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Henry Melson Stommel

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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 Deaconness Hospital in Brookline, Mass. 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 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 was 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 thoughout 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 qualititative 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.

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 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 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.

Carl Rossby, who was then at the University of Chicago, met Hank in Woods Hole just after the end of the war and invited him to visit Chicago in spring, 1946. During those postwar years and even up to 1960 when he was appointed Professor at Harvard, Hank bemoaned his lack of a Ph.D. His visit to Chicago was an exploration of possible graduate study there.

After delivering his westward intensification paper at the AGU meeting in Woods Hole in September, 1947, he left for England on a six-month leave of absence without pay. From his meager salary (he received $1300/year initially) and the royalities from *Science of the Seven Seas* he had saved $1500 to finance the trip. His initial purpose was to visit Southwell and become really familiar with the relaxation method, but the temperaments of the two men were so different, that Hank soon abandoned that idea and spent his time with the group working on meteorology and oceanography at Imperial College in London. His most enjoyable visit was with another pacifist, L. F. Richardson, who had retired to Scotland. The two hit it off immediately, and even though the visit was short, it resulted in a joint note on turbulent diffusion, reporting their observations of the relative motions of pairs of parsnips thrown into Loch Long from a pier.

On his return to WHOI Hank extended his areas of research activity by contributing to a lengthy treatise on the ecology of the Atlantic and another on atmospheric observations over the Caribbean Sea. He then spent several months at Scripps in 1949. It may have been during that trip or sometime earlier that he probed the possibility of graduate study at Scripps. He was not encouraged. He told me not long after we met that he thought that H. U. Sverdrup, the Director of Scripps, disapproved of his watered-down treatment of oceanography in *Science of the Seven Seas.* In any event, after that trip he never seriously considered graduate study again even though he continued to feel uncertified.

In 1950 as Stommel's westward intensification model was gaining recognition, Ray Montgomery moved to Brown, where he occupied an office next to the Applied Mathematics Department. He invited various physical oceanographers, Stommel and Walter Munk among them, to visit and within months Stommel's westward intensification model was reinterpreted by George Carrier in terms of a viscous western boundary layer and an inviscid interior. That separation of the flow into two regions that could be joined caught on quickly and in practically no time Stommel's model was regarded as providing the viscous closure in an enclosed basin of the Sverdrup planetary vorticity balance. That is a logical fluid dynamical interpretation but the Sverdrup relation and Stommel's model were derived completely independently. However, the boundary-layer approach, in particular, and geophysical fluid dynamics, in general, had an enormous impact on the development of circulation theories. Although he retained the original analysis in summarizing his model of ocean circulation in his book, *The Gulf Stream,* published in 1958, Stommel used boundary-layer ideas to discuss other models and developments.

N. P. Fofonoff's 1954 inertial circulation model was extended by Stommel, Charney and George Morgan in terms of an inertial boundary layer appended to an inviscid interior. Although boundary-layer methods made the analysis much easier, a treatment with Fourier series also gave the solution (as Fofonoff demonstrated). But neither Stommel's survey of ocean current theory nor his model for the abyssal circulation of the world ocean would have been so clearly pertinent without boundary-layer ideas for providing closure to simple interior flows. Hank immediately recognized the significance of the separation into interior and boundary layer for the vertically integrated flow. But he also realized how the idea could be applied in the vertical direction so that the different driving mechanisms (wind, thermal, and precipitation and evaporation) could be incorporated into one treatment by specifying the vertical velocity at the top "boundary." It was that idea that made it possible to explore different aspects of the circulation in a familiar framework.

For the abyssal circulation Hank reasoned that turbulent mixing in the upper waters would transport warm water downward and, in order to keep the temperature constant (in a statistical sense) at a given level, he invoked an upward flow of (cold) deep water. Since the two major sources of abyssal water are in the polar seas of the Atlantic, he incorporated them into his model. The abyssal western boundary layer needed to provide mass conservation in the Atlantic required a southward flow beneath the Gulf Stream. This theoretical prediction was verified observationally in the spring of 1957 by John Swallow. It stands as a major triumph for theoretical oceanography, although Hank admitted in private that he wasn't all that convinced that a prediction based on such an idealized model would hold up.

He also realized that the reasoning that led to the model of the abyssal circulation could be checked in the laboratory. In 1957 he, Alan Faller and Arnold Arons verified all of the major features of the theory by testing its predictions in a set of rotating-tank experiments. In a continuing collaboration over the following 15 years he and Arons explored the implications of the theory as it applied to ocean basins on a sphere, to the distributions of chemical tracers in abyssal waters, and to topographic effects on the circulation.

During the 1950's Hank continued to pursue his interests in observational oceanography. He and his family spent the spring of 1953 in Bermuda where he started fortnightly measurements of the deep waters off Bermuda. These *Panulirus* stations have continued over the years and provide one of the few sources of data for documenting climatic change. A few years later after Swallow had developed his neutrally buoyant float for longer-term tracking, he and Stommel tried to measure the creeping motions predicted by Hank's abyssal circulation theory. They obtained the first evidence that the deep ocean has eddy velocities orders of magnitude larger than the slow motions predicted by linear, steady theory.

During that period Hank was also working on a theory of the thermocline. He and I used linear theory to estimate the thermocline depth but the problem is essentially nonlinear. Two years later he and Allan Robinson succeeded in producing a more appropriate model incorporating nonlinear effects. Their paper was published together with an independent study of the same problem by Pierre Welander. In an article with J. Webster in 1962 Hank reexplored the dynamics of the thermocline with a numerical model involving only vertical processes. That same balance forms a part of the definitive treatment of the thermocline produced by Rick Salmon in 1990.

After moving to Harvard in 1960 as Professor of Oceanography and then to MIT in 1963, he resolved his misgivings about not having a Ph.D. by certifying Ph.D. students himself. In 1964 he was awarded an honorary doctorate by Göteborg University in Sweden. Six years later Yale and Chicago followed suit. He continued to sign his name, Henry Stommel, Esq.

His commitment to observations led him to help in planning a survey of the Kuroshio in the Pacific Ocean in the late 1960's. He and K. Yoshida edited a book summarizing the efforts of the different parts of that program. For the Atlantic he proposed a dense network of stations, akin to a meteorological array, over a small (300 km × 300 km) region in the vicinity of Bermuda. The result was the mid-ocean dynamics experiment (MODE) in the mid-1970's which made use of moored current meters, neutrally buoyant floats, hydrographic stations, deep pressure measurements and meteorological observations to provide an unprecedented data base for studying ocean dynamics. In 1968 he corralled a group of geochemists including W. Broecker, H. Craig, D. Simpson, and K. Turekian and motivated them to produce a worldwide set of geochemical sections (GEOSECS) as baseline data for future studies. Essentially, he acted as catalyst for these programs. Once they were functioning, he retired to the wings, letting others run the main show while he remained available as an advisor and counselor.

In looking back on his career he felt that his most rewarding observational effort was the MEDOC 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% 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 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 vorticies and in the past few years he and Nelson Hogg explored the representation of heat flux in terms of point baroclinic vorticies. 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 1980's 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 conceptual 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.

His persistent preoccupation with maps started even before he arrived in Woods Hole and it continued throughout his career. He would often seek out cartographers when he visited foreign cities and ports and add to his store of knowledge and sometimes to his map collection. His appreciation of history was reinforced by his fascination with maps and the two interests came together in his 1984 book, *Lost Islands, the Story of Islands that Have Vanished from Nautical Maps,* in which he recounts the appearance, disappearance and reappearance on maps of non-existent islands. A brief account of these lost islands had been included in *Science of the Seven Seas.*

Because of his desire to have quick access to oceanographic data, he dreamed of having all of the data on a computer together with programs that would give him maximum flexiblity for relating variables. A meeting with Ed Fredkin in 1961 convinced him that his dream could become reality and in 1963, together with M. Pivar, they produced a primitive electronic version with a limited data set. Although Hank supported the idea with vigor, he didn't have the temperament to set up the type of extensive service structure that the grand scheme required. His time was better spent in following the paths suggested by his fertile and innovative imagination.

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 a 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 in 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 $3000). 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 would help him with a detailed calculation to carry the idea further. His generosity in sharing his own original thoughts 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.

The collegiality that Hank fostered grew out of a deeper commitment that he felt for his fellow human beings. In September 1984 the Falmouth Enterprise started publishing a series of essays that he had written under the pseudonym, Starbuck. One of them, "The Viper's Jaws," was read at his memorial service on 22 January, 1992, attended by some 400 friends and colleagues who had come from all over the world. In it he lamented the fact that an active, young mind should be chained to a dying animal body. He ended the essay with words that describe the philosophy of the man who was our friend:

"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."

George Veronis