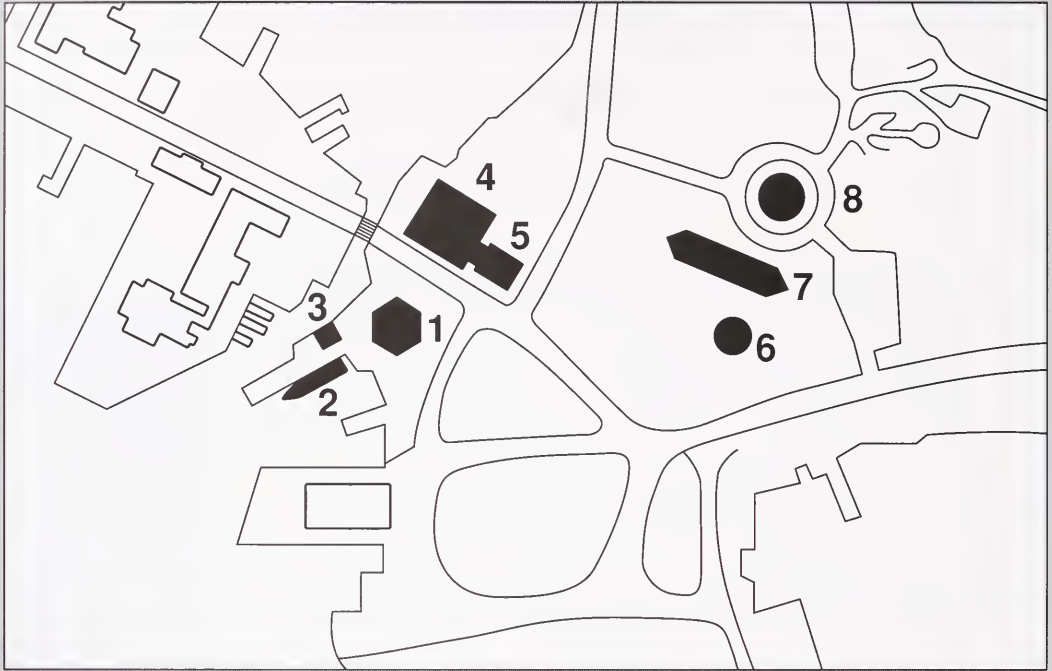
Hank and Chickie were very close. Once, when Chickie was in the hospital, we invited Hank for dinner. At the last minute, Hank called, saying that he could not leave the phone. So we took the dinner over to his place, which cheered him up a bit. When visiting hours came, Elijah and Hank went to see Chickie.

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Continued on page 124

The Woods Hole Marine Exhibit

During 1989 and 1990, a Master Plan was drawn up for Woods Hole Oceanographic Institution property. After hearing about a proposal for a deep-sea "telepresence" center, Hank Stommel offered this idea.



1. Information Center
(gift shop, rest rooms)
2. R/V *Oceanus*
3. DSRV *Alvin* (The Abyss)
4. Museum
5. Lecture Hall
6. Hydrosphere
7. The Wave
8. Operations Center

1. Information Center

It is a good idea to start your tour of the Marine Exhibit at the Information Center, where you can obtain a free brochure and a map of the exhibit. The center also has a souvenir shop and rest room facilities.

2. R/V *Oceanus*

R/V *Oceanus* was one of Woods Hole's most active research vessels. When it was retired, *Oceanus* was moored permanently along the dock at the point on the map indicated by the numeral 2. Visitors are welcome aboard to inspect

the laboratories, deck equipment, winches, living quarters, engine room, and pilothouse.

3. The Abyss

On the dock, next to R/V *Oceanus*, there is a rather ordinary inconspicuous Cape Cod shingled shed. Inside you will find the widely known deep-sea submersible *Alvin*, open for your inspection. This is the vehicle that enabled the discovery of the thermal vents along the deep crests of mid-ocean ridges.

The exhibit also includes a special chamber in which visitors can experience a simulated deep dive to view one of these vents, with the flood lights of *Alvin* illuminating a "black smoker" and the other-worldly life forms around it.

4/5. The Redfield Marine Museum

The Redfield Marine Museum is housed in the remodelled Redfield Laboratory. The main building contains a wide variety of exhibits, such as the collection of historical instruments, models of research vessels, specimens of marine animals and plants, etc.

It also houses a lecture hall (5) in which formal lectures for the public are regularly scheduled during the summer months.

6. The Hydrosphere

One of the main results of the World Ocean Circulation Experiment (WOCE) was the series of hydrographic sections of the WOCE Hydrographic Programme (WHP). These were prepared in the uniform style of the 1960 Fuglister IGY atlas of the Atlantic, the

Scorpio sections, the Warren and Toole sections of the Indian Ocean, and the Atlantic sections by McCartney.

At the end of WHP there were 30 modern sections in this color-printed format, each section comprising seven panels about 2 feet by 3 feet. They are mounted in seven superposed horizontal rings within a cylindrical wall 30 feet in diameter and 12 feet high. It is an impressive and informative exhibit, rich in detail. It gives a good, accurate, and positive picture of what we do at WHOI.

The exhibit is presented inside a special building "The Hydrosphere," something like the display of the celestial sphere in a planetarium or the fish world in the Boston Aquarium.

You are suspended inside the building. If you look up or down you will see that inside the hemispherical ceiling

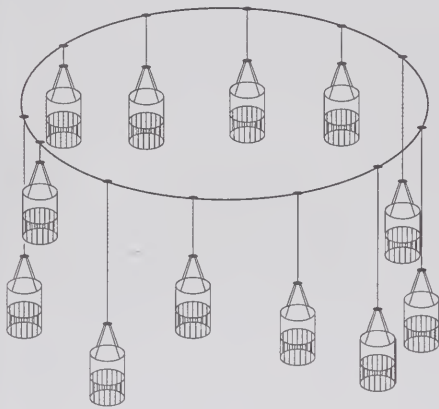


and floor there are maps of submarine topography. The lines of the sections are clearly marked on these maps.

The color-printed sections are mounted on the inside of the cylindrical walls.

Access to the display is made possible by fairly narrow balconies. However, it is more exciting for visitors to be moved about by a carousel-like ring of baskets (made to resemble CTD rosettes that measure conductivity, temperature, and depth and take water samples) that are suspended from trolleys on a circular track around the ceiling. Winches raise or lower the baskets so that the various circles of panels can be viewed closely. The winch can be controlled by the lecturer for formal presentations to groups, or can be run personally by the individual rider who is exploring the exhibit by himself with aid of an explanatory audio set.

There are various dramatic visual effects such as controllable spotlight-

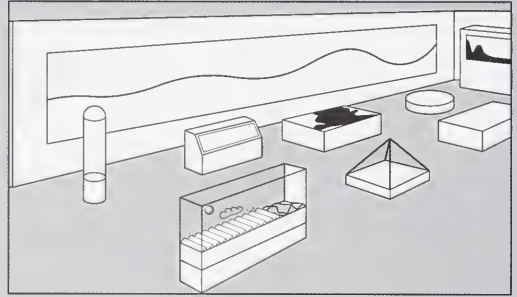


ing. Some sense of the vertical distribution of temperature in the ocean is imparted by strong thermal stratification of the temperature of the air within the building, so that one runs through a strong temperature change as the rosette ascends and descends.

7. The Wave

The Wave houses several exhibits that illustrate various features of the ocean

in motion. Among these is a working Air—Sea Climate Model that illustrates the complex interactions of the atmosphere and ocean as they absorb and reflect radiation energy from the sun, then are set in motion by buoyancy



forces. The motions of air and ocean are the winds and currents that transport the heat to other geographical locations, from which the energy is all dispersed to space again by infrared radiation. These processes are all simulated by the working model using real fluids. The physical principles are described and explained. Another model shows the Gulf Stream, how it gathers up waters flowing around the subtropical gyre of the mid-ocean, how it intensifies and channels the warm waters back towards the north, and how it fans out to modify the climate of Europe. This model also operates using real fluids. It illustrates the process of "ventilation."

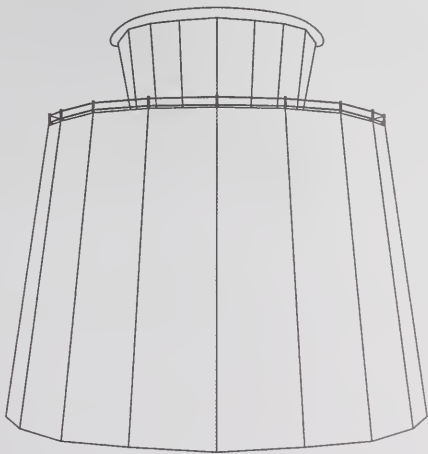
The Deep Ocean model shows how deep waters are formed by sinking in polar regions, and by overflows from adjacent seas such as the Norwegian Sea and the Mediterranean Sea. These waters have different colors in the model, and one can see how they make up different mixtures in each of the deep sea basins.

El Niño is an exhibit that describes the interannual fluctuations of temperature that occur in the equatorial Pacific Ocean. The idea of data assimilating dynamic modelling and prediction of El Niño is illustrated with an actual computer.

There are also some smaller exhibits that illustrate the physical ideas behind Coriolis Force, the reason for two simultaneous high waters of the semi-diurnal tide, ideas about sea ice, etc.

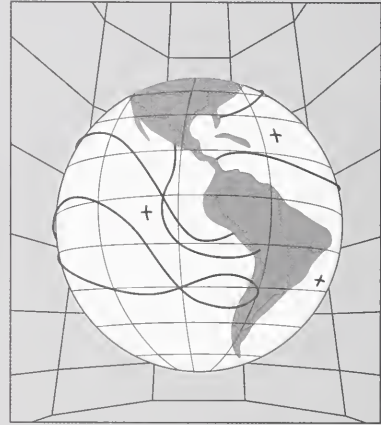
The building is a long rectangle in plan. It has a low profile. The south wall is a double thickness of heavy plate glass, extending the full length of the building. In the space between the two sheets of glass are two immiscible fluids of different density and color, in which a giant internal wave is maintained in motion.

8. Operations Center



Built in the form of a rather broad lighthouse, sheathed with a dark blue-green glass exterior, this is an exhibit aimed at communicating the magnitude of past and ongoing efforts to gather data about the ocean. Upon

entering, visitors find themselves enveloped in a dim blue-green light on all sides, filtering through the outside walls, much as though they are standing at the bottom of a shallow sea. The light is brighter and yellow near the



top. In the center of the tower is a nighttime globe of the world 15 feet in diameter. Images can be projected onto the translucent surface of the globe from a computer-controlled laser gun inside the globe to depict patterns of geographical distribution of stations, moorings, float tracks, XBT lines, etc. drawn from a complete and exhaustive data file of all historical data sets. In a sense this globe simulates the "card index" of a world-class library; it lists and locates both in space and time all the observational material that oceanographers have to work with. The display can be manipulated by the tour guide, or lecturer, to illustrate the entire history of scientific oceanic exploration over the past century and a half. In this sense it is something like a planetarium (seen in this case from the outside rather than the inside of the globe): one views the evolving constellations of data records as patterns

of bright-colored dots over the surface of the ocean. The lecture begins in 1860 with the surface observations begun by Maury. We see how the data follow the shipping lanes up until our own time. Then the lecture turns to deep-sea hydrographic measurements, beginning with the historic cruise of HMS *Challenger*, and shows the routes of all the succeeding cruises over the world as the years roll by.

Next the lecturer shows, in more recent years, how moorings have proliferated over the oceans, and the tracks of neutrally buoyant floats, whose depths are coded by color.

Next a composite cinematic view of satellite thermometric data is projected upon the globe.

Finally the actual tides over the whole globe are predicted using the Schwiderski model. The present daily progress of the tide is displayed on the sphere so that the viewer can find the height of the tide at any point in the world during the day.

Ascending to the top of the tower visitors enter the actual Operations Center. This room occupies what would ordinarily be the lantern of a lighthouse. Outside it, there is a surrounding broad, railed platform with splendid views of the Hole, the Elizabeth Islands, the Vineyard, and the ocean beyond.

The Operations Center is in touch with all programs of oceanographic observation as they are occurring in real time. There is a large map that locates the position of all research vessels, and indicates the nature of the measurements they are engaged in making. It is updated daily by electronic mail. It also shows the status of major moored buoy arrays, such as the Tropical Ocean-Global Atmosphere network in the equatorial Pacific. When a tsunami alert occurs, the pertinent epicenter and times of arrival of the tsunami are plotted on this chart. The general effect emphasizes the importance of collecting data from the ocean, and the magnitude of the effort required to do so. ❖



Nantucket

Dear Aunt Anne,

We have arrived here at last, and the house seems to have survived the winter, so we are practically already settled in with the kids.

The ferry boat was delayed for two hours and we had some time to spare in Woods Hole. You would be amazed by the changes there.

The Oceanographic Institution has built a wonderful new exhibition center that gives a splendid overall view of its activities. There are so many different exhibits that I haven't the time to tell you about the Hydrosphere or the Ocean Climate buildings. They have moored one of their retired research vessels at the little dock behind where the old drug store used to be; it is open to visitors and it is arranged to show how different equipment is used. I rather liked the wet lab with

its arrays of reversing thermometers, the winches, various buoys and instruments cluttering the deck, and the kids loved the pilothouse.

To my mind the best part of the exhibit is near the head of Little Harbor—the so-called Operations Center. It is a four-story structure sheathed entirely in translucent green and blue glass, shaded more lightly at the top than at the bottom. When you enter, you get the impression of being at the bottom of the sea, with the sunlight filtering down from above. It is a little eerie and cool. Nested inside there is a second building with spiraling floors with various exhibits, of which more later. At the top one emerges upon a large open roof with a railing around it, like the platform about the lantern of a great lighthouse, and splendid views of the Hole, the Elizabeth Islands extending down the Sound, and the Vineyard.

In the center of the roof in the place of what would ordinarily be the lantern is a large hall that contains the Operations Center. I can best describe it as the brain of the Institution. The dominant feature is a great map of the world on which the locations of all the world's research vessels are marked by lights, and it is kept up-to-date by electronic mail, and the nature of the scientific work that they are doing is cleverly indicated. You can also see at a glance where major experiments are being conducted. I was impressed to see the extent of ship-borne research activity being carried out around Japan. Also I was unaware of such enterprises as the arrays of moorings set out along the Pacific equator. They have hundreds of free-floating instrument packages in the ocean too. The complexity of the tracks that they follow is intriguing, and I wonder that the scientists can make any sense of them. One cannot come away from this exhibit without being impressed by the global nature of ocean research that these people are attempting to conduct.

There is a great deal else to see, and it was wonderful for keeping the children occupied during our brief layover. Jimmy was thrilled by *The Abyss*—a simulation of a deep sea dive down to the Mid-Ocean Ridge to see a thermal vent. Quite spooky with the black smoke and other-worldly (plastic, I think) animals.

Jimmy says he wants to be an oceanographer. Joyce, all eight years of her, is now going to become a maritime lawyer.

When you come to visit us this summer, be sure to allow enough time to have a look for yourself. It is very much worth arriving in Woods Hole well ahead of the departure time of the ferry.

Your loving nephew,
John

Roberto Frassetto

Istituto per lo Studio della Dinamica delle Grandi Masse

(Venice, Italy)



Oceanography from a Camel. A magnificent disaster and a triumph for superstition: It was 1977. Henry wanted to record over a sufficiently long term the temperature variation in the Somali upwelling area. There was no oceanographic ship available.



The lighthouse: filling the oil lamp for the night

As an Italian, assumed to be a friend of the Somali people and their authorities, I was to take Henry's temperature recording instruments, calibrated for the purpose, carry them 1,000 kilometers north of Mogadiscio (we did not know how) to a beach near Bender Beila, a small, superstitious village of African huts.

The people ate fish and a few gazelles, traded with India on small handmade, wooden sailboats that were constructed on the beach without a nail or screw, rather sort of sewn together.

Through the Italian ambassador in Mogadiscio I got the Somali Police Department's one-engine Cessna plane to take me, a diver, and a minimum of essential equipment, to a stony landing strip along a long, moonlike coastal

area. We stopped midway to get fuel from barrels that had been rolled over sand, which raised great concern for us travelers and perhaps also for the plane's engine.

We arrived at the end of a day, 30 kilometers from the coast and from Bender Beila. Luckily, we found a camel and his desert-dwelling owner willing to transport our instruments all the way to the sea, provided we would run along after the camel at the camel's pace! We traveled during a moonless night with distant wild animals roaring.

We were expected at the village and given rest in the only empty stone building, which served, with an old oil lamp, as a lighthouse for the village. This is the reason we fortunately found, in the following days, several shipwrecks on the beach, from which we cut off all the material we needed for mooring our instruments.



Finding the first anchor at Bender Beila

We traveled during a moonless night with distant wild animals roaring.

In a week we put together a reasonable anchoring system and moored the instruments offshore in a well-fixed position with the help of two "sambuca" sailboats, eight men rowing through the breaking waves to get off to sea.



Ready to conquer the breaking waves

With great relief we succeeded in returning to Mogadiscio and Italy after having instructed the telegraph man of Bender Beila to take wind, air temperature, and pressure measurements regularly from his so-called station, with our instruments.



Dropping the anchor

Six months later we returned. This time Henry sent with us his freshly graduated Ph.D. Ants Leetmaa, who could not fit in the small plane but, being a new, young professor, could endure a three-day trip in a bumpy Land Rover. He made it, and we began searching for our instruments in the sea, exploring the bottom for days in a shark-infested area with a dense biological chain. No success.

Noticing our desperation, the local wise men took pity on us and said, "Perhaps you should look in the desert."

The people of the village had feared that those instruments on the bottom of their sea would bring evil and would make trouble for fishermen and commercial activities. To make matters worse, the instruments were painted black, so they had to be destroyed and thrown far away in the desert.

It was the end of our adventure, from a camel to a magnificent disaster. We had not considered the most important factor: superstition.

A few years later the same measurements were made from ships and proper oceanographic buoys. ❖



Sailing in the sambuca to the mooring point. The "chief scientist" carries the umbrella, a symbol of power, given him by the village chief.

*It was
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from a
camel to a
magnificent
disaster.*

❖—**Gabe Csanady** Continued from page 115—❖

We never talked “shop” on social occasions, but Hank called on the phone occasionally to discuss what he called one of his “wild ideas.” It was a privilege to be in on the workings of his original mind. The greatest compliment I ever had was when Tom Spence remarked that I had “the Stommel view of the world.” At my farewell party at WHOI, Hank presented me with the medal of the Maria Theresa Shoals Society. Apparently, Hank and I were the only members.

Last summer we were fortunate to have Hank and Chickie visit us at our cottage on Lake Huron. We did not know it would be our farewell to Hank. We had our usual lively chats, sitting on the deck, watching the waves, Hank smoking his cigar. We saw him to the airport bus in Kitchener, and waved good-bye to a wonderful friend. ❖

Oleg Mamaev

Moscow State University



Editor’s note: This contribution is excerpted from a longer biographical note that O.I. Mamaev wrote about Henry Stommel. It was translated for this volume by Kirill Pankratov.

“It seems that he will never stop doing oceanography,” answered WHOI Oceanographer Terrence Joyce, when I asked him about Stommel during his 1989 visit to Moscow University. This phrase was literally imprinted on my memory, and now has surfaced again. “Great Euler stopped computing” I read somewhere long ago, “stopped computing at the moment of his death.” Great Stommel stopped doing oceanography but will remain in oceanography forever.

Stommel was a person who helped other people to live. Working in oceanography (though quite weakly), I always remembered that there is somewhere on Earth a man whose very existence gives one strength and confidence. I felt this particularly when reading his book *The Gulf Stream*. I was astonished by its simplicity and clarity, and it seemed to me that I could write something like this. And although it was not so, I could not, of course, write like that—the book did not repel (as unjustifiably complicated books sometimes do) but attracted, seemingly saying, “Do not be afraid! You too can contribute.” His works were always like a hand of help and support.



Having arrived at the UNESCO oceanographic service in Paris at the end of 1969 I learned that Henry Stommel was also in Paris as an invited lecturer at the physical oceanography laboratory of the National Museum of Natural History. I wrote to him, and he answered instantly with his letter of December 18: “I was glad to receive your letter of December 16 and hope to see you soon. Tomorrow I will go to Switzerland, where I will stay until January 3. I will try to visit you at UNESCO immediately after my return.”

*It was a
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of his
original
mind.*

And he did—he came to my office, took off his old beige mackintosh, and, as if we had never parted, said, “Look, 1962 was not so long ago!” (1962 was the year we first met in Moscow.) I do not recall all the details of this meeting but he requested that I visit him at the laboratory of physical oceanography, and I gladly accepted.

When I visited, we met with the head of the laboratory, Henri Lacombe and his colleague Paul Tchernia, and after some general, polite conversation Stommel said: “Now we’ll go. I will give a lecture to Mamaev!” I was embarrassed: Stommel himself will give a lecture, just for me alone? And we went to his office, and he began a “lecture” as he put it. He did not write complicated things on the blackboard, which I would not understand, but instead showed me “simple” oceanographic sections, observational results, records of Webb’s vertical floats, and other materials—the results of the studies of a deep convection in the Gulf of Lyon when a winter mistral cools surface waters, making them sink spontaneously to great depths. These were the results of the MEDOC project in which Stommel participated. Stommel gave me a lecture that I will remember for life—this was one of our few meetings, and he talked with me as an equal, like an old friend—could I forget that? Did he want to enlighten me or check some of his conclusions with me? Probably both, because contacts with his colleagues satisfied his internal need as a scientist, regardless of whether his colleague was a student or a well-known specialist. The ocean interested him as a whole and a “simple” oceanographic section was as dear to him as a new idea.



Henry Stommel was ailing for a long time with cancer. As I remember, I first learned about it from Joseph Reid in 1982 during a short visit to Scripps Institution of Oceanography in San Diego. I came to see him during a break in the session. Reid worked far away from the main Institution buildings, in a small wooden house on a hill where there was a wide path from the road among dwarf pine bushes, where many striped squirrels ran away from me. Joe told me about Stommel’s illness, and it was strange to see him, with the words “God, grant him health,” cross himself as Russians do (I haven’t seen anybody abroad crossing oneself when not in a church), and I felt uneasy.

Stommel bore his illness stoically because of his spiritual and moral strength; he talked about it in a matter-of-fact manner. In a short letter he wrote: “Dear Oleg, thank you for your Christmas card and good wishes. Three weeks ago I had an operation for cancer, but I am already at home and feel well. My best wishes to you and your wife. Henry.”

These words were written on a small card with the picture of his home in Falmouth, an old Quaker wooden cottage, almost 200 years old. (There is no date on this card, and postal stamps of Buzzards Bay, Massachusetts, and Moscow are unreadable. I think it was 1987, or maybe 1988?)



Henry Stommel was awarded honorary degrees by several universities and many prizes, but he was indifferent to all these decorations. In

The ocean interested him as a whole and a “simple” oceanographic section was as dear to him as a new idea.

1976 he was elected a foreign member of the USSR Academy of Sciences; in those years this was a rare occasion, suggesting that he was above contemporary events and fashions.

Once Ludwig Boltzman said about Kirchoff: "In Kirchoff's life there was nothing extraordinary, characteristic to his genius....Great events happened exclusively in his head." Those words are applicable to the great personality of Henry Stommel, who has left us now forever. ❖

Rui Xin Huang

Woods Hole Oceanographic Institution



A *Trip to the MBL Library with Hank:* It was a quiet winter day, January 9, 1992. I was busy working on the generalized Goldsbrough solution in my office. Hank came in twice. We were puzzled by the oscillations existing in a purely haline circulation.

Suddenly, Hank and I remembered an article in *Scientific American* published some years ago about diffusive waves. Hank immediately decided to go to the MBL Library to find the article. "I can think about the salt oscillator when I stay in the hospital," he said. On inspiration, I thought maybe it would be fun to go with him, so off we went in his car.

Just as we passed Fenno House and headed for the main road, Hank said, "Could you show me where those old trees are you told me about a few months ago, which are dying?" Well, Hank turned the car around and we went along Oyster Pond Road to a swamp beside the road. Hank and I went down to the swamp along a tiny path. As we soon found out, there are many big trees in the small swamp, Atlantic white cedars. They are not so old after all. I was rather embarrassed, for I thought they were very old and valuable. Hank was quite cheerful saying, "That is good. Since I have seen them, I don't have to worry about them when I go to the hospital."

In the library, it took us a few minutes to find two articles about diffusive waves. Hank was very pleased and told jokes and funny stories all the way back to our office.

That was one of the last few days before he went to the hospital, and he was busy talking to several people about some hot topics he had been working on. Hank's enthusiasm, curiosity, and his deep love of Mother Nature were always the same, even before he took the last journey to the hospital. ❖

*Hank's
enthusiasm,
curiosity, and
his deep love
of Mother
Nature were
always the
same....*

The Viper's Jaws

by Starbuck

The narrators of those sentimental nature documentaries are fond of reassuring us that the field mouse in the viper's jaws feels no pain—that it is in a state of shock. But I have seen men die. The poet W.B. Yeats, in his poignant "Sailing to Byzantium," portrays the death of old men more truly: how the soul, sick with desire and fastened to a dying animal, knows not what it is.

The operation is over. The nurses tend you with gentle hands. The surgeon describes what he has found, but his moist, compassionate eyes tell you that there are limits to his skill. Your old friend, the family physician, reassures you that you will not suffer pain and sadly averts his face. Bewildered family members gingerly enter the sterile room, awkwardly attempt to express their love and hastily depart, with sorrow and relief, to the sun and fresh air in the parking lot. And you are left to face the unknown terror of the coming night alone.

As Mark Twain grew older, and others in his generation began to pass away, he asserted that though others died, perhaps in his case there might be an exception. An active mind stays young, though chained to a dying animal body. We have been given the precious gift of life, a chance to contemplate a surpassingly beautiful universe, minds in which to cultivate some measure of wisdom and the companionship of other creatures no less mortal than we.

If in the midst of an often crass and strident society, we have learned to love this world, if we have managed to control our avarice and learned to give rather than to take, and above all to give ourselves to fellow human beings, then we may discover how, with grace, to give ourselves to death. ❖

The Enterprise
July 3, 1985

Harmon Craig

Scripps Institution of Oceanography



28 August 1988

President's Committee on the
National Medal of Science
National Science Foundation
Washington, DC 20550

Gentlemen:

I am writing to second the nomination of Henry Stommel for the National Medal of Science. Although nominations like this one generally deal in hyperbole, there is no way that one could overexaggerate the role that this amazing scientist has played in the development of dynamical oceanography, to say nothing of meteorology, in which he has also had a strong hand. Henry Stommel is the complete scientist, naturalist, and sailor, with an eye to every interesting problem and observation that comes along. One of the best examples of this wide-ranging perception of new and interesting developments has been his interest in welding together the tracer geochemistry people and the physical oceanographers for a total look at oceanic circulation and mixing. One result of Henry Stommel's interest in this area was the GEOSECS (Geochemical Ocean Sections Study) Program, which was overwhelmingly considered the best of the NSF-sponsored IDOE (International Decade of Ocean Exploration) programs, and is the model for the present WOCE (World Ocean Circulation Experiment) initiative, which will expand on and continue the GEOSECS studies during the next decade.

One very instructive aspect of Stommel's interactions with areas such as the tracer studies has been his continued interest in the meaning of these data. Thus while the majority of physical oceanographers continue to ignore the discovery of the large zonally spreading plumes of helium-3 in the Pacific (or, in some cases, to wish that they would disappear!), Stommel was totally delighted that they did not fit into his earlier model of geostrophic circulation of the deep and bottom water.

Indeed, he has written a provocative and inspiring paper actually proposing that the deep hydrothermal vents on the East Pacific Rise may actually *drive the deep circulation* of the Pacific. This ability to couple new observations and a deep appreciation of the possible physical effects of such phenomena are Stommel's trademark. While the plume paper is by no means his magnum opus, it stands out to me in the sense that one could read this paper without knowing the author, and immediately identify it as a Stommel paper. "By his claw the lion" is as applicable to Henry Stommel as it was to the original scientific carnivore who feasted on natural phenomena!

Stommel once explained in his typical half-apologetic way that he is like a circus billboard painter, who comes to town before the big performance, posts all the signs and gets everyone's attention on the show, and then when the circus arrives, he has quietly and peacefully moved

Henry Stommel is the complete scientist, naturalist, and sailor, with an eye to every interesting problem and observation.

on to the next town, and the next carnival. It is literally true that there are one-, two-, and three-ring performances going on all over the world that have been ushered in by Stommel's inspiring leadership. Many of these are listed in the narrative statement describing his work, and do not need further amplification here. What *should* be emphasized is the strongly personal nature of his leadership in oceanography, and his function as a centripetal and seminal role in holding together such a diverse and wide-ranging field, in both the exploration and theoretical development of the subject.

It is rare that one can find a major field of science so dominated by a single person and by those who have worked with him at sea and ashore. I am particularly struck with the way Stommel blends expedition work at sea with the insight and perceptiveness that mark his work in theoretical developments and in the laboratory (e.g., the early rotating dishpan experiments, a typical Stommelian tour de force). It is not often given to individual scientists to be almost single-handedly the creator, developer, and expositor of a major field of science, and it is important that we recognize the extraordinary contributions that such innovators have made.

Henry Stommel has been the "Cook and the Captain bold, and the Mate of the Nancy Brig" in this field, and the coupling of the "Captain" and the "Cook" in this citation is as apt as it is felicitous and appropriate.

Oceanography has been fortunate among the sciences to have Stommel (who might have been a cloud

physicist, or even a paleobotanist, for example) as the leader who, in the post-war period, more than any other person, established the entire field as a major scientific field of substance, importance, and, in addition, just generally an endeavor of good scholarship, good fun, and good fellowship, as exemplified in all cases by Henry Stommel himself. The National Medal of Science could not be awarded to a worthier scientist, or to one who has contributed more at all levels and in every area within a major field of scholarship.

Sincerely,
Harmon Craig
Professor of Geochemistry



Henry Stommel receives the National Medal of Science from President George Bush.

Sixtieth Birthday Party



Michèle Fleux

On September 28, 1980, friends and colleagues from around the world gathered at the Nimrod Club in Falmouth, Massachusetts, to celebrate Henry Stommel's 60th birthday. The photo above shows Hank arriving at the Nimrod. Matt Stommel is in the doorway, and Chickie Stommel is at right.



Vicky Cullen

Around the birthday table, from left, are Koji Hidaka, representing his Japanese colleagues, Hank, Marjory Wunsch, Carl Wunsch, Jule Charney, and Chickie Stommel. Countries represented included the US, Canada, England, France, Germany, Sweden, and Australia. Dennis Moore brought the leis from Hawaii.

Henry Melson Stommel

September 27, 1920–January 17, 1992

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

Instructor in Mathematics and Astronomy, 1942 to 1944, Yale University
Research Associate, 1944 to 1960; Physical Oceanographer
(non-resident), 1960 to 1978; Senior Scientist, 1978 to 1992,
Woods Hole Oceanographic Institution
Professor of Oceanography, 1960 to 1963, Harvard University
Guest Lecturer, 1969 to 1970, Laboratoire d'Océanographie Physique
du Muséum National d'Histoire Naturelle, Paris, France
Professor of Oceanography, 1963 to 1978, Massachusetts Institute of Technology

Affiliations and Awards

Phi Beta Kappa, Sigma Xi
Fellow, American Academy of Arts and Sciences, 1959
Member, National Academy of Sciences, 1961
Sverdrup Medalist, American Meteorological Society, 1964
Albatross Award, American Miscellaneous Society, 1966
Fellow, American Geophysical Union, 1972
Henry Bryant Bigelow Medal, Woods Hole Oceanographic Institution, 1974
Foreign Member, Soviet Academy of Sciences, 1976
Maurice Ewing Award, American Geophysical Union, 1977
Rosenstiel Award, American Association for the Advancement of Science, 1978
Alexander Agassiz Medal, National Academy of Sciences, 1979
Huntsman Award, Bedford Institute of Oceanography, 1980
Bowie Award, American Geophysical Union, 1982
Grand Prix d'Océanographie de Monaco, 1982
Membre d'Honneur, Société de Géographie, Paris, 1983
Crafoord Prize, Royal Swedish Academy of Sciences, 1983
Foreign Member, The Royal Society, London, 1983
Foreign Associate, Académie des Sciences de Paris, 1984
Albert Defant Medal, German Meteorological Society, 1986
National Medal of Science (USA), 1989

Research Interests:

General circulation of the ocean, Gulf Stream, Kuroshio, eddy dynamics, equatorial currents, deep-water convection, planetary flow patterns, historical data, climate, Indian Ocean and the monsoons

Author or co-author of 138 scientific papers, 12 books, 40 nonrefereed publications, and 24 technical reports and miscellaneous publications ❖

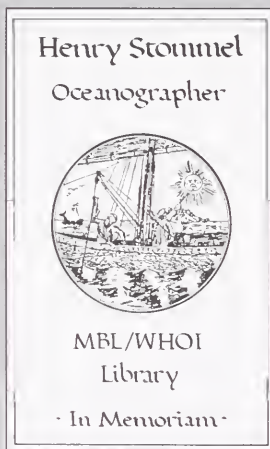
A fund
honoring the memory of
Henry Melson Stommel
has been established
to endow
the atlas collection
of the
MBL/WHOI Library.

Maps were one of Hank Stommel's abiding fascinations. He often sought out cartographers on foreign visits to add to his knowledge and his collection of maps.

His interest in cartography and his appreciation of history came together in his 1984 book, *Lost Islands*, which details the appearance, disappearance, and reappearance of nonexistent islands on various maps.

Contributions to this endowment fund may be addressed to:

The Development Office
Fenno House
Woods Hole Oceanographic Institution
Woods Hole, MA 02543



This bookplate, featuring an image Henry Stommel often used on his own printing press, will be placed in volumes purchased with the endowment fund.

***The Starbuck
Essays of
Henry Stommel***



As the contributions to this volume attest, Hank Stommel was a wonderful human being as well as a major contributor to oceanography. George Veronis wrote that he "understood the human condition and the value of humor in coping with life."

Using the pen name Starbuck, Hank wrote more than 70 essays for the Falmouth, Massachusetts, newspaper *The Enterprise*, mainly in the mid 1980s. Collected in this volume, published by Stommel's friends and colleagues, they are thoughtful considerations of Falmouth and Woods Hole Oceanographic Institution events and personalities—and unique observations on life from an unusually observant man.



Proceeds of the volume's sale will augment the Henry Melson Stommel Fund established to endow the atlas collection of the MBL/WHOI Library.

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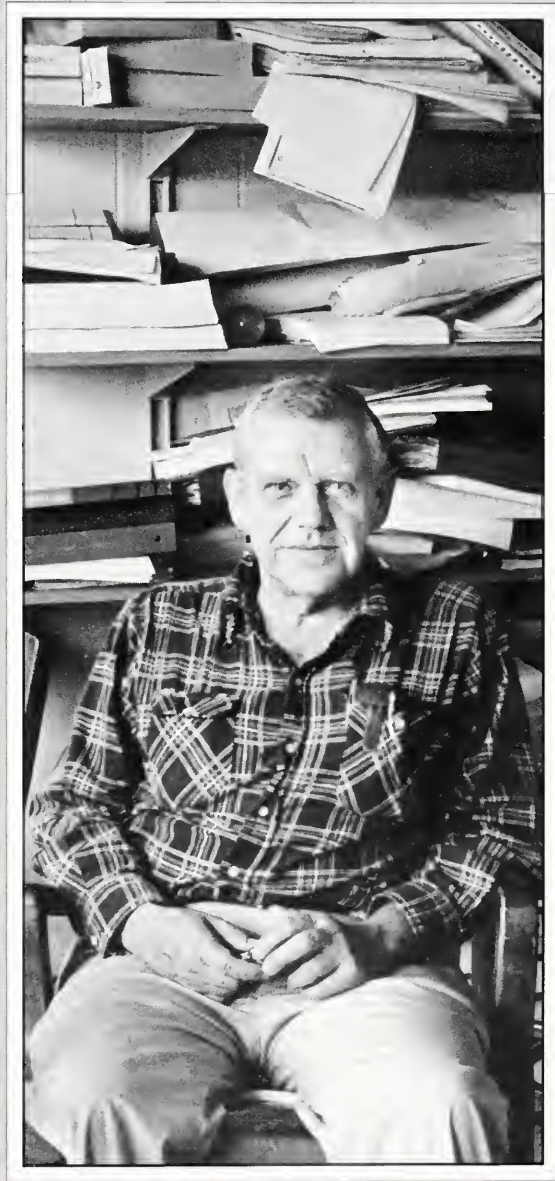
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Vicky Cullen

Henry Stommel in his office in 1979.



Henry Stommel designed this logo for INDEX, a late-1970s program that focused on the monsoonal currents of the northwestern Indian Ocean. The name *La Curieuse* was drawn from a charter vessel Stommel and colleagues used for a time out of the Seychelles. (The real *La Curieuse* was said to be marginally more suited for oceanographic research than the one depicted on the logo.)