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The delicate interplay between wind-stress and buoyancy input in ocean circulation: the Goldsbrough variations*

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Let us imagine that we are at Stratford on Avon. It is May 26, 1963. The last strains of the band concert have faded away. The audience has dispersed. The musicians are packing up their instruments. They notice an old gentleman, apparently asleep in one of the chairs. So ended gently the last holiday of George Ridsdale Goldsbrough, just one week past his 82nd birthday. He played a curious indirect role in the development of the theory of ocean circulation; many times over. The title of my talk might well be called the Goldsbrough variations.

Goldsbrough was seven years younger than the much more productive and recognized Swedish oceanographer, V. Walfrid Ekman. In the years before the First World War, when Ekman and the Norwegian Vilhelm Bjerknes were laying the groundwork for dynamical oceanography, Ekman with his wind-driven spiral and determination of the equation of state of sea-water, Bjerknes with his careful formulation of a practical method of dynamic current determination at sea, Goldsbrough was teaching secondary school students, and published two little studies of tides in simple basins. It was not until 1919, at the age of 38, that Goldsbrough was able to enter an academic career at the University of Durham as a Lecturer in Mathematics. By this time Ekman was grappling with the problem of coupling his theory of the surface wind-drift to a geostrophic current below, exploring the influence of coastlines, bottom topography, and the role of another spiral frictional layer at the bottom. Ekman published the results of these important labors in 1923, and he acknowledged that the model considered still fell far short of a theory of the circulation in an oceanic basin. In particular Ekman recognized that he had not taken sufficiently into account the variation of the Coriolis parameter with latitude; expressing this concern in his own words, "If finally a considerable region of the sea is considered so that the differences of latitude cannot be neglected, Cartesian coordinates x,y may not be admissible, and if in their place we introduce for instance the geographical longitude and latitude as independent variables, we obtain a differential equation which seems rather intractable." Formulating problems on a spherical earth was, however, familiar ground to Goldsbrough—as a tidal theorist—and in 1933 and 1934 he published two papers on steady ocean circulation that incorporated this extension exactly. They were very dry. The second was rather intricate. They did not seem very realistic, oceanographically speaking. Goldsbrough did not interpret the physics involved, and the papers were simply overlooked in the subsequent oceanographic literature. And yet they contained the seeds, had anyone studied them carefully, of Rossby's planetary wave theory of 1939, of Sverdrup's theory of 1947 relating curl of wind-stress to meridional transport, and the western boundary theories of 1948–56. I have found Goldsbrough's idea very instructive, as a tool for physical understanding of large-scale oceanic circulation, and as an example of how reasonable questions that were not asked of it at the time failed to lead to opportunities that were finally not realized until the same questions came up again independently much later. Thus I would like, this afternoon, to introduce Goldsbrough's 1933 theory of the circulation of the North Atlantic, and with the benefit of hindsight, indicate how with slight extensions it might have anticipated the work of following years.

I have sketched here (in Fig. 1) the large Goldsbrough anticyclonic gyre that fills the ocean between two meridional coastal barriers and the

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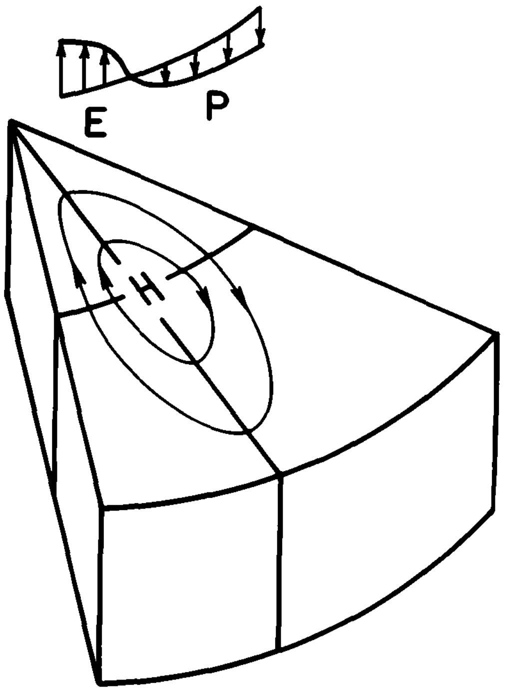

Fig. 1. The Goldsbrough gyre driven by evaporation–precipitation and presented by him in 1933 as a model of the north Atlantic.

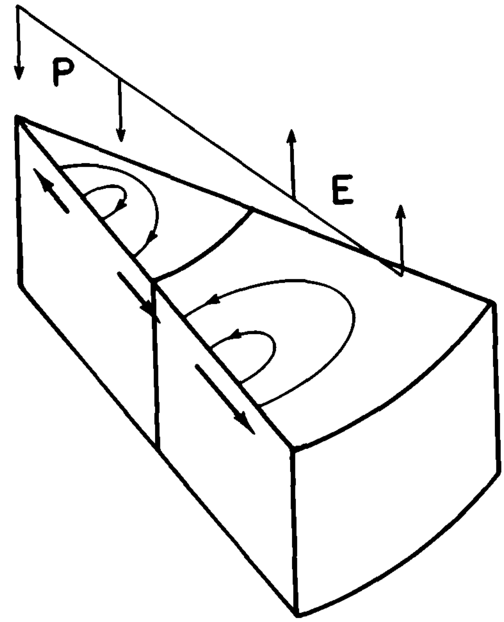

Fig. 2. Effects of using a more realistic distribution of evaporation–precipitation, and including western boundary currents.

equator. The point of the pie is the North Pole, the circular arc is part of the equator and the whole thing flattened out as shown. It is a single homogeneous layer in an ocean of constant depth. The flow is supposed to be inviscid, slow, hydrostatic, geostrophic, and steady. The contours are isobars, due to a very small deviation from level of the ocean's free surface—so small as to have negligible effect on the overall thickness of the ocean: perhaps a range of 2 cm elevation as compared to 4000 m depth. As Ekman knew, a steady geostrophic gyre on this scale cannot support itself. The horizontal currents must flow between and parallel to the isobars, but at low latitudes the Coriolis parameter is smaller, and more mass transport is required for the Coriolis force to balance the horizontal pressure gradients. Without external driving of some sort, the accumulating shortage of transport on the eastern side of this gyre would cause the surface level and pressure to drop; the opposite would happen on the western side, and there would appear a set of westward moving waves—what we call Rossby waves today after Carl Gustav Rossby who first appreciated the physical implications of change of latitude in geostrophy and called it the planetary divergence. I should mention that Goldsbrough himself had previously found such slow waves in a study of free oscillations in this same basin, but did not, it seems, fully recognize their physical importance. Goldsbrough made the gyre stationary by introducing a source of mass at the surface: a distribution of evaporation and precipitation so arranged as to balance the planetary divergence of mass transport everywhere. Because along any latitude, the geostrophic transport in this steady model must sum to zero, the applied evaporation–precipitation must also sum to zero. This is a very unrealistic restriction geophysically, and in any case the amount of current that would be driven by evaporation/precipitation in the real ocean is an order of magnitude less than that in the real North Atlantic.

If Goldsbrough had asked the question "what happens when the evaporation–precipitation summed along a latitude circle does not vanish, he could not have got balance in this simple geostrophic gyre (Fig. 2). He might then have decided to introduce viscosity and Ekman bottom frictional layers, and the existence of western boundary currents would have been revealed. We know from his second current paper that he could easily deal with the mathematical technicalities of Ekman layers on a rotating globe without meridional boundaries, and by benefit of hindsight it would seem he could have easily made this step. If he then replaced his unrealistic east–west distribution of evaporation/precipitation with a more realistic north–south one, with evaporation over the subpolar region and precipitation over the subtropics, the direction of flow of his model would have been in the wrong direction, a strong indication that evaporation and precipitation do not account for the main features of the North Atlantic circulation. It would have been a logical next step to replace the evaporation/precipitation driving by the much stronger vertical pumping from a convergent wind-driven surface Ekman layer, and he would have anticipated the work of the next 18 years. Most of the ideas which Arnold Arons and I wrote about the deep circulation in the 1960s are rooted in modification of Goldsbrough work.

The average winds are from the west in mid-latitudes, but in low latitudes the Tradewinds come from the northeast. From Ekman's 1902 theory of wind-drift of surface currents, we expect the drift to be to the right of the wind. Practical people always have difficulty in believing that the winds can cause ocean currents. After all, the Trades are scarcely strong enough to blow dry leaves over a well-trimmed lawn. It is not obvious by everyday intuition that they can drive the ocean to depths of hundreds of meters. Ekman himself tried to measure his spiral of 1902 from the research vessel *Michael Sars* in the 1930s, using a current meter of his own design. His measured results, published 50 years after the appearance of his theory, were disappointingly inconclusive. Only recently have oceanographers been able to verify the existence of the weak Ekman drifts beneath the masking noise due to surface waves and ship motion. In 1977, off California, Robert Weller, using delicate vector averaging current meters from the stable platform called FLIP, obtained what I think to be the most convincing verification of Ekman drift.

In a configuration of winds like those which actually prevail, therefore, there is a convergence of Ekman drift in the subtropics and a divergence of Ekman drift in the subpolar regions. These lateral flows are confined to the upper few hundred meters at most, and where they converge the only way for the water to escape without perpetually accumulating into an evergrowing mountain, is virtually downwards. Therefore a convergent surface Ekman layer can play a role similar to Goldsbrough's precipitation. It can supply water to a deeper geostrophic flow to make up for the planetary divergence. Moreover, from estimates of the magnitude of windstress it appears that the Ekman layer pumps down ten times more water than normal precipitation rates can. The idea of Ekman pumping seems to have been first explicitly described by Charney and Eliassen, in 1949. In Fig. 3 we show such a regime. The first panel shows the winds, and how they vary with latitude. The direction of Ekman drift to the right of the winds is shown in the next panel. The vertical pumping velocities driven by the converging Ekman drifts are shown in the next panel. And finally the pattern of geostrophic flow is shown in the last panel. The Ekman pumping does not sum up to zero along latitude circles, the amount of interior flow as shown is independent of longitude, and consequently boundary currents must be fitted to the western side to preserve conservation of water in the model. They are a rudimentary form of the Gulf Stream and Labrador Current.

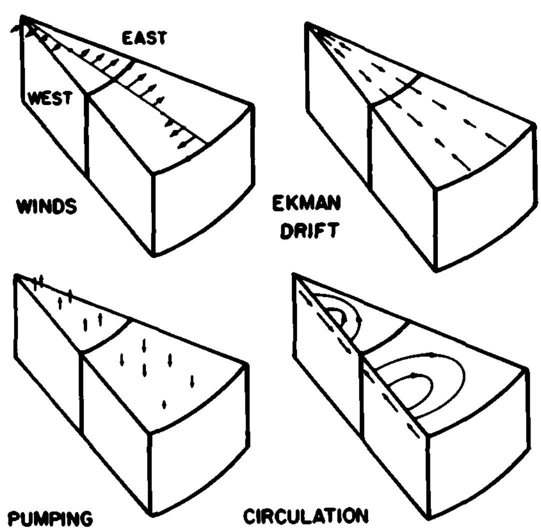

Fig. 3. Four panels showing winds, Ekman drift, vertical pumping, and horizontal flow patterns in a more realistic rendering of the North Atlantic circulation.

If we can learn why Goldsbrough did not make these steps we can perhaps appreciate and understand why and how today we often are in a similar position, poised uncertainly at the threshold of new discovery, and yet unable, somehow, to make the next steps (which will be obvious to a later generation). Certainly Goldsbrough at the University of Durham was isolated intellectually in the sense that there was virtually no one to whom he could talk who had an interest in what he was thinking about: no one to ask useful stimulating questions, or to suggest alternative approaches to puzzles. His special field of interest, classical tidal theory, was the preserve of a tiny formal minded group of applied mathematicians, who did not share the strong sense of physical priority which characterized the meteorologists and oceanographers of the Scandinavian schools of Bjerknes, Ekman, Sverdrup and Rossby. And furthermore, Goldsbrough was distracted by his delight in academic administration, and had just been elected Dean of the Faculty, as Fate would have it.

We have here an interesting instance of a case in which the mathematics led to the very brink of some interesting physical ideas, but because of the structure of classical mathematical methods the ideas did not get sorted out into a form that would be useful for the further progress. This did occur later when, driven by a strong heuristic physical intuition, Rossby was skillful enough to use the planetary divergence of geostrophic flow as the keystone of his atmospheric waves theory, and eight years later Sverdrup and I, neither of us quite understanding what we were doing, but motivated by questions about observed features of the ocean, traversed some of the same ground that Goldsbrough had, and produced rudimentary theories of the horizontal ocean circulation in closed ocean basins, driven by the winds. Also about this time in the late 1940s the mathematician George Carrier was inventing the singular perturbation method, for breaking troublesome differential equations into various parts each with fewer terms so that they could be solved separately and then pieced together afterwards. Carrier's method led to the identification of many physically different boundary layers, and because it was rather rigorous, rapidly displaced much of the heuristic *ad hoc* assumptions that were previously so embarrassing to explain, and, even more importantly, led to the identification of many different new geophysical regimes and boundary layers whose physical characteristics could be interpreted, and whose interplay understood. Suddenly many formal hydrodynamical problems became solvable, their mechanics transparent, and a new discipline called geophysical fluid dynamics evolved. During the ensuing land-rush the most readily-accessible flat fertile fields were quickly settled by analytical methods, but over the horizon there was still plenty of rough terrain uncultivatable by singular perturbations, into which a few individuals began to prospect. I am thinking primarily of rocky grounds such as the role of the turbulent mesoscale eddy field in the ocean, field programs like the Mid-Ocean Dynamics Experiment in 1970–73, and the extensive numerical experimentation with computer controlled ocean models. The results of present-day numerical ocean modelling can be as murky as attempts to generalize upon the elemental Navier–Stokes equations themselves, some of the simplifications as difficult to justify as those made in some of the early analytical models, and the computing power of even the largest machines is scarcely adequate to deal with all the relevant oceanic scales simultaneously. Nevertheless one has confidence that by assiduous application of the mind, human ingenuity will eventually harness these numerical monsters, and learn to drive them with apparently effortless skill. And then a new cultivation of the rough terrain will ensue.

On the other hand, even on the flat land of ordinary analysis, the settlement is far from complete, as anyone who can take his eye off the travelled road for a moment will see. As an example, let me offer the problem of the vertical density stratification of the sea—a subject today very much in flux.

The models discussed so far were those for a homogeneous ocean, or in terms of a vertically integrated one. The real ocean is vertically stratified, and in the different density layers the current velocities are different. Fig. 4 shows a vertical sounding through both the air and ocean near Puerto Rico. The temperature is shown at the left, the amount of fresh water in grams per kilogram on the right. Much of the vertical structure *in the air* can be explained through the local vertical mechanism of Tradewind cumulus convection. However the distribution of fresh water *in the ocean* has maxima and minima which are not

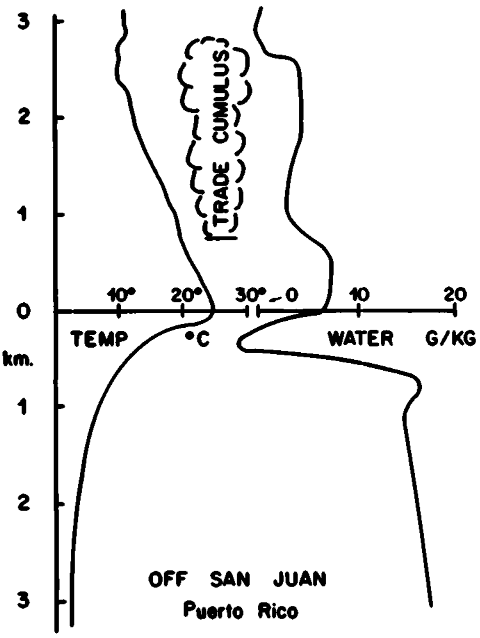

Fig. 4. A vertical sounding off Puerto Rico in both air and sea, showing temperature and concentration of fresh water in g/kg. Most features could be explained by local vertical processes except the reversals of fresh water content in the sea, which seem to require horizontal advection from far away.

easily explained by locally vertical processes and suggests advection from geographically remote regions. Efforts to account for the vertical structure *of the ocean* were made by Welander and Robinson and me in terms of similarity transformations, which reduced the non-linear density advection equation to a separable form, with all the complexity concentrated into the vertical differential equation. The thermocline models of 1958–71 were very restricted in form, were for only the interior of single gyres, and had that same disturbing ethereal quality of Goldsbrough which calls for physical interpretation, coupling with other regions and boundary layers, and generalization to accommodate independent variations in boundary conditions on Ekman pumping and surface density. There was something paralyzing about them. Of course in those years there was a great deal of other activity in other dynamical circulation problems, but these similarity theories of the vertical structure gave the impression of embarrassing ghosts at the party. They were, in a sense, frigid—unfruitful. They gave very little information about the relative roles of advection, vertical and horizontal mixing in the mid-ocean. Their boundary conditions cannot be varied in interesting ways to pursue questions about climatic variation. Some escape from the impasse had to be discovered. Numerical models were not much help: like vegetable soup, they have too many ingredients to reveal which one imparts the flavor.

Recently oceanographers have been able to find an alternate interpretative pathway through this problem area. I think that the point of entry to this new pathway was Adrian Gill's (1975) demonstration that in the spin-up of a stratified ocean from rest, the final Sverdrup solution converges to a surface catastrophe. In Gill's spin-up model, all the ocean's transport eventually was concentrated in the uppermost density layer, a situation contrary to the facts. There is nothing like a clear paradox to break the grip of preconceptions. When thinking about the vertical stratification, it is convenient to think again in terms of the old Goldsbrough framework. We may think of the real ocean subtropical gyre as being modelled by that eastern portion of the figure under the region of precipitation (which really is to be thought of as the Ekman pumping between the Trades and Westerlies), and ignore the western portion of the solution except to say it ought to be replaced by a western boundary current. Thus, if the circulation is confined to the uppermost layer of a density layered system the surface elevation and pressure distribution in that layer can be identical to that shown in Fig. 1, and we only need add that by hydrostasy, and assumption of no motion in the lower layers, the interface boundary at the bottom of the upper layer is of the same form, but greatly amplified in amplitude—being deepest at the center of high pressure. But these large variations in depth of as much as 1 kilometer cannot offset the planetary divergence because the geostrophic flow is parallel to the depth contours. Any deeper interfaces remain level, by Gill's catastrophe.

A north–south section through the middle of this Goldsbrough gyre (the western edge of the precipitation region, and of that portion of the solution which we take to represent the subtropical gyre) is shown in Fig. 5.

I want you to look at two regions in this figure. The region A is the upper layer water, moving

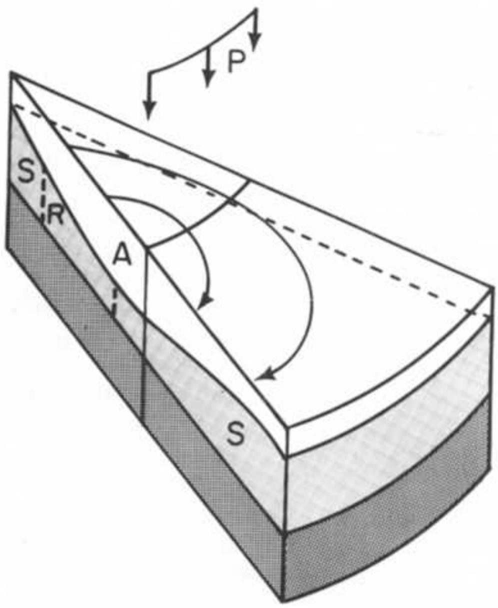

Fig. 5. Geostrophic gyre confined to upper density layer, driven by precipitation at surface (or Ekman pumping) and with the western boundary cut off. Lower interfaces are flat. most of the lower layers are stagnant (S), except possibly the pools recognized by Rhines (R).

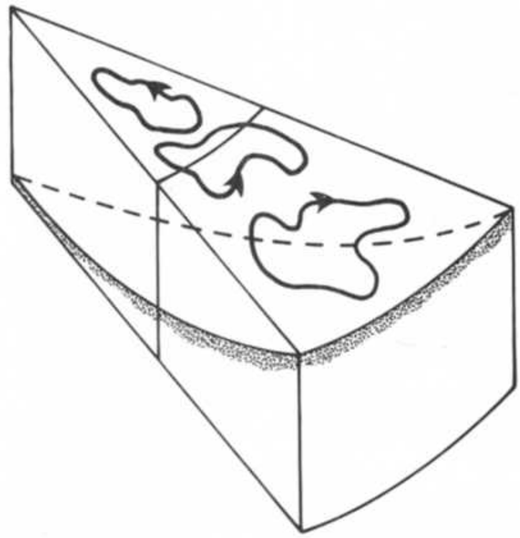

Fig. 6. Degenerate case of a homogeneous ocean with depth proportional to the Coriolis parameter, in which any arbitrary geostrophic flow is non-divergent and can exist without surface pumping.

under the direct driving of the Ekman pumping. Because of large downward deformation of the interface beneath A the thickness of the next layer down is no longer uniform, even though deeper interfaces are all level and the water mostly at rest, or stagnant as indicated by the letter S. The water at the bottom is at rest. The region R caught the eye of Peter Rhines and William Young in 1980 because they recognized it as a place where the planetary divergence of geostrophic flow might be offset by latitudinal change in thickness. Now let us step backwards for a moment to the question of the effect of large variations in thickness in the simpler density homogeneous model. It is known that one way of assuring that in a density homogeneous ocean the mass flow per unit depth between isobars should increase to make up for decrease in Coriolis force is to arrange the depth of the ocean to decrease at a proper rate with latitude as shown in Fig. 6. Then the Goldsbrough gyre could remain steady without external driving, and indeed the problem is degenerate in the sense that any geostrophic gyre of arbitrary shape and amplitude could exist without forcing in an ocean with such a depth law. Some are shown.

Rhines and Young showed that in region R of Fig. 5 we may have an analogously degenerate region of geostrophic circulation—unconstrained by planetary divergence. The particular flow patterns realized, they hypothesize, may be determined by very small interfacial eddy stresses from the top layer and homogenized in potential vorticity by lateral mixing. These regions of deep layer flow—unventilated by steady flow from the surface Ekman pumping—were then offered by Rhines and Young as a way of avoiding the Gill surface catastrophe and achieving some closer representation to real flow in the deeper laws.

This interesting new idea stimulated Luyten, Pedlosky and me to construct another Goldsbrough-type model that would allow water to be pumped down directly into a number of different layers—as it seems to be in the upper thermocline —so that they would all be partly ventilated. We were also motivated by the knowledge that quiet portions of the subtropical gyre exhibit a remarkable rotation of geostrophic velocity with depth—called the beta spiral—and a desire to reproduce it. What we did was to arrange the layer density of the water pumped downward from the Ekman layer so that it is greater at high latitudes than at low latitudes. The outcropping arrangement is shown in Fig. 7. The top layer does not cover the

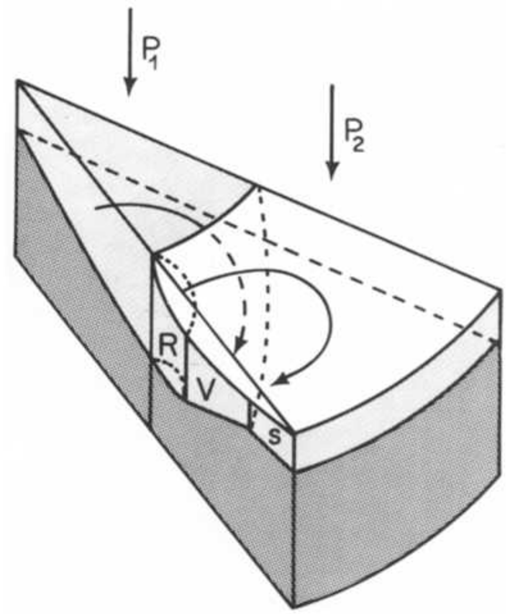

Fig. 7. The ventilated thermocline with the interface breaking the surface at mid-latitude. The recirculating pool R and stagnant region S are separated by a ventilated region V. There is flow in much of both the two upper layers.

entire basin. The figure shows only the eastern half of a Goldsbrough circulation with two moving layers.

As before we are supposed to ignore the western half of the diagram, assuming that in reality it can be replaced by western boundary currents. The "precipitation" in the western half models the Ekman pumping over the subtropical gyre (here covering the full meridional extent of the ocean). The lightest density water is pumped down at low latitudes at rate P_1, the intermediate density water is pumped down at rate P_2. The densest water does not reach the surface anywhere, and may be considered to be at rest. Stagnant regions in the middle layer are indicated by the letter S.

Both upper layer waters are exposed to direct driving from the Ekman pumping P_1 and P_2 and it is clear that these supplies of water will supply the planetary divergences in them where they are exposed at the surface. What is new is that the transport of the midlayer water subducts at the outcropping of the upper density interface, flows underneath the upper layer and that because this layer bulges downward the lower layer has the possibility of changing its thickness in such a way as to flow geostrophically with vanishing planetary divergence. In fact it chooses just those flow patterns which permit this, and there is a ventilated region V where both layers move.

The currents in the lower layer tend to turn southwestward under the deeper parts of the upper layer so that they can most effectively become thinner. Geostrophy requires that the bottom streamlines of the lower layer be level, and hence the only way this layer can get thinner is to duck under low lying portions of the overhead layer. This skewing of the two interfaces means that the direction of the currents is different from one layer to the other. Thus the ventilated model replicates the real beta-spiral observed in the North Atlantic ocean.

The direction of turning of this spiral is immediately apparent from the following consideration. Fig. 8 lets us imagine that we are viewing a vertical cross-section of the thermocline perpendicular and upstream to the direction of the deep layer flow in the northern hemisphere. Since this is geostrophic the depth of the lower interface I_2, which determines the direction of deep layer flow does not change normal to the section. On the other hand, due to the southward component of flow, the thickness of the lower layer must increase upstream to offset the planetary divergence and this can only occur by a shallowing upstream of the upper interface I_1 to I_1'. This means that the upper layer has a

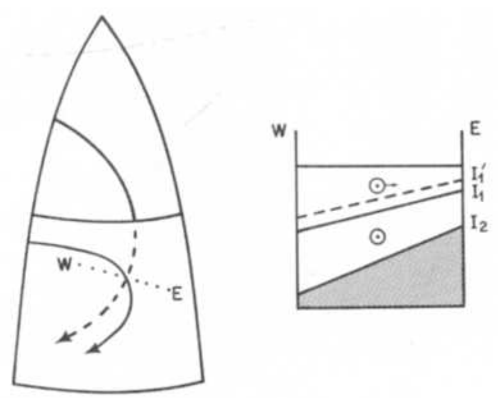

Fig. 8. Horizontal plan (on left) of two flow lines, one in each layer. The submerged portion of the lower layer's flow line is dashed. A vertical section WE (on right) is shown to illustrate why the geostrophic velocities rotate with depth (the beta-spiral).

tangential component of geostrophic flow to the left of the lower layer. In a model with many layers this emerges as the beta-spiral.

This curious clockwise geostrophic spiral extends through the subtropical thermocline in the quiet eddy-free eastern portion of the subtropical gyre of the North Atlantic. Since 1978 David Behringer and I have conducted five separate hydrographic surveys in this region to verify the existence of the spiral and its degree of steadiness. I believe that the existence of the beta-spiral is now definitely established, but am uncertain about how useful it may be as a diagnostic tool in the sense in which Friedrich Schott and I once thought it might be.

In addition to the ventilated region V in the lower layer there are also stagnant regions S that are inaccessible to the surface forcing, and if P_1 is greater than P_2, there may be a Rhines–Young homogenized region R as well. This new picture of the oceanic thermocline may be regarded as an extension of the old Goldsbrough model, and one can only wonder why such simple extensions are so long delayed, and what these delays teach us of the nature of progress of geophysical knowledge. I think that it is one more example of how many really simple but useful ideas there are still to be found, and that it should serve as an encouragement for younger people—who sometimes think that everything easy has already been done. And the story has by no means come to an end.

Now that we have a simple model of the ventilated thermocline, it is clear that we should try to fit a western boundary current to it, to close the model of the subtropical gyre. In the spirit of George Morgan and Jules Charney, who fitted inertial boundary currents to a model in which only one upper layer moved, we might expect to be able to fit an inertial boundary current to a model with two or more moving layers. Here we encounter still another paradox. Robert Blandford once tried to make such a fit to a two moving layer interior in 1963. He encountered a puzzle: the solution was continuous except at one point, at which there was such a big jump it could not be easily bridged, he thought, except by introducing some higher order dynamics. This puzzle was published in 1965, but, like Goldsbrough's paper, attracted little attention. Apparently no one recognized what it signified, and the only result was that investigators henceforth avoided the subject of two moving layer boundary currents. However we have now been forced to face the puzzle directly, because of the need to fit the new ventilated thermocline theory to a western boundary current, and we have now discovered that we can get continuous boundary currents if we relax the degree of specification in the interior. In other words, as can happen in many familiar hydraulic regimes, the inertial boundary current of Blandford was overspecified. The implication is that we must relax some of the conditions specified in the ventilated thermocline theory: for example, the surface density condition, by fitting it to an active mixed layer model, or perhaps by introducing an eastern boundary current, or by possible free modes of circulation in deeper layers. Our picture is not completed yet, and we have far to go to construct a satisfactory model of this latest Goldsbrough variation.

When I was 26-years-old I visited the Imperial College in London for a term. Sverdrup had just published his important 1947 paper on the relation of curl of the wind-stress to ocean transport—a clear case of a Goldsbrough variation—but since he published it in the somewhat obscure *Proceedings of the U.S. National Academy of Sciences*, I did not know of it. I had just completed my Gulf Stream model of westward intensification, and mailed a hectographed copy of the manuscript to Goldsbrough together with a request for permission to visit him. But he was old—older than I am now—and he told me not to come. Perhaps by then he was weary of circulation problems, or he did not like the elementary form of my model. Sad to say, I never met him. From what I have heard, he was a good and cheerful man, devoted to his students, an earnest Christian, and organist at his church. By this talk, perhaps, I can acknowledge an old and continuing debt, and to convince younger people to play with simple ideas and to believe that the pencil is mightier than the computer.

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