寒風双渦

COLD WIND TWO GYRES

A Tribute To

VAL WORTHINGTON

by a few of his friends
in honor of his forty-one years
of activity in oceanography

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EDITORIAL PREFACE

Val Worthington has worked in oceanography for forty-one years. In honor of his long career, and on the occasion of his sixty-second birthday and retirement from the Woods Hole Oceanographic Institution, we offer this collection of forty-one papers by some of his friends. The subtitle for the volume, “Cold Wind-Two Gyres,” is a free translation of his Japanese nickname, given him by Hideo Kawai and Susumu Honjo. It refers to two of his more controversial interpretations of the general circulation of the North Atlantic.

The main emphasis of the collection is physical oceanography; in particular the general circulation of "his ocean," the North Atlantic (ten papers). Twenty-nine papers deal with physical oceanographic studies in other regions, modeling and techniques. There is one paper on the “Worthington effect” in paleo oceanography and one on fishes – this last being a topic dear to Val's heart, but one on which his direct influence has been mainly on population levels in Vineyard Sound.

Many more people would like to have contributed to the volume but were prevented by the tight time table, the editorial and referee process, or the paper limit of forty-one. We are sure that we speak for all these people as well as the authors and referees of the collection when we wish Val a happy retirement and good fishing. We are sure that Val's ideas will continue to stimulate physical oceanography for many years to come and hope that Val does not choose to remain totally “on the sidelines.”

Mike McCartney, Bob Beardsley, Harry Bryden
Special Editors

L. VALENTINE WORTHINGTON

Introductory Note

Customarily the editors of a volume such as this include an overview discussion of the honoree's career. For Val Worthington, we have elected to do something different. Upon retirement, Val produced two summaries of his career, one written, one oral. Much of his funding over the years came from the Office of Naval Research, and in late 1981 he produced a contract final report, "North Atlantic Circulation and Water Mass Formation." As this summarizes a large part of his professional career, and does it much more eloquently than we could hope to, it is included here. One of the great pleasures in being around Val is hearing him tell stories. In January 1982, just before leaving Woods Hole for his winter retirement home in the Bahamas, he gave a final seminar to the Physical Oceanography Department of the Woods Hole Oceanographic Institution. This seminar, entitled "Apologia pro vita sua," was presented from a more general point of view than the written final report, and we have therefore supplemented the report with some quotations from this seminar, as well as some additional information. It is hoped that most of the facts and events of Val's professional career have been included. It is impossible to capture the humor and style of Val Worthington on paper.

Mike McCartney

BIOGRAPHICAL NOTES

L. Valentine Worthington

Date of Birth:	March 6, 1920.
Place.	Chelsea, London, England.
Education:	
1938-1941	Princeton University.
Positions.	
1941-1943	Technician, Woods Hole Oceanographic Institution.
1943-1946	U.S. Navy.
1946-1951	Technician, Woods Hole Oceanographic Institution.
1951-1958	Research Associate, Woods Hole Oceanographic Institution.
1958-1963	Physical Oceanographer, Woods Hole Oceanographic Institution.
196 I-present	Ambassador to the Court of St. James, Society of Subprofessional Oceanographers (SOSO).
1963-1982	Senior Scientist, Woods Hole Oceanographic Institution.
1974-1981	Chairman, Department of Physical Oceanography, Woods Hole Oceanographic Institution.

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North Atlantic circulation and water mass formation

by Valentine Worthington

When I returned to Woods Hole after serving in the Navy[1] from 1943 to 1946, I worked as a technician for F. C. Fuglister during his classic descriptions of Gulf Stream meanders and Gulf Stream ring formation. Our work resulted in a joint paper (Fuglister and Worthington, 1951) on a multiple ship survey[2] of the Gulf Stream. It also resulted in my promotion to the scientific staff in September 1951. During this work it was observed that the measured surface velocity of the Gulf Stream often reached 250 cm s^{-1}, about double the values obtained before the war by conventional dynamic calculations. We reasoned that with the new, precise LORAN navigation it might be possible to make hydrographic sections with closely spaced stations at right angles to the Stream so that the true slopes of the isobaric surfaces could be calculated. Three such sections were made in October-November 1950, and, although the Gulf Stream transport was far lower than average, the calculated surface velocities closely approximated the measured velocities and were as high as 225 cm s^{-1} (Worthington, 1954a).[3]

In early spring of 1951 and 1952 a Navy expedition made numerous landings on the ice-pack in the North Polar Sea. Hydrographic stations were made through holes drilled in the ice at some of these landing sites using a light-weight winch mounted

1. Val leaves out the pre-1946 period. He came to the Woods Hole Oceanographic Institution in 1941. He was hired (as a "sturdy non-professional") to help with Columbus Iselin's planned bathythermograph (BT) survey of the western North Atlantic. This survey was intended to be completed before the U.S. got in the war. After two training cruises ("I was a little backward. I had to go have two training cruises on *Atlantis"),* he began field work on this sonar survey. He claims to have made more BT lowerings than anyone else. He also worked on improving the BT. During this time, Alfred Woodcock got interested in Langmuir cells and Val participated in a dingy-based experiment with small surface drifters in the Gulf of Mexico ("Woodcock rowed and I dropped these things into the ocean"). He served for three years in the Navy ("The part I took in the war was a minor one: the battle of Florida, where you did not actually see anyone more hostile than a bartender"). He participated in the field demonstration of Maurice Ewing's SOFAR principle. After the war, two developed tools were available to oceanography; LORAN navigation and the perfected BT. He started work with Fritz Fuglister on tracking the Gulf Stream ("As soon as I got out of the Navy I was able to go to sea again").

2. This was the Navy operation CABOT ("The Navy has to have a funny name for everything, and CABOT stands for Current And Bathythermograph Observations Trip").

3. Later Fritz Fuglister handed over the *Atlantis* to Val in St. Johns, Newfoundland, with the instruction to follow the Gulf Stream to Europe. "But as we all know now, the Gulf Stream doesn't go to Europe and I had certain difficulty."

in the R4D aircraft. On the basis of these stations I postulated the existence of an anticyclonic gyre[4] in the Beaufort Sea North of Alaska, and of a submarine ridge that separated the deepest waters of the European-Asian side of the North Polar Sea from those of the Canadian-Alaskan sector (Worthington, 1953).[5] Both these features were subsequently confirmed – the "Lomonosov Ridge" was later described in detail in the Soviet literature.

On a cruise to the Puerto Rico Trough in spring 1954 measurements of dissolved oxygen showed far lower concentrations in the deep water than those from prewar cruises. This led to the supposition that bottom water was formed only in periods of cold climatic fluctuation and that formation of North Atlantic deep water had been suspended since the "Little Ice Age" of the late eighteenth and early nineteenth centuries (Worthington, 1954b).[6]

Although this hypothesis was later shown to be erroneous, it had the result of persuading our director, C. O'D. Iselin, that the western North Atlantic deserved another look. A series of cruises was begun to resurvey that part of the ocean. In these cruises I followed the example of Georg Wüst and his colleagues in the *Meteor* expedition in that I attempted to sample the whole water column on every station.

The first of these cruises, in September 1954, consisted of a deep hydrographic section from Nova Scotia to Bermuda. This section revealed, for the first time, that the slopes of the isotherms associated with the Gulf Stream did not diminish with increasing depth but persisted all the way to the bottom. If the bottom was used as a reference surface, the calculated Gulf Stream's baroclinic transport was about 129×10^6 m^3 s^{-1}, nearly double the value obtained if the customary 2000 m level was used. This led me to believe that the Gulf Stream did in fact, flow in the same direction top to bottom, but this, in turn, forced me to conclude that the return flow was not strictly geostrophic – an hypothesis that has won few adherents. Stommel (1958a) was quick to see the implications of the new, deep observations, but he reasoned that one could conserve mass in the Gulf Stream System without violating geostrophy if the level of no motion was assumed to lie at about 1600 m in the western North Atlantic.

In November 1954 the Schleicher and Bradshaw (1956) salinometer was introduced, and we found that, with careful sampling, salinity could be measured to

4. "sort of a preliminary 'two gyre paper' on the North Polar Sea."

5. "I thought it was pretty hot stuff, I had written a paper and established an eddy and deduced a ridge that was subsequently announced by the Russians. I sat around waiting for requests for reprints, and none came. Finally in about a week I got two letters. One was from a gentleman in the Geological Survey who wanted to measure the Mississippi in winter. He said, 'How do you dig the holes in the ice?' The second was from a man who operated a pump factory in Connecticut and his interest was similar: 'That's just what we need. How did you do it?' This is a nice lesson in scientific humility; I don't think you should get too excited about your work."

6. On this trip Val learned to measure oxygen. His long term interest in oxygen measurements seems to have been a prime moving force behind the improvement in techniques and standardization procedures that followed the I.G.Y. His work in this area continues: Worthington, 1982.

three decimal places. This instrument made chemical titration of open-ocean samples obsolete, and for a few years, until the Schleicher and Bradshaw instrument had been copied elsewhere, Woods Hole had a monopoly on high quality hydrographic data.

The first long cruise using this instrument went east from Bermuda to the Mid-Atlantic Ridge, thence southwest to Barbados and into the Caribbean Sea. It included a station in the Cariaco Trough north of Venezuela. This station revealed that the deep water in the trough was anaerobic, and the trough has been a source of fascination to chemical oceanographers ever since. The deep dissolved oxygen in the eastern basin of the Caribbean Sea suggested to me that the sea had filled catastrophically (Worthington, 1955). All hypotheses based on Woods Hole oxygen data taken before June 1959 are suspect, since before that date we could not measure dissolved oxygen properly. However, I still believe that this hypothesis is nearly correct since later current measurements indicated a negligible inflow of dense water into the eastern Caribbean (about 56×10^3 m^3 s^{-1} according to Stalcup and Metcalf, 1975). In June 1955 the third cruise of this series covered the Hatteras-Bermuda-Florida triangle;[7] the quantity of data coming in became too great for me-to handle and Lorraine Barbour became my assistant. This step resulted in a big increase in data quality and in my scientific output; it still does.

An early spinoff from this resurvey of the western North Atlantic was the suggestion, by Henry Stommel, that it would be interesting to measure the deep currents indicated by one of my sections, off Cape Romain, North Carolina. In this section the deep gradients lay along the Blake Outer Ridge but the rapid surface Gulf Stream, which might make measurements difficult, was farther inshore. In early 1957 we induced John Swallow to come over from England in *Discovery II* and deploy his neutrally buoyant floats in the deep gradients. Below about 2000 m the floats drifted steadily southward, and my accompanying hydrographic data taken from *Atlantis* indicated a deep western boundary current of about 6×10^6 m^3 s^{-1} (Swallow and Worthington, 1961). This provided powerful evidence that there was indeed an intermediate level of no motion in the western North Atlantic as Stommel (1958a) had proposed. I was dumbfounded.

On the return trip to England, we took the Schleicher-Bradshaw machine aboard *Discovery II* and made the first of a series of deep transatlantic sections which were to be translated into our International Geophysical Year program. This initiated a very happy cooperation with the National Institute of Oceanography. In the years 1957-1959, F. C. Fuglister, W. G. Metcalf, A. R. Miller and I continued this work

7. Val issued a challenge based on this "Bermuda Triangle" dataset. The challenge is to produce a circulation solution that simultaneously satisfies geostrophy and mass conservation and that does not do violence to water mass distributions. The prize of a case of prime whisky was claimed by Carl Wunsch based on his 1977 inverse solution. Val remains skeptical, so gave a case of pony bottles rather than full bottles. A more recent pending claim is by George Veronis who has applied the technique described in Fiadeiro and Veronis (this volume).

in Atlantis, Crawford and Discovery II. The resulting data were published in Fuglister's (1960) atlas of vertical sections of temperature and salinity.[8] There were gaps in the data base but one of these was soon filled by G. Dietrich (1960) of Kiel University, who surveyed the Irminger Sea in the summer of 1958, in the F.F.S. *Anton Dohrn,* with K. E. Schleicher aboard. Later my own cruise in *Erika Dan* (in 1962) covered the Labrador Sea and included two more transatlantic sections. At this point the whole North Atlantic had been pretty well covered by high-quality temperature and salinity observations. This coverage resulted in two more atlases, that of Worthington and Wright (1970), a quasi-horizontal atlas with charts of salinity on potential temperature surfaces, and that of Wright and Worthington (1970), which provided a fine-scale volumetric census of the water masses of the whole North Atlantic. The tragic shortcoming of the Woods Hole I.G.Y. data set was that accurate oxygen and nutrient data were lacking.

In the autumn of 1956 when C. G. Rossby was visiting Woods Hole, Iselin introduced me to him as the "man who has to rewrite my 1936 paper" (Iselin, 1936). This was news to me, but later, when the I.G.Y. data were in, Iselin reiterated this mandate. I told Iselin that it would be premature to embark on a new paper until a few questions that had been raised by the I.G.Y. data had been answered. In fact, these "few questions" multiplied, and it was not until after Wright and I had published our two atlases that I actually started to write.

The first of these questions involved the production of North Atlantic Deep Water. In earlier work on the time-scale of the North Atlantic circulation (Worthington, 1954b) it had seemed reasonable to assume that North Atlantic Deep Water was not being formed in the modem era because the density in the upper layers of the Labrador Sea, where it was supposed to be formed, never seemed to exceed about σ_t 27.78, while that in most of the deep water exceeded 27.90. My error, which was common to many of us, from H. U. Sverdrup on down, was that of neglecting the Norwegian Sea overflows. Cooper (1955) focussed new attention on these overflows, and the I.G.Y. data confirmed their importance as a source of North Atlantic Deep Water. One of the first efforts to assess the amount of the Iceland-Scotland overflow was made in the autumn of 1960 (Steele, Barrett and Worthington, 1962). We found a rapid current (20-30 cm s^{-1}) composed of dense overflow water mixed with entrained Atlantic water, flowing along the continental slope south of Iceland. We estimated its volume flux at 5×10^6 m^3 s^{-1}. The I.G.Y. data had confirmed Cooper's suggestion that this Iceland-Scotland overflow water passed through a gap in the Mid-Atlantic Ridge near 5ON and thereafter flowed northward along the western slope of the Reykjanes Ridge. In 1964 moored current

8. "One of the other outputs from the International Geophysical Year was an obscure paper that Metcalf and I gave at a meeting in Copenhagen in 1959.... This proved to be a sleeper." This paper, Worthington and Metcalf (1961), remains an important benchmark for North Atlantic water mass circulation.

meters and neutrally buoyant floats were used to estimate the westward flux through this gap – the Gibbs Fracture Zone – about $5 \times 10^6\ m^3\ s^{-1}$ (Worthington and Volkmann, 1965).[9]

Earlier, on the *Erika Dan* cruise that completed the I.G.Y. survey of the North Atlantic, similar deep measurements had been made southwest of Cape Farewell, Greenland and off southeastern Labrador. From dynamic computations and the float drifts, we estimated a net southward flow of $10 \times 10^6\ m^3\ s^{-1}$ off Labrador (Swallow and Worthington, 1969). I believe this number is the most reliable estimate of the total production of North Atlantic Deep Water, consisting of $5 \times 10^6\ m^3\ s^{-1}$ of Iceland-Scotland overflow combined with $5 \times 10^6\ m^3\ s^{-1}$ of Denmark Strait overflow. The currents measured earlier off North Carolina (Swallow and Worthington, 1961) are the continuation of the same deep western boundary current that conforms quite closely with Stommel's (1958b) theoretical model except that this current originates as Norwegian Sea overflows and not as Stommel originally supposed, through deep water convection in the Labrador Sea. Other work by Barrett (1965) and by Worthington and Kawai (1972) has suggested that the volume transport of this current is highly variable – as little as 2 and as much as $12 \times 10^6\ m^3\ s^{-1}$. It is clearly an interesting feature that deserves much more attention.

In 1963 sections were made across the two deepest sills that separate the Caribbean Basins from the outside Atlantic – the Virgin Islands Passage and the Windward Passage. These sections confirmed my earlier impression that the deepest waters of the Caribbean were not being renewed at the present time. The western section also suggested that the deepest waters in the Cayman Basin south of Cuba had warmed up by about 0.03°C since 1934 when Parr (1937) made his original study of the Caribbean. This warming, it appeared, had been the result of geothermal heat flow and it seemed possible that the deep water was neutrally stable; if so, it should be overturning convectively. To measure vertical motion in the ocean, D. C. Webb came up with a neutrally buoyant float with inclined blades on the outside housing that caused the float to rotate in response to any vertical movement past it. In February 1965 three of these instruments were launched in the deep water south of Cuba, two of them at 2000 m where the water was not neutrally stable and one at 5000 m where the water *might* have been neutrally stable. The results were inconclusive – the 2000 m and 5000 m floats behaved in the same way – they all indicated upward and downward flows of about 60 m with a very roughly diurnal period. However, at least we had established that vertical motion could be measured.

9. Val later ("right after the first superbowl game") attempted a moored current meter study (Worthington, 1969) of the Denmark Strait overflow. "It was a disaster.... When you put out 30 current meters and get one usable record, you can't crow too much." An overview of most of Val's studies of overflows is contained in Worthington (1970).

The greater part of my research during the I.G.Y. and immediately thereafter had been in observations at sea, and my attention had naturally been focussed on the deep water because it was virgin territory-before I began the resurvey in 1954 there were only five stations that reached to a depth of 5000 m in the western North Atlantic and in these the salinity data were inaccurate. The data processing for the two atlases (Worthington and Wright, 1970 and Wright and Worthington, 1970) had started in the late fifties and continued into the sixties. It was a laborious process, unaided by a computer, carried out chiefly by Lorraine Barbour.

However, this data processing did embrace the thermocline and surface waters of the North Atlantic, and it revealed a distinct discontinuity near the Grand Banks of Newfoundland. It seemed that the thermocline waters east of the Grand Banks contained more dissolved oxygen than those in the Sargasso Sea. Examination of historical data convinced me that the main circulation of the North Atlantic consisted of two, separate anticyclonic gyres (Worthington, 1962).[10]

The data from a cruise to this region in early 1964 strengthened my conviction that there were two distinct current systems confined to the western side of the North Atlantic.[11] Also established was the existence of a cold, low pressure trough that separates the two gyres. This hypothesis has had some opposition (Mann, 1967; Clarke *et al.,* 1980) on the ground that it violates geostrophy, but I have yet to see a one-gyre circulation scheme that conserves mass and salt and does not make nonsense of water mass distribution in the North Atlantic or of such current measurements as are available.

If the two-gyre system proposed was, indeed, a rough description of the general circulation, it clearly bore little resemblance either in shape or in volume transport to earlier wind-driven circulation schemes. I had always been dissatisfied with wind-driven circulation, principally because its proponents ignored water mass characteristics; their source region did not contain western North Atlantic water that is transported, exclusively, by the Gulf Stream. In 1972 in a volume dedicated to Georg Wüst's 80th birthday, I proposed a mechanism of anticyclogenesis that, paradoxically, attributed the asymmetrical lens of warm water in the northern Sargasso Sea, around which the Gulf Stream System revolves, to winter outbreaks of polar continental air.[12]

From our earliest collaboration Fuglister and I had been fascinated by the deep layer of 18'C water in the Sargasso Sea. In those days, when the Navy insisted on the Fahrenheit scale, the 900-foot bathythermograph traces invariably crossed the

10. An hypothesis which "has received mixed reviews, I must say."

11. "When one makes revolting things of this nature, you have to give a P.O. seminar on it, and afterwards a bright young theoretician from Harvard came up and showed me a picture and said, 'Look, two gyres!' I said, 'What the hell, your northern one is going cyclonically, not anticyclonically.' He replied, 'That doesn't matter!'"

12. "If you have something really controversial, publish it in a festschrift, cause they hate to turn it down."

65° isotherm (18.33°C) but never the 64° isotherm (17.78°C) south of the Gulf Stream (except in Gulf Stream rings). If any trace appeared to cross the 64° isotherm, the instrument was off calibration. We know from Iselin's work (1936, 1940) that in wintertime deep mixed layers were formed at a temperature of 18°, but it was obvious that there was a distinct thermostad at 18° that had been observed as far south as 2ON where the winter surface temperature never drops below 25°C. In 1959 I suggested that 18° water was formed in a manner similar to bottom water formation at high latitudes — it was cooled in excess quantities and flowed off to the south and warm surface water was advected north to replace it. In the case of 18° water the main thermocline was analogous to the bottom (Worthington, 1959).

The greatest depth of the main thermocline south of the Gulf Stream is *always* found beneath the deepest mixed layer of 18° water. This greatest thermocline depth represents the center of the oceanic anticyclone that is the Gulf Stream System. Moreover, Iselin's (1940) hydrographic sections made between 1937 and 1940 indicated (roughly) that the Gulf Stream baroclinic transport was greatest at the end of winter (after the 18° water had been formed); thereafter, the transport diminished gradually to a minimum in late fall.

The conditions south of the Kuroshirol[13] off Japan are strikingly similar-again, deep mixed layers are formed in late winter by polar continental air and the greatest thermocline depths in the western North Pacific lie beneath these mixed layers. It appeared to me that western boundary currents may owe their energy to polar continental air rather than to the wind stress.[14] In the southern hemisphere where the land masses are thin and do not reach to high latitudes, the currents are notably weaker.

After about three years of writing, my long paper. "On the North Atlantic Circulation" was submitted to the Johns Hopkins Oceanographic Studies series in January 1974. In roughest outline I divided the North Atlantic into five layers — deep water (<4°), lower thermocline (4°-7°), mid-thermocline (7°-12°), upper tbermocline (12°-17°) and warm water (>17°). I drew circulation diagrams for each of these layers in which each flow line represents 5×10^6 m^3 s^{-1}. These flow lines, as far as possible, were consistent with water mass distribution; for example, a flow line that transported fresh Labrador Sea Water was not presumed to trespass into an area dominated by salty Mediterranean water. Also, no flow line was allowed to show water emerging, miraculously, from a land mass or vanishing into a land mass as flow lines based on relative dynamic topography invariably do.

The main circulation consisted of two anticyclonic gyres – the Gulf Stream System and the Northern Gyre. The Gulf Stream System below 7° was confined to the

13. Val's contribution to direct measurements of currents is most familiar for the North Atlantic, but also includes the North Pacific (Worthington and Kawai, 1972).

14. Interest in air-sea heat exchange and dissatisfaction with existing tabulations lead Val to his collaboration with the late Andy Bunker (Bunker and Worthington, 1976).

Sargasso Sea since little water colder than 7º flows through the Florida Straits. Above 7°, the anticyclone broadened to permit 30×10^6 cm^2 s^{-1} to pass through the Caribbean Sea and the Straits. However, this represented only one fifth of the anticyclone-the full Gulf Stream, south of Nova Scotia was estimated to transport 150×10^6 m^3 s^{-1}.[15] Northern Gyre, situated east of Newfoundland was smaller and weaker, transporting only 74×10^6 m^3 s^{-1} – it embraced only four of the five layers since water warmer than 17° is absent from it, except as part of the seasonal thermocline.

Also a box model was provided that illustrated my best estimate of water flux into and out of each layer. These box models are quite crude since all estimates of mg in the oceans seem to be too subjective.

The main objection to my circulation scheme for the North Atlantic is that it is not in geostrophic balance. I was not able to conserve mass geostrophically without grossly violating water mass boundaries or moving water through solid pieces of continent. If it has done nothing else, the paper has rekindled interest in the general North Atlantic circulation, particularly among the more theoretically inclined oceanographers.

A cruise to look at the Gulf Stream transport and the 18° water before the onset of winter was made in October 1974; we covered much of the western Sargasso Sea with XBTs and hydrographic stations. A late winter cruise in March 1975 was taken to observe the effect of winter, specifically the production of 18° water and the intensification of the Gulf Stream. The winter was an excessively mild one in the region and the winter-mixed layers did not fall below 19°. The Gulf Stream baroclinic transport actually weakened from 83 to 81×10^6 m^3 s^{-1}; these are the two smallest transports ever computed relative to the bottom, in this region. This was certainly consistent with the hypothesis that anticyclogenesis results from cold air, but it was disappointing to obtain only negative evidence. The main positive result from these cruises was the identification of a new class of eddies that we called "Big Babies." These eddies were about twice the size of conventional Gulf Stream rings and about half as energetic. They also behaved like giant drift bottles-being carried to the west by the Gulf Stream return flow. They originate in the extreme northeastern Sargasso Sea (McCartney, Worthington and Schmitz, 1978).[16]

It was not until the cold winter of 1976-1977 that any positive evidence for winter anticyclogenesis turned up. A cruise was made with the specific purpose of observing the effect of this really splendid winter in the northwest Sargasso Sea. Our XBTs revealed the deepest mixed layers ever observed in the Sargasso Sea, up to 650 m, and the main thermocline was about 100-150 m deeper than usual throughout the

15. "I've often been blamed or given credit for saying the Gulf Stream carries $150 \times 10^6 m^3$/sec. It's really other people's data that has done it. The big Gulf Stream seemed (1970) well established."

16. Talking about the general field of baroclinic eddies in the North Atlantic, Val notes, "All these eddies seem to be rooted in the deep water. You don't seem to be able to form an eddy from just surface water."

area(Leetmaa, 1977). The baroclinic transport of the Gulf Stream relative to 2000 m was 95 × 10^6 m3 s-1, the largest computed relative to this level, and and 145 × 10^6 m^3 s^{-1} relative to the bottom (Worthington, 1977b).

Some results of a census of the world water masses were published last year (Worthington, 1980). In this work the deep ocean volume was divided into very small classes of potential temperature versus salinity (0.1°C × *‰S),* in a manner similar to that employed in Wright and Worthington's (1970) North Atlantic atlas. The oceans were divided into eight different regions-North Pacific, South Pacific, and Southern Ocean Pacific; North Atlantic, South Atlantic, and Southern Ocean Atlantic; and Indian and Southern Ocean Indian. The total and the percentage of each temperature/salinity class are provided in tables. Simulated three-dimensional T-S diagrams also reveal quite clearly the relative abundance of the water masses in the three principal oceans.

Working on this census has convinced me more than ever that the North Atlantic Deep Water is the only water mass being produced at the present time. In an earlier work (Worthington, 1977a) it was impossible to reconcile the silica distribution in the Pacific (and by inference the Indian Ocean), with present bottom water production.[17] The difficulty is that the bottom water is rich in silica, but the surface water from which it must be formed is silica-free. The conventional process of deep water sinking, upwelling through the thermocline and poleward flow of surface water requires an unreasonable *input* and *output* of silica — about 30 × 10^{14} g y^{-1} of each, in the Pacific. Newly formed North Atlantic Deep Water is nearly silica-free as one would expect it to be since it is formed out of silica-free surface water. Also, as in 1960, we established that bottom water *was* being formed at the rate of 10 ×10^6 m^3 s-1. There seems to be much renewed interest in the poleward flow of warm surface water since it is a problem that has direct bearing on the world climate.

The next ten years should be among the most exciting of all time for physical oceanographers interested in the general circulation. The proposed reoccupation of some of the old transatlantic sections should help to determine the slow exchange between water masses, and it is these exchanges and the process of water mass formation that are most climate related. The ongoing SOFAR float program could very well answer the principal questions that remain concerning the more vigorous anticyclones[18] in the western North Atlantic if the investigators can see through the clouds of eddies that obscure the general circulation. I shall hope to watch some of these new efforts-but strictly from the sidelines.

17. Val's work on Antarctic Bottom Water continues with Whitehead and Worthington (1982).

18. "I don't think it matters whether I'm right or not, really. I think the point of the matter is always, 'are the observations any good?' One tends to get one's pride too much mixed up in one's work. The ocean is in good hands.... I feel I can retire to the Bahamas with a clear conscience."

Talking about his retirement home in the Bahamas, "While I have to go a long ways offshore to reach the Antarctic Bottom Water, the Deep Western Boundary Current washes my island's eastern boundary fairly close, and I can practically spit into Mediterranean water!"

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