Henry Stommel

The Scientific Work of Henry Stommel

Arnold B. Arons

Tanquam ex ungue leonem.
Jean Bernoulli, 1697

Henry Stommel graduated from Yale University in 1942 with a major in astronomy and remained at Yale as an instructor in mathematics and astronomy through the wartime years of 1942–1944. In 1944 he took a job with Maurice Ewing, whose group was then located at the Woods Hole Oceanographic Institution. The job, however, initially did not involve full-time residence in Woods Hole. A Coast and Geodetic Survey chart of the Mississippi Delta can still draw Stommel into reminiscences about a period of instrument monitoring at an isolated station in the far reaches of the delta, finger pointing to the location of the remote outpost.

On taking up more continuous residence at Woods Hole, Stommel became one of a group of bachelors who formed a kind of informal fraternity residing in the old rectory of the Episcopal church on the east corner of Church Street and Woods Hole Road. All the inmates were members of the Institution staff, and to describe some of them as "characters" would be a form of gentle British understatement. The atmosphere of the rectory, however, was stimulating and congenial and constitutes another source of Stommel's anecdotes and reminiscences. Some of the more senior individuals of this group were quick to grasp the quality of their new young member's intellect. I recall two of them, at that early date, predicting his future leadership in research.

Toward the end of World War II, Stommel, becoming interested in the oceans, educated himself in basic oceanography. A by-product of this study was the charming little book *Science of the Seven Seas,* published in 1945. There followed a period of casting about for lines of research: a note on use of the *T–S* correlation in dynamic height anomaly computations, an exploration of the theory of convection cells, a sally into cloud physics. At one point in this interval he consulted Ray Montgomery about outstanding problems in oceanography, and Montgomery pointed to the dynamically unexplained phenomenon of the Gulf Stream.

In 1948 the *Transactions of the American Geophysical Union* carried a short paper entitled, "The Westward Intensification of Wind-Driven Ocean Currents." This was to become a classic, one of the most frequently cited papers in modern physical oceanography. Some years later I once heard Hans Panofsky say, "That paper? Oh, that's the paper which made Henry Stommel famous!" In an elegantly simple model—a plane, rectangular, homogeneous ocean driven by wind torque at the surface and braked by bottom friction—Stommel showed that the basic dynamical equations predicted a flow symmetrical about the central meridian if the Coriolis term is held constant over the plane but that a westward intensification (as in the Gulf Stream) emerges if the Coriolis term varies linearly with latitude (the so-called β-effect).

In this day, when the fundamental equations of geophysical hydrodynamics, including variation of the Coriolis parameter, are casually written down in text and lecture presentations for each new generation of students, it is probably difficult for many to grasp the research atmosphere of a time when the dynamical significance of variation of the Coriolis parameter had not yet been fully appreciated by either oceanographers or meteorologists. Though it all seems so obvious and compelling in retrospect, it is well to note that Bjerknes, Ekman, Defant, Sverdrup, and Rossby had not perceived the connection with westward intensification in a bounded basin; it was the youthful Henry Stommel who did. The 1948 paper coincided with the beginning of a new epoch of research in physical oceanography and constitutes a prototypical example of the Stommel style.

Reference to style calls for explanation of the Latin quotation at the head of this essay. In 1696 Leibniz and Jean Bernoulli, striving to demonstrate the power and significance of the new mathematical methods of "analysis" (the differential and integral calculus) as opposed to the ancient methods of "synthesis" (geometry), posed as a challenge to European mathematicians the now well-known brachistochrone problem.* They knew that this problem could be solved only by use of the new analytic methods, and they speculated that L'Hôpital, James Bernoulli, and Isaac Newton would be among the few likely to meet the challenge. When, early in 1697, Jean Bernoulli saw the correct and powerful solution published anonymously by Newton in *Philosophical Transactions of the Royal Society*, he is said to have remarked, "Tanquam ex ungue leonem"—literally translated, "As from the claw, the lion"; freely translated, "You can tell the lion by his claw."

This is a very fitting metaphor; in Henry Stommel's papers you can almost invariably tell the lion by his claw. He is diffident, almost apologetic, for what he regards as his "limited mathematical capacity" in dealing with the complexity of oceanographic problems, yet in this "limitation" perhaps lies much of his strength. With consummate artistry he constructs a model having just the right idealizations to make it tractable and just the right physical content to make it illuminating; then with the simplest mathematical methods he extracts the deep and significant physical insights that hitherto had not been attained.

At this point the most expert applied mathematicians take over, usually with Stommel's active encouragement and cooperation, and proceed to extend and refine the original picture. So it went with the westward intensification: Munk worked out a more sophisticated model with its multiplicity of "gyres" and with dissipation provided by a horizontal austausch coefficient rather than by bottom friction; Morgan and Charney, in continual personal contact with Stommel, examined the nonlinear aspects; Munk and Carrier worked out solutions for nonrectangular basins. But the deep physical insight opening up the entire field was in the 1948 paper. This paper, as well as the syntheses contained in the 1957 "Survey of Ocean Current Theory" and the 1958 book *The Gulf Stream*, continue to be deeply influential and widely cited.

Stommel has a way of looking at new papers published by others, frequently imposingly complex and difficult to penetrate, and, after a relatively short study, stripping away the complexity, revealing the essence of the paper in something of the form and style that he himself might have used had he formulated the problem *ab initio*. His unerring penetration of the essential physical content is steadily guided by his deep, reliable intuition for every aspect of fluid flow.

During a period in which we were deeply immersed in thinking about Rossby waves, long after Rossby's classic paper of 1939, I recall a moment at which Stommel emerged from the library where he had been reading Laplace. In a characteristically bubbly way he said, "You know, Laplace's tidal motions of the second class are simply Rossby waves; I hadn't realized this till now." Although we were aware of Haurwitz's 1940 paper in *Journal of Marine Research*, in which he had examined the formalism of Rossby waves in spherical coordinates and pointed out their identity with "motions of the second class" discussed by Margules, we were not fully sensitive to the latter comment. Stommel's insight quite independently penetrated the physical content of the arcane Laplace formulation.

His depth of intuition sustains another characteristic that I have frequently seen at play—an almost inarticulate, unswerving sense of when he is on the right track with some physical idea. When he has this sense, he will not be deflected, and he will not take his teeth out of a problem. Others will give up and fall by the wayside, but he persists until the initial hunch is brought to fruition.

In addition to his profound grasp of dynamics, Stommel has a broad descriptive knowledge of oceanic data and phenomena. If, however, he does not happen to have something you ask him about at his finger tips, he will vanish into the recesses of the library and

* To find the curve connecting two points, at different heights and not in the same vertical line, along which a body acted upon only by gravity will fall in the shortest time.

emerge a little while later with the crucial material in hand; he knows exactly where it is located.

The range of Stommel's more than 100 publications embraces not only almost all aspects of physical oceanography (both theoretical and observational) but also extends into cloud physics, limnology, and estuarine circulation. There are observational and theoretical papers on oceanic and limnological thermoclines, on time-series observations of thermal "unrest," on the formation and sinking of water cold enough to drop to the bottom, on oceanic Rossby waves, on monsoon effects in the Indian Ocean, on tidal mixing and density currents in estuaries, on the Kuroshio, on dynamical transients in the ocean—all containing some illuminating physical insight or significant observational data.

One characteristic of his work has been a steadfast concern for the most basic problems of interpretation of hydrographic data. He repeatedly returns, with increasing depth of insight, to matters such as the origin and significance of the T–S correlation, the problem of the depth of no motion, conservation of potential vorticity, formation of the main thermocline. Most recently his publications have dealt with the *β-spiral*—another effort to extract the absolute velocity field from hydrographic data.

As early as 1958, I heard Albert Defant, on a visit to Stockholm, characterize Stommel as the world's leading physical oceanographer, but during the 1950s Stommel, apparently diffident about not possessing the golden academic key, briefly toyed with the idea of going back to graduate school to earn a Ph.D. I was undoubtedly not the only one who was skeptical of this notion. When he mentioned it to me, I remarked that it was clearly far more appropriate for him to be directing the theses of others rather than pausing under someone else's direction.

Stommel's outside interests go through phases that range from the collection, repair, and playing of harmoniums and music boxes, to tracking down the literature on Harrison's invention of the chronometer and his winning of the Queen Anne prize, or to (a recent effort) tracking down the meteorological facts on the famous 1816 "year without a summer." At one time during the 1950s there was a printing-press phase, and the Institution suddenly blossomed with puckish printed notices, announcements, and invitations.

The printing-press phase, however, was characterized by another kind of output—a set of useful pamphlets, one, for example, detailing the hydrodynamical equations in finite-difference form. Another was titled "Why Do Our Ideas about the Ocean Circulation Have Such a Peculiarly Dream-Like Quality?" In the title alone you can tell the lion by his claw. Although never published other than by the Stommel underground press, this was a seminal paper. A penetrating, incisive critique of the status of ocean-current theory, it made the rounds of the active theoreticians and deeply influenced the direction of their thinking. I recall it being referred to repeatedly in seminars and colloquia of that period.

During the early 1950s, while engaging in a wide variety of studies ranging from estuarine dynamics, through the monitoring of a long series of temperature measurements on the bottom off Bermuda, to the use of submarine cables in measuring potential differences across oceanic currents, Stommel began to talk more and more explicitly about the need for time-series observations of pressure, temperature, and current in the deep ocean. He recognized the possibilities offered by moored-instrument strings and recoverable-instrument packages and envisaged the acquisition of synoptic data from such arrays. These ideas were, of course, "in the air" at that time, catalyzed by revolutionary progress in electronic instrumentation, but applications to oceanography were still almost nonexistent. It was with this need in mind that we brought David Frantz and then William S. Richardson to Woods Hole, and got them started on the buoy work that subsequently was massively extended and powerfully implemented by Nick Fofonoff and Ferris Webster.

During the mid-1950s, Stommel's imagination was strongly captivated by John Swallow's development of the neutrally buoyant float for the tracking of deep currents. Following Swallow's early observations off the Straits of Gibraltar, Stommel participated in organizing the *Aries* work off Bermuda in which Swallow and Crease provided some of the earliest indications of the now widely recognized deep oceanic fluctuations.

In 1956–1957 Stommel began thinking seriously about the abyssal circulation. Recognizing that geostrophic flow in the ocean basin, subject to planetary divergence (the β-effect), could not have an equatorward meridional component without an accompanying *downward* vertical velocity, and feeling compelled to reject a downward velocity in the abyssal layer, he reached the conclusion that little cold abyssal water from high latitudes would be able to enter the interior of an ocean basin directly. On this basis he predicted the existence of a deep equatorward boundary current along the western side of the ocean, transporting dense water from high to lower latitudes. The prediction was verified for the North Atlantic by the joint *Atlantis–Discovery II* expedition in the spring of 1957, an expedition in which the Swallow floats also played a key role.

Rather than being the more usual a posteriori rationalization of an observed phenomenon, this was one of the very first a priori predictions of a hitherto unobserved, major feature of oceanic circulation—a prediction derived in characteristic style from a deep understanding of geophysical fluid dynamics.

These insights motivated Alan Faller's beautiful rotating-tank experiments on stationary planetary flow patterns in bounded basins, exploring flows arising in the presence and absence of vertical velocities imposed on the basin by a distribution of sources and sinks. Faller compellingly demonstrated the refusal of the system to accommodate a meridional flow component in the interior geostrophic regime under conditions of zero vertical velocity. He demonstrated the intense southward western boundary current arising when a northern source imposed a vertically upward velocity (with its associated northward geostrophic flow in the interior) on the entire basin.

I vividly remember the morning on which Stommel stormed into the office and said, "There is more water being transported northward than is rising vertically." He was referring to the experiment with the northern source and the uniformly distributed upward velocity. In characteristic style, he had done what no one else had thought of doing. He had taken a careful look at the *continuity* of the flow in the bounded basin. The simplest kind of algebra reveals that, at any vertical zonal section, the horizontal poleward transport of the meridional geostrophic flow through the section is larger than the total vertical transport in the region north of the given section. The extra horizontal flow simply *had* to be recirculated in the southbound western boundary current. Faller quickly confirmed the hitherto unnoticed recirculation while Stommel and I showed that analogous conditions carried over to a spherical ocean and that recirculation was to be anticipated in real abyssal western boundary currents.

On another occasion, Stommel and I were engaged in one of our frequent discussions of how one might measure pressure variations at the bottom of the deep ocean over long time periods to an accuracy of a few centimeters of water. (Others finally did this more or less successfully; we did not.) In desperation we were considering the brute-force technique of making a 3-mile-long manometer, i.e., literally extending a tube from the ocean surface to the bottom and drawing abyssal water up into the tube. Since the salinity of the abyssal water is invariably lower than the average salinity of the column above it, the water in the tube, on coming to thermal equilibrium with its surroundings, would stand above the level of the surrounding ocean surface, and we could watch the level in the tube go up and down with variations of pressure at the bottom. We had this picture sketched on the chalkboard and were entirely focused on the manometric aspect when my own mind took a divergent turn. In some astonishment, I added a faucet to the upper level of our manometer and said, "Hank, if we open the faucet, it will run forever."

After we satisfied ourselves concerning the nature and temporal limitations of the physical phenomenon, Stommel ran down to Duncan Blanchard's laboratory and recruited this ready and skillful gadgeteer to our party. Blanchard quickly set up a large beaker with a layer of hot salty water floating on cold fresh, and we blissfully watched the little fountain that spurted for a long time out of the glass tube in which the cold fresh water had been drawn upward to start the sequence.

This was the genesis of our oft-cited short letter to *Deep-Sea Research* describing the "perpetual" salt fountain. We recognized that the key lay in blocking salt transfer while allowing thermal equilibrium; we recognized that if surface water were initially drawn downward in the tube, there would be a steady downward flow; but we did not perceive a deeper significance. We quickly convinced ourselves that this would not be a practicable way of inducing significant rates of upwelling of nutrient-rich water, and we dropped the subject.

Not long afterward, Melvin Stern, in his quite independent investigation of the stability problem, became aware of the dynamic significance of the huge difference between the molecular diffusion coefficients of heat and salt and thus discovered double-diffusive convection and "salt fingers." I believe that Stommel was probably a bit chagrined about having missed this himself, but he was strongly supportive of Stern's priority for the discovery and unstinting in his praise and enthusiasm.

In the third paper of our series on the abyssal oceanic circulation, Stommel and I were able to look at the distribution of oxygen and radiocarbon in the North Atlantic in a somewhat more sophisticated manner than that afforded by conventional "box models." We used the distribution of the chemical properties as an index to the dynamics. After completion of this paper, in the summer of 1966, Stommel assembled some of the leading ocean chemists who happened to be attending a National Academy of Sciences conference at Woods Hole and fired their interest and enthusiasm with a glimpse of the impact that modern, accurate, simultaneously made chemical measurements might have on all of physical, as well as chemical, oceanography. This seed, with subsequent watering by many other individuals, evolved into the Geochemical Ocean Sections Studies (GEOSECS), which was incorporated into the activities of the International Decade of Ocean Exploration (IDOE) and which is still bearing fruit throughout oceanographic science. In an even more direct fashion, Stommel catalyzed and helped sustain the Mid-Ocean Dynamics Experiment (MODE) and POLYMODE programs. During the 1960s, as recovery of buoyed instrumentation became increasingly reliable and as SOFAR float techniques looked promising, many individuals began to discuss large-scale observational efforts in the deep ocean. A principal objective was to describe in more complete detail the scale and periodicity of the eddy motions initially suggested by the results of the *Aries* expedition and imperfectly delineated by the buoy data acquired in subsequent years. Stommel was a participant, either in person or through radiated influence, in most of these discussions. When the MODE experiment was being organized in 1970, Stommel became cochairman (with Allan Robinson) of the steering committee.

"It is fair to say," Carl Wunsch, one of the principal participants, writes in 1978, "that it was Hank's presence and huge enthusiasm which allowed us to entrain a remarkable number of people. . . . The MODE experiment is notable in Hank's career in that he has never handed it off to others the way he has with most of the other major programs he was instrumental in launching. He is still officially the co-chairman of POLYMODE, eight years after it all began."

In essentially similar ways, Stommel's influence extended to work in the Kuroshio and to some of the Indian Ocean programs of the last decade. Throughout this continuing association with the genesis and fruition of large and *complicated* projects, Stommel has exhibited a pronounced talent for avoiding *complex* ones. (Complex projects, of course, are those for which costs are real and results imaginary.)

Most fine scientists almost automatically collect a cloud of individuals around them, forming something of a "school" of research. Stommel is no exception; the cloud collects wherever he is located. His prodigality of ideas is so vast that he cannot deal with all of them himself, and he hands them out right and left to other individuals. All who have worked in Stommel's vicinity are familiar with the explosive laughter that frequently reverberates in the course of conversations in his office. His own sense of humor (gentle and never at the expense of someone's feelings), his ebullience, his enjoyment of intellectual activity are infectious and pervade the atmosphere that surrounds him.

Discussions with him are invariably a chase and a challenge. He can be irascible with slow-wittedness. His quickness and penetration are such that he frequently leaves the verbal train of thought behind, but he makes things so interesting that individuals are attracted into an area of investigation largely because of the color and fascination he has infused.

When during the 1960s the wags that formed the American Miscellaneous Society used to make their annual Albatross Award, they would nominate a truly leading oceanographer and accord him a citation with irreverent and irrelevant content. In the citation for the 1966 Albatross Award, Stommel was twitted for his propensity toward "abandoning oceanography's most cherished chairs." This was an oblique reference to the sequence of changes of position that he had been making. In 1960, unhappy with some aspects of the administration at Woods Hole and attracted by a tempting academic offer, Stommel left the Oceanographic Institution for a professorship at Harvard. Unhappy at Harvard, he moved in 1963 to the Department of Meteorology at MIT; Cambridge and Lexington turned out to have little attraction as places of residence, and the Stommel family moved back to Sippewissett, eventually acquiring Sippewissett Farm, where one could ride a tractor, till a huge garden, and even raise sheep. During the interval, Stommel spent some of his time at the Oceanographic Institution, commuting to Cambridge to discharge his obligations at MIT In 1978 he returned to a full-time position as Senior Scientist in Woods Hole, confirming the fundamental indestructability of his ties to the institution at which he began his scientific career.

No contributor to this volume is without some debt to Henry Stommel, whether it be in the way of an important scientific insight, a suggestion for fruitful activity, some kind of generous assistance, or some token of personal friendship. I am sure I speak for the contributors, as well as for the oceanographic constituency at large, in saluting him and his distinguished career with pleasure, respect, and enthusiasm on his sixtieth birthday.

Arnold B. Arons

A Theoretical Model of Henry Stommel

George Veronis

From a theoretician's viewpoint, the most significant contribution that Henry Stommel has made to oceanography is his development of simple physical models to demonstrate important processes in oceanic flows. His mastery of that approach, his innovative ideas, the generous sharing of his thoughts and his work, and his open-minded appreciation of new ideas by others are characteristics that have earned him wide recognition and many honors in oceanography. Yet he has always appreciated the awesome scope of science, and in that context he has retained a modest view of his own contributions.*

When he graduated from Yale near the beginning of World War II, Stommel planned to attend divinity school. What made him change his mind is not certain—perhaps he realized that there would be little opportunity to develop simple models in that area and even less likelihood that he could test his ideas against data. In any event, he dropped his sights from the spiritual to the celestial sphere when he entered the Yale Astronomy Department as a graduate student. He found little attraction in celestial mechanics, the main area of research in that department, and in 1944 at the suggestion of L. Spitzer he applied for a position at the Woods Hole Oceanographic Institution. Stommel was a conscientious objector to war, a position that was extremely difficult to maintain during World War II. Working at WHOI was considered an "acceptable" substitute for military service.

In 1945 he produced his first work, *Science of the Seven Seas,* a popular book on oceanography. The book was written in the evening during a 4-week period. Though the book was a modest financial success,† it was a mixed blessing. When he applied to Scripps for graduate school, he was turned down. He thinks this happened because H. U. Sverdrup was annoyed with him for writing a nonprofessional book on oceanography.

Intent on developing his scientific talents, Stommel bought Southwell's book on relaxation methods just after it appeared in 1946. He planned to apply the method to the solution of a system of elliptic equations that he had formulated for the tides. At about that time, R. B. Montgomery had mentioned to him the east-west asymmetry in the response of the ocean to a symmetric wind stress and Stommel proceeded to formulate that problem in terms of an elliptic partial differential equation. In solving it by relaxation, he noted that the β-term led to westward intensification. He realized the significance of that result and reformulated the problem in simpler form, one amenable to analytic solution. That was the first of his many simple and informative models of oceanic phenomena. It also influenced him strongly in realizing the significance of isolating the important physics in the simplest possible context.

His westward intensification paper was submitted to the American Geophysical Union in 1947 before Sverdrup published the relation between meridional transport and wind-stress curl. Today we look upon these papers as a logical sequence, with the Sverdrup transport relation providing a solution for the interior of the ocean and Stommel's frictional boundary layer closing the flow on the western side, but they were quite independent contributions. It was not until 1950 when G. F. Carrier, in collaboration with W. H. Munk, introduced boundary-layer methods to the circulation problem that the connection became clear and Stommel's model was fully appreciated. Initially the main reaction to it was that it was an interesting curiosity—shallow, homogeneous oceans with friction acting through a bottom drag had little to do with the "real" ocean.

During that period, Stommel was living in Woods Hole in the building known as the Rectory, a kind of rooming house shared by several bachelors.* His annual salary was $1,300. Out of that and the royalties on his book, he had saved $1,500. In early fall 1947, he obtained a leave of absence without salary from WHOI and used his savings to sail to England to spend the next half-year at Imperial College. In England he met Sheppard, Brunt, Francis, Deacon, Swallow, Charnock, Longuet-Higgins, and others who were active in meteorology and oceanography. He spent some time with Southwell learning about numerical relaxation procedures. He also journeyed to Scotland to meet and to collaborate with L. F. Richardson on an experiment to study turbulent eddy diffusion by measuring the separation with time of pairs of parsnip pieces thrown from a pier into a lake. He shared an interest with M. S. Longuet-Higgins in determining the electric field induced by ocean currents, but the two followed in-

* Hank has long been a voracious reader (sometimes three books in one evening) and the incredibly broad knowledge that he has acquired provided him with a realistic perspective on man. He told me once that he had learned to read fast at an early age because an optometrist had given him an incorrect prescription—the eyeglasses caused headaches if he read for any length of time—and he developed speed to get through books before a headache set in. He eventually prescribed enlarging spherical lenses for himself and got rid of the headaches, but his speed reading has remained.

† Two decades later Hank remarked to me that he had earned more money from that book (~$5,000) than from *The Gulf Stream,* a book that summarized his scientific efforts of 10 years.

* Hank clowned around a great deal in those days. I saw one of his acts during an evening at his house in the early 1950s. He played the organ and, with a tremulous voice, sang "Baby Hands," a very emotional funeral hymn that was sometimes sung at burial services for an infant.

dependent paths and did not collaborate on the topic until some years later.

A little over a year after returning to WHOI, Stommel bought a used 1946 Ford and drove to Scripps, where he spent the summer of 1949.* In the late 1940s and early 1950s, he continued to expand his interests in natural phenomena, and he collaborated with an increasingly diverse group of people† on topics including ecology, estuary circulation, hydraulic flows, and dynamic effects of rotation. As he developed his skills in constructing theoretical models, he familiarized himself with observations by working up charts of distributions of properties. The marriage of theory to data has characterized his work ever since.

In 1950, he began a collaboration (marriage) with Elizabeth Brown,‡ which culminated in two sons, a daughter, and most recently, a forthcoming book, *The Year without a Summer: 1816*, written with his wife. His enthusiastic concern for oceanography has never distracted him from an equally intense involvement with his family. He made most of his children's toys and the range was impressive. I remember an endless variety of tools made of wood, a 14-foot-high swing, and a one-passenger railway system around his property with the rails made of 2 × 4's and a locomotive powered by a lawn-mower engine.

The summer of 1953 was spent in Bermuda and Stommel returned there in 1954 to initiate the biweekly *Panulirus* hydrographic stations, which continue to the present time to provide an invaluable, nearly continuous record of "deep" ocean data.

As the first decade of his work in oceanography came to a close, Stommel began his synthesis of ideas and observations of ocean circulation and the Gulf Stream. When he visited the Institute for Advanced Study in Princeton in 1955 for a 3-month period, he had in hand a first-draft, typewritten manuscript of *The Gulf Stream.** He had already started to put together his thoughts about the thermohaline circulation, a topic that claimed a good part of his time over the next decade. He was interested in the adjustment problem and during that visit, he and I collaborated on a paper that described the response of a two-layer, β-plane ocean to a variable wind stress.

Gnawing away at him at this time were the problems associated with an inertially generated Gulf Stream. A year earlier, N. P. Fofonoff had published his paper on steady frictionless flows showing the formation of an inertial western boundary current, and Stommel was trying to construct a forced model with the same dynamical control. Actually, he already had a solution for a model with constant potential vorticity, which was to appear in his book. In retrospect, it is clear that the essential physical processes were contained in these two simple models, but the ideas were new and the implications of the simpler models were not fully appreciated at the time. In any event, Stommel had discussed the problem with G. W. Morgan at Brown, and, while at Princeton, he outlined his thoughts to J. G. Charney. Shortly afterward, Morgan was invited to Princeton for a few days and a long discussion of the problem took place. Charney and Morgan became caught up in a fierce competition to outdo each other, and they eventually published separate papers containing essentially the same analysis of the problem. Stommel's input was lost sight of during that frenzied activity, and it reemerged only in the acknowledgements of the two papers.

A year later, he initiated the research into double-diffusive phenomena in a joint publication with A. B. Arons and D. Blanchard on the perpetual salt fountain, a phenomenon in which the salinity distribution in the thermocline (a destabilizing contribution to the density stratification) drives vertical motions. Sometime after that he tried without much success to demonstrate the principle using a pipe during a cruise near Bermuda. It was not until 1960 when M. E. Stern brought out his paper on salt fingers that it became evident that the process might be significant in the ocean without mechanical aids. What none of us knew at the time was that the original salt-finger experiment, with a proper explanation, had been reported a century before by D. Jevons (1857). In 1964, Stommel planted a second seed in the field of double-diffusive convection when he and J. S. Turner published a paper demonstrating the for-

* The speedometer of the Ford, with 59,000 miles on it, stopped functioning during that trip but Hank continued to drive it at ever decreasing speeds for another decade. He became known as the slowest and most careful driver in Woods Hole. It turned out, however, that his driving habits had been only temporarily conditioned by the reliability of that car, as we soon learned when he bought a more reliable one in 1960.

† As one can see from Hank's list of publications, his collaborators have ranged across the spectrum. He has more often provided the basic idea but he also acts as a catalyst for others' ideas. But whether the final publications are routine reports of data or new and exciting ideas, they all have been spiced with his ringing laughter, sometimes triggered by a humorous incident and sometimes by an unexpected development.

‡ At the beginning of his marriage Hank evidently decided to eat lunch at home with his wife whenever it was possible. In earlier years at about 11:50 A.M. each day an internal alarm must have been triggered and he would rush off for home irrespective of what he had been involved in moments before. Lately his lunchtime departures have been somewhat smoother but no less determined.

* J. G. Charney, who has been known to forget things now and then, borrowed the manuscript to read on the train during a 3-day trip. Hank aged 3 years during those 3 days, wondering whether the only copy of his manuscript would end up in some Lost and Found office of a disorganized railway system.

mation of layers when a fluid is stabilized by salinity and destabilized by temperature. Those brief forays into thermohaline convection provide an example of another of Stommel's characteristics. Having collaborated with an effective researcher in a particular subject, he withdraws from further active participation, assured that the development of the area is in competent hands.

The period 1955–1960 was an extremely prolific one, during which his own research embraced an ever expanding set of ideas. Noting that turbulent processes in the stably stratified upper ocean would cause a downward flux of heat, he assumed a deep upwelling of cold water to keep the temperature constant at a given level. The resultant circulation patterns of the abyssal waters had not been anticipated previously and were reported in short notes to *Nature* (1957a) and *Deep-Sea Research* (1958). He also drew attention to the fact that vertical velocities had the same dynamical effect on large-scale circulation whether they were induced by evaporation and precipitation, by thermohaline forcing, or by Ekman pumping. This idea was exploited in his survey on ocean current theory in *Deep-Sea Research* (1957b). In a publication with A. B. Arons and A. J. Faller in 1958, he made use of the same idea to suggest a laboratory analogue of ocean circulation. The first two papers with Arons on the abyssal circulation of the world oceans (1960a,b) provided a quantitative extension of some of his earlier work on the subject.

One consequence of Stommel's inquiries into the deep circulation was his theoretical prediction of a southward-flowing countercurrent under the Gulf Stream. This remarkable result was confirmed in 1957 by tracking neutrally buoyant floats off the coast south of Cape Hatteras (Swallow and Worthington, 1957). It is one of the few purely theoretical predictions of a significant oceanic phenomenon.

Along with these investigations, Stommel was working toward a theory of the thermohaline circulation. He and I published a linear study of the penetration of thermal anomalies from the surface into the deeper regions of a stratified ocean and derived an expression for the thermocline depth. Shortly after that, he and A. R. Robinson derived a similarity solution for a steady, nonlinear thermocline model with geostrophic dynamics and convective–diffusive thermal processes. P. Welander had been working on the same problem independently and produced a solution for an ideal-fluid model of the thermocline. Those two papers were published together in *Tellus* (1959) and formed the basis for a series of investigations into the effects of wind and thermal forcing on the interior of the ocean.

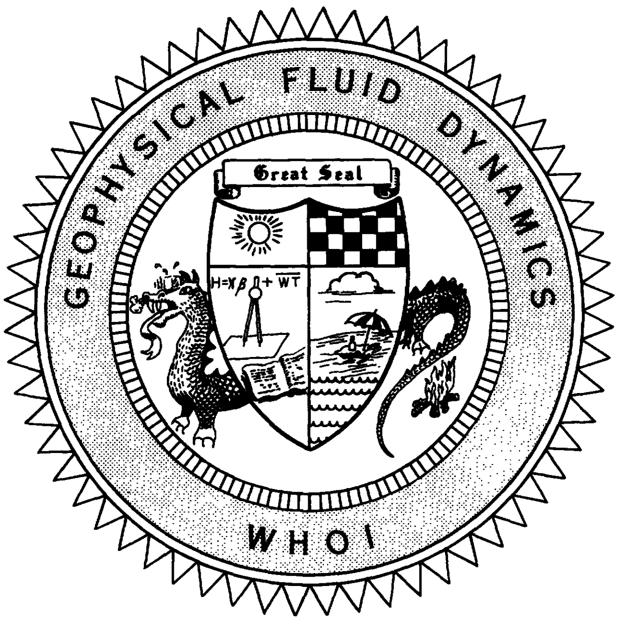
The thermocline model brought together many of the ideas that Stommel had suggested previously for slow, steady flows in the ocean, and to a certain extent, I think that it provides a test of the validity of those ideas. Although the model has inadequacies—e.g., it has never been satisfactorily closed on the west, and if it were, the interior solution would undoubtedly be modified—it contains satisfactory elements for a zero-order description of the oceanic interior. The overall advances leading up to the thermocline model are quite remarkable, especially when one remembers that when Stommel began his studies there were no theoretical models of the oceanic circulation. In any event, the end of the story is not yet in print. Welander has made several important advances in the development of the thermocline model since it was first introduced, and the β-spiral recently demonstrated by Stommel and Schott (1977) is a continuing part of the story.

An important development in the mid-1950s was the establishment of biweekly geophysical fluid dynamics (GFD) seminars between WHOI and MIT. When the numerical forecasting group left the Institute for Advanced Study in summer 1956, Charney and N. A. Phillips moved to MIT and I joined Stommel at WHOI. That fall, we set up the biweekly seminar, held on Fridays alternately at MIT and WHOI. Regular participants included Charney, Phillips, L. N. Howard, C. C. Lin, H.-L. Kuo, E. N. Lorenz, and J. T. Stuart (visiting from England) from MIT. WHOI participants included Stommel, W. Malkus, J. Malkus, A. J. Faller, M. E. Stern, F. Fuglister and W. S. von Arx. On occasion scientists from Harvard, the Geophysical Research Directorate, the Cambridge Air Force Research Center, and A. D. Little took part. One object of the time was to entice promising young scientists from other disciplines into GFD.* After R. Goody, A. Robinson, and Stommel joined the Harvard faculty, the seminar was held cyclically at MIT, Harvard, and WHOI and in later years occasionally at Brown, Yale, and the University of Rhode Island. The entire participating group was rather small during the early period, and nearly everyone attended nearly all the seminars. The after-seminar cocktail parties and dinners were important occasions for the exchange of ideas.†

In 1960, Stommel was elected to the National Academy of Sciences and he also accepted a position as

* For the first few years all notices were printed on Hank's printing press. We used a variety of types and different colored inks for the announcements.

† At one of the early seminars Hank talked on convection driven by a horizontally varying temperature field imposed at the top boundary. Starting from a purely diffusive basic state, he introduced the buoyancy term as a perturbation by expanding in powers of the thermal expansion coefficient. His argument was that the natural expansion parameter was the coefficient of expansion.



The Great Seal of the Summer Study Program in Geophysical Fluid Dynamics at the Woods Hole Oceanographic Institution; an emblem designed by Henry Stommel. The attention devoted to convection in the early years of the summer program inspired the figure of the dragon heated from below and cooled from above.

Professor of Oceanography at Harvard.* The move from a position of pure research to one involving teaching obligations and other academic duties was not a happy one for a man who had never felt comfortable giving a 1-hour seminar, let alone regularly scheduled courses. He never liked lecturing and it is one of the few activities in which he did not excel. Also, by that time Stommel was firmly committed to oceanography and he missed the daily discussions with observational oceanographers and the immediate access to oceanographic data that he had enjoyed at WHOI. In 1963, he left Harvard for MIT, where his position in the (graduate) Meteorology Department involved less emphasis on teaching. At about that time, he decided to maintain his residence near Woods Hole, and shortly afterward he bought a small farm there that he has operated ever since. For 15 years he commuted to Cambridge two or three times a week and spent the remainder of the work week at WHOI. He finally returned to a full-time position at WHOI in 1978.

On the whole, I think that he enjoyed his years at MIT. Certainly he accepted enormous responsibilities in the research effort there. At one point during his period at MIT in the mid-1960s, he was supporting 13 research students on his grants. The normal procedure was that at some time during a student's first year Stommel would suggest an idea by developing a simple model for a particular process or phenomenon, and he would then have the student develop the idea in his own fashion. He was a willing listener when a student had something to report but he never babied the students along.*

His interest in simple modeling of oceanic phenomena and in mapping of distributions of physical and chemical properties continued during the 1960s with the series of papers on the abyssal circulation of the world ocean, modeling of sinking and upwelling motions in the ocean, several articles on observations and analyses of motions in the Indian Ocean, and the work on double-diffusive phenomena. In addition to publications on a variety of other topics, he produced global charts relating currents to wind stresses, and together with different coworkers, he presented the results from several extended oceanographic cruises.

He was also becoming increasingly involved with oceanography on a grander scale, sparking much of the exploration into the physical oceanography of the Indian Ocean, and using his stature and influence to establish international observational programs. He played key roles in getting the Geochemical Oceans Sections Studies (GEOSECS) and the Mid-Ocean Dynamics Experiment (MODE) programs launched. His interest in making oceanographic data much more accessible was expressed in a publication with Pivar and Fredkin (1963) on the use of a computer to produce graphs of different oceanographic variables on command. At that time that approach was little more than a dream, but it has since been realized and is now operational on some computer systems.

In 1964 he was awarded an honorary Ph.D. by the University of Gothenberg, and a decade ago, Yale and the University of Chicago also recognized Stommel's achievements with honorary doctorates.†

In 1969, he participated in the multiship MEDOC campaign, which he had organized for the purpose of observing the formation of deep water in the western Mediterranean Sea. The success of that effort was reported by the MEDOC Group (1970). Actual instances of bottom water formation have rarely been observed,

* During the 1950s Hank had often talked about returning to graduate school for a Ph.D. He was very conscious of the fact that he did not have that degree and was wont to address even beginning graduate students as "Doctor." I think that the move to Harvard resolved the problem by placing him in the position of producing Ph.D.s. There were other reasons for his departure from WHOI but my own feeling is that the main reason was that the Harvard position provided academic recognition of his achievements.

* One student who listened to Hank go through one of these pilot studies emerged with a bewildered look and said to me, "I think that I understood what he did but what's the problem?"

† Alumni officials at Yale were surprised when Hank was proposed for an honorary degree because he had long since disappeared from their lists. Many years earlier, tired of being dunned for alumni contributions, he had written DECEASED on a pledge form and returned it to Yale. It worked, but he was reincarnated by the honorary degree.

and the MEDOC report contains a well-documented account, one that will be a useful starting point for theories. The treatise on the Kuroshio (1972), edited with K. Yoshida, occupied a good deal of his time in the late 1960s. He has continued to produce intriguing, theoretical models, as is evidenced by the last of the abyssal circulation papers with Arons (1972), showing how deep western boundary currents can be extensively broadened by topography. His lifelong interest in maps and charts is reflected in a guide to oceanographic atlases published with M. Fieux (1978). His faith in at least the statistical simplicity of ocean circulation appears in the study with A. Leetmaa and P. Niiler (1977) of the applicability of the Sverdrup relation to mid-Atlantic circulation and in the exploration of oceanographic data with F. Schott (1977) that led to the idea of the β-spiral. The latter study was produced during a sabbatical term at Kiel in 1976, and it has been extended in papers with Schott (1978) and D. Behringer (1980).

It is interesting to note that Stommel was a principal driving force in establishing the MODE program. He thought that a thorough study of oceanographic data in a restricted area would reveal strong mesoscale motions similar to cyclones and anitcyclones in the atmosphere, and this proved to be the case. Yet even though he remains associated with the successor POLYMODE program, he himself has not become involved in research on geostrophic eddies.

In looking back over Stommel's voluminous output, one is struck by his persistent search for simplicity in explaining observed phenomena. His models of the ocean vary depending on the problem. They may consist of infinitely deep or infinitely wide layers, one-dimensional channels or pipes and reservoirs.* He is much more likely to focus on some truth and an approximation to the truth rather than the whole truth and nothing but the truth. To my knowledge, he has never contrived ad hoc structure in his parameterizations in order to explain phenomena. His models are often constructed so that they can be analyzed with relatively modest mathematical tools, a characteristic that gives them a deceptively simple appearance. Not infrequently, his mathematical solutions have contained mistakes, but his superb intuition has invariably led him to the correct physical results.

With his move to WHOI in 1978, Stommel has returned to the research environment that is so dear to him. His concern for pertinent problems is evident in his increasing involvement with the climatic application of ocean–atmosphere interaction. But for most of us the best news is that once again he is immersed in the search for simple models to elucidate important oceanic phenomena.

* In the early 1950s W. Malkus, who was then at WHOI doing turbulence experiments with water, was frustrated by Hank's laminar, inviscid models of oceanic flow which nevertheless seemed to yield qualitatively correct results. For Stommel's benefit he had pasted the values of the viscosity coefficient ν on the fresh- and salt-water taps in his laboratory. He had marked the fresh water tap "$\nu \neq 0$" and the salt water tap "$\nu = 0$."

Henry Stommel and Louis N. Howard on a rotating table in the cellar of Walsh Cottage at WHOI, summer 1968.

Notes Related to Stommel's Early Years in Woods Hole

Raymond B. Montgomery

Henry Stommel's connection with Woods Hole Oceanographic Institution began when he came from Yale University in 1944. My association with him began in 1945. I want to mention some of the older scientists who were important to him and to me.

My own connection with WHOI had begun in 1931, and I should like to record the great benefit I derived from the leadership of H. B. Bigelow (1879–1967) and C. O'D. Iselin (1904–1971). As a student of C.-G. Rossby (1898–1957) at Massachusetts Institute of Technology, I had become familiar with the names of European, especially Scandinavian, oceanographers. I met H. U. Sverdrup (1888–1957) during his visits to Woods Hole. Rossby arranged that I work with A. Defant (1884–1974) in Berlin during the winter of 1938–1939 and with E. Palmén in Helsingfors during the summer of 1939. My return journey on the *Stavangerfjord* from Bergen to New York was made in company with B. Helland-Hansen (1877–1957) and H. Mosby. They were bound for the seventh general assembly, in Washington, of the International Union of Geodesy and Geophysics, Helland-Hansen being President of the Association d'Océanographie Physique. This assembly of IUGG was the first in America and was beclouded by the outbreak of World War II (1 September) just before the assembly (4–15 September). Attendance by Defant and other German oceanographers was canceled, and many more Europeans were absent. V. W. Ekman (1874–1954), during what must have been his only trip to America, lectured in Woods Hole and at the Washington assembly, and I was privileged to meet him in both places. He had been awarded the Agassiz Medal for Oceanography by the National Academy of Sciences in Washington 24 April 1928 but was not present in person.

In November 1945, following the close of the war, Iselin allowed me to return to Woods Hole, and that is when my thoroughly enjoyable acquaintance with Stommel began. Rossby again became a frequent visitor to Woods Hole, as he had been during his MIT years preceding the war. Attracted by Rossby, Stommel spent the spring of 1946 in Chicago.

Ekman had been strongly influenced by F. Nansen (1861–1930) and by V. Bjerknes (1862–1951), the two men who suggested to Ekman the topics for his early oceanographic studies. The progression of ideas is interesting. Ekman's work, especially his paper (1905) concerning the influence of the earth's rotation on wind-driven currents and his paper (1923) introducing the concept of the vertical component of vorticity, formed the background for Rossby's (1936) development and application of the vorticity equation. In turn, Stommel's influential paper (1948) on the westward intensification of wind-driven ocean currents resulted from his familiarity with the Rossby vorticity equation.

Stommel's paper was preceded by another very influential paper, that by Sverdrup (1947), and important features are common to both papers. While the casual reader might assume that Stommel had been helped by Sverdrup's slightly earlier work, I am convinced that the two papers were prepared quite independently. Sverdrup's was published in the November 1947 issue of the *Proceedings of the National Academy of Sciences.* Stommel had already presented his paper on 18 September 1947 on the unusual occasion of a Woods Hole meeting of the American Geophysical Union, and his paper was received for publication on 25 September. It appeared in the April 1948 issue of the AGU *Transactions.*

Henry Stommel in his typically cluttered office at WHOI, August 1979. Photograph by V. Cullen.

I am pleased to have played a part in the genesis of Stommel's paper. I think he and I were sharing an automobile trip between Woods Hole and Providence, probably in early 1947, when I recounted to him that Iselin had once pointed out to me an important problem: Why is the Gulf Stream narrow and swift and pressed against the western boundary of the North Atlantic Ocean? (This question, now so obvious, was novel then. And I do not pretend to reproduce the words that Iselin used; the term "boundary current" had not come into use.) If I remember correctly, Stommel answered the question qualitatively on a scrap of paper during a few minutes of discussion at a coffee stop during our short trip. My small part is generously acknowledged in his Gulf Stream book (1965; first edition 1958). The origin of the question is an example of the help Stommel and I and many others received from Iselin.

Stommel sought out stimulating scientists. Immediately following the September 1947 meeting already mentioned, he went to London to learn relaxation techniques from R. V. Southwell (1888–1970). Among the persons Stommel enjoyed meeting in London were D. Brunt (1886–1965), G. E. R. Deacon, and P. A. Sheppard (1902–1977). Stommel visited Cambridge to talk with G. I. Taylor (1886–1975). The highlight of Stommel's stay in Great Britain was the few days in January 1948 spent with L. F. Richardson (1881–1953) in Argyll, Scotland; the measurements they made together resulted in the joint paper in *Journal of Meteorology* that year. In 1949 Stommel spent several months at Scripps Institution of Oceanography. Besides meeting other oceanographers of his own generation, he became acquainted with C. Eckart (1902–1973) and E. C. Bullard (1907–1980). Stommel wrote me with special pleasure of time spent with Defant, whose delayed journey to America brought him to Scripps in October 1949 for several months and to Woods Hole in February 1950 for several days.

The eleventh general assembly of IUGG, the second in America, was held in Toronto on September 1957. Mosby was then President of the Association d'Océanographie Physique. As part of this assembly, Stommel arranged an outstanding Symposium on the General Circulation of the Ocean, with Particular Emphasis on the Deep-Water Movements. The symposium filled two entire days, 5–6 September. A feature memorable to many of us Americans was the opportunity to meet some of our Soviet counterparts for the first time. Despite the symposium's success, we keenly felt the absence of certain leaders. Stommel had invited Rossby and Sverdrup. But Rossby had died in Stockholm on 19 August and Sverdrup had died in Oslo on 21 August. Helland-Hansen, also unable to attend, died in Bergen on 7 September, while the assembly was in progress.

Henry Stommel

G. E. R. Deacon

Marine scientists all over the world hold Henry Stommel in great affection and esteem. His kindness and sincerity, supported by intellectual curiosity and clear thinking, are a universal source of inspiration.

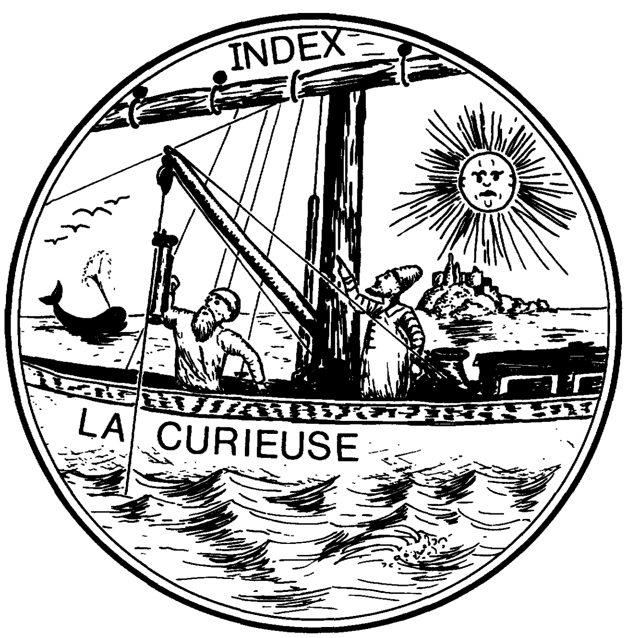
He came to Woods Hole when oceanography was still in a primitive state. *The Oceans* by Sverdrup, Johnson, and Fleming had been published, war-time needs had promoted rapid expansion, and we had the bathythermograph, but most of the observations were made with methods nearly 50 years old. We had to titrate all our salinities, and our data, widely separated in space and time, had to be interpreted on the assumption that they represented steady-state conditions. It was not until 1950 that six ships working together between Cape Hatteras and Newfoundland gave us much idea of detail and variability, and not until 1957 that the joint British-American cruise in the *Aries*, sparked off by Stommel's ideas on deep-water movements, began to show that the deep circulation might consist of a wide spectrum of motions, some of them with velocities at least an order of magnitude faster than the mean velocities. It is only recently that we have been able to make the continuous, long-term recordings required for a realistic picture, and to apply theories that take reasonable account of both winds and density gradients.

Stommel can look back on this lively period with much satisfaction. The impressive list of his personal contributions is enhanced by so many joint publications that it reads like an author index to an "advances in oceanography."

His unpretentious approach and happy turn of phrase are magnetic. Who but he and L. F. Richardson would begin a serious study of eddy diffusion in the sea with the words "we have observed the relative motion of two floating pieces of parsnip." Other pleasing images are the salt fountain, varieties of oceanographic experience, submarine clouds, and the smallness of sinking regions. His surveys of progress, and his emphasis on the value of individual ideas, activity, and enthusiasm, and of directing expedition plans toward specific questions have been very timely. He was, I believe, the unintentional originator of the International Indian Ocean Expedition when he wrote round to all his friends to ask for information likely to be of help in a study of the time taken by the currents to respond to monsoonal reversal of the winds. But he was also, I am sure, editor of the five issues of *The Indian Ocean Bubble*, which "in gently pejorative tone" resisted too much regimentation and helped to formulate key problems.

I think some of us, rather overpowered by his achievements, have sometimes been revived at seeing him no less interested in simpler things: in the beginning of science, and in everyday life. He told us about William Leighton Jordan, the amateur oceanographer who received little help or sympathy from the professionals, about Whiston and Ditton, who suggested midnight guns fired from hulls moored along the shipping lanes as a means of determining longitude, and about Halley, the scientist and ship's captain, who, with good reason, had to court-martial all his officers.

It would be delightful to have an intimate record of his experiences, from the early postwar years in Woods Hole, when the Old Rectory was shared by a remarkable group of young scientists, into all the widening circle of his work and interests. He could write a fascinating autobiography.



The logo designed by Henry Stommel for INDEX, a program of the late 1970s focused on the monsoonal currents of the northwestern Indian Ocean. The name, *La Curieuse,* derived from a charter vessel that Stommel and colleagues used for a time out of the Seychelles, although that latter vessel was marginally more suited for oceanographic research than the one depicted on the logo.

Henry Stommel—On The Light Side

F. C. Fuglister

In the beginning, God said, "Let there be light." How long He had been stumbling around in the dark before He had this brilliant idea we are not told, but we can well imagine His delight when, at last, with the light on, He could see! The most impressive sight of all must have been His first glimpse of the world's ocean, that vast, awesome, mysterious deep. Later God created man to study this phenomenon and clear up the mysteries; and this leads us inevitably to Henry Melson Stommel, the man whose sixtieth anniversary is being celebrated with this volume.

To what extent Stommel has carried out God's plan is described elsewhere in this book; here I will just note that on some occasions he (Hank) may not have been entirely serious about his job and may even have slowed down the plan.

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 (see the prior sketches by Arons and Veronis). 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 (M. Deacon, 1971, p. 376), in the following announcement, issued some years ago:

Society of Subprofessional Oceanographers
Special Committee
for

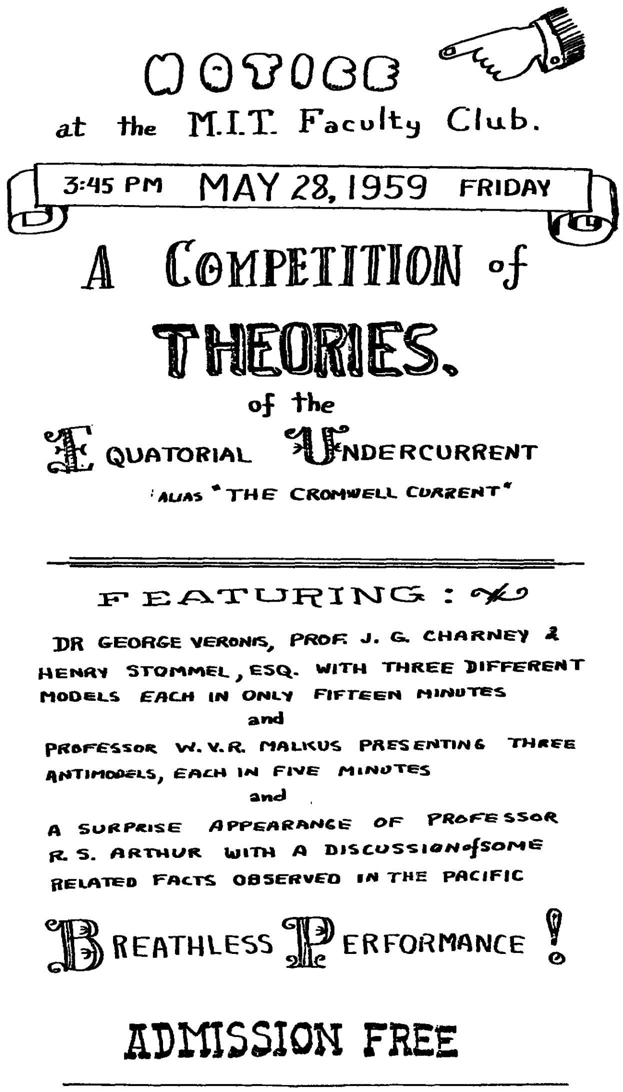
THE WILLIAM LEIGHTON JORDAN ESQ. AWARD

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 overambitious, 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.

Stommel puts out other announcements, as Arons has already recalled in this volume. Another example, advertising a seminar he organized on the newly rediscovered Equatorial Undercurrent, is reproduced here. These are all characteristic of his own appreciation for "the light side."


NOTICE
at the M.I.T. Faculty Club.
3:45 PM MAY 28, 1959 FRIDAY
A COMPETITION of
THEORIES.
of the
EQUATORIAL UNDERCURRENT
ALIAS "THE CROMWELL CURRENT"
FEATURING:
DR GEORGE VERONIS, PROF. J. G. CHARNEY & HENRY STOMMEL, ESQ. WITH THREE DIFFERENT MODELS EACH IN ONLY FIFTEEN MINUTES
and
PROFESSOR W. V. R. MALKUS PRESENTING THREE ANTIMODELS, EACH IN FIVE MINUTES
and
A SURPRISE APPEARANCE OF PROFESSOR R. S. ARTHUR WITH A DISCUSSION of SOME RELATED FACTS OBSERVED IN THE PACIFIC
BREATHLESS PERFORMANCE!
ADMISSION FREE

Life and Work of Henry Stommel

Born September 27, 1920

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

Instructor in Mathematics and Astronomy, 1942–1944, Yale University
Research Associate, 1944–1960, Physical Oceanographer (nonresident), 1960–1978, Woods Hole Oceanographic Institution
Professor of Oceanography, 1960–1963, Harvard University
Guest Lecturer, 1969–1970, Laboratoire d'Océanographie Physique du Muséum National d'Histoire Naturelle, Paris, France
Professor of Oceanography, 1963–1978, Massachusetts Institute of Technology
Senior Scientist, 1978–, Woods Hole Oceanographic Institution

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
Elected to 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

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