

(1958), Cochrane (1958), and Pollak (1958) still stand, but I believe that their classes are too large. One can see that by merging the present classes into divisions as large as 0.5‰ of salinity, as Cochrane and Pollak have done, or 1.0‰, as Montgomery has done, the high-volume warm prongs (figure 2.9) would be artificially blurred and merged with one another.

One or two colleagues have asked whether I could not use a logarithmic volume scale in such presentations as figures 2.2-2.5 so that the warm water masses (if included) could be made to stand out more clearly, but one of the principal virtues of the volumetric T - S diagram is that it displays the relative abundances of the water masses as they actually exist. The *concentration* of water in the most abundant North Pacific class exceeds that in the warm-water prongs (shown in figure 2.9) by a ratio of about 25,973 to 10 or less. This is analogous to comparing the elevation of Mount Everest to that of Water Street, Woods Hole, near the original building of the Woods Hole Oceanographic Institution. In fact, if we were able to sample and measure salinity more perfectly, the apparent elevations shown in the deep water in figure 2.8 would probably be even higher.

The feelings I have about the census are compounded equally of fascination and frustration. The frustration is the result of the decrease in the rate of acquisition of new high-quality data. This decrease is due in part to the trends in modern physical oceanography in which the dramatic improvements in direct current measurements have understandably taken priority over routine measurements of water properties on a large scale. It is also clear that there is a long delay (as much as 5 years) between the time hydrographic data are obtained at sea and the time these data become available on tape from the National Oceanographic Data Center (in part because some investigators take a long time to turn their data in to the Data Center). I have been reluctant to obtain new data informally, from friendly colleagues, however, because I do not think that the Data Center should be bypassed at present; its function would be impaired if data were only exchanged between a cabal of skilled observers.

The fascination results from the precise but peculiar way in which the water masses of the oceans are arranged—particularly the deep water masses that make up the greater part of the oceans. Why, for instance, are the big, exclusive North Pacific classes fresher than existing circumpolar and South Pacific waters? Are they fossil water masses that were formed in some past millennium when the oceans were somewhat fresher, or are they still undergoing a change toward the fresher as the result of slow vertical mixing (across density surfaces) with the still fresher water that lies above them at the present time? I do not think that we can supply answers to such questions at present, and an-

swers will not be available even in the future without painstaking observations. There are indications this style of observations may be coming back into vogue. The authorless Scripps data report of the INDOPAC expedition (Scripps Institution of Oceanography Reference: 78-21) is an excellent example. It should be worthwhile to reactivate this census (which was closed as of June 1977) when more high-quality data of this kind are available from NODC, and I shall probably propose to do so at some time in the future.

2.4 The Formation of Water Masses

There is only one hypothesis about water-mass formation that is universally agreed upon, that is, that the cold, dense water that fills the great ocean basins has been formed at high latitudes. The manner in which the thermocline-halocline is formed is under dispute, and there are almost as many notions of the *rate* at which all the various water masses are formed as there are investigators.

Given the extraordinary regularity of the T - S curves that are found in much of the oceans, it is natural to assume that these curves are the result of vertical mixing between two end water masses. Very simply stated, this assumption implies that the bottom water (as all agree) has been formed at high latitudes, that the surface water at middle and low latitudes has received its T - S characteristics from the atmosphere by the uneven processes of evaporation and heating, and that the remainder of the water column is a mixture of surface and bottom water. Wüst (1935) clearly recognized that this was an oversimplification, and his use of the "core-layer" method reflects his conviction that different water masses can be traced to a small number of more-or-less point sources at the sea surface over a wide range of latitude.

The notion that *all* the thermocline water masses can be traced to the sea surface is generally attributed to Iselin (1939a). He constructed a T - S diagram from winter observations at the surface of the western North Atlantic, and found that it corresponded closely to the T - S diagram obtained from a typical hydrographic station in that ocean. It is worth noting that Wüst (1935, p. 3) anticipated Iselin (in the South Atlantic) by 4 years. He wrote, "The vertical structure of the Subantarctic Intermediate Water, with its horizontal spreading at depths, is analogous to a vertical figure of the horizontal arrangement of temperature and salinity at the surface of the formation region." Wüst did not dwell on this subject further, and it is clear that he regarded core layers as more important as indices of ocean circulation.

In his 1939a paper, Iselin stressed "lateral mixing" as responsible for the T - S curve in the western North Atlantic. Sverdrup, in chapter XV of *The Oceans* (Sver-

drup, Johnson, and Fleming, 1942) amplified Iselin's concept; he suggested that "subtropical convergences" were the dominant source of the waters in the thermocline-halocline. In these convergences, according to Sverdrup, surface water sinks, in late winter, over a wide range of latitude. He compared late-winter, sea-surface T - S points to the T - S curves obtained from subsurface hydrographic data and found a close correspondence in the south Indian Ocean, the eastern and western South Pacific, and the western North Pacific, just as Iselin (1939a) had done for the North Atlantic.

In this type of water-mass formation, very little change takes place in each individual water type—when 10°C water outcrops at the sea surface, it sinks again at 10°C or nearly so, not at 4°C . In vertical-mixing models, the water types are constantly changing. In the most violent of these, Stommel (1958) held that bottom water was produced at two sinks, the Weddell Sea in the south and the Irminger Sea in the north. He theorized that bottom water was produced constantly and moved upward through the thermocline. The thermocline in this theory was maintained by a downward diffusion of heat that balanced the upward advection of cold bottom water. Later, Stommel and Arons (1960b) produced a simple schematic model in which $20 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ was formed in each sink and flowed equatorward in the form of deep western boundary currents. Subsequently, this water upwelled through the thermocline (as outlined in Stommel's earlier paper) with an upward velocity of about 4 m yr^{-1} .

Cooper (1955a) refocused attention on the Norwegian Sea overflows as a source of the North Atlantic Deep Water, and this paper touched off a series of investigations into these overflows, using newly developed methods of direct current measurement such as the Swallow float (Swallow, 1955). I (1970) have summarized some of the earlier investigations (see also chapter 1, this volume). In essence, it was evident that if cold bottom water does in fact flow across the sills of the Norwegian Sea into the deep Atlantic Ocean, relatively warm surface water must be drawn into the Norwegian Sea to replace it. It was possible to write water and heat budgets for the Norwegian Sea, the cold outflows ($9 \times 10^6 \text{ m}^3 \text{ s}^{-1}$) transported 63×10^{12} less calories per second than the warm inflows (also $9 \times 10^6 \text{ m}^3 \text{ s}^{-1}$). This excess heat must go to the atmosphere, and the calculated heat loss from the oceanographic data, $75 \text{ kcal cm}^{-2} \text{ yr}^{-1}$, corresponded pretty well with Budyko's (1963) calculated heat loss from meteorological data—about $60 \text{ kcal cm}^{-2} \text{ yr}^{-1}$.

Of the $9 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ of cold water flowing out of the Norwegian Sea, $3 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ is embodied in the shallow, fresh East Greenland Current. The remaining $6 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ is dense overflow water; the overflows entrain a further $4 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ of Atlantic water at their sills and a total of $10 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ flows southward

along the western side of the North Atlantic as a narrow deep western boundary current. The fact that this current originates in the Norwegian Sea and not in the Irminger Sea does not affect the theory, but it is naturally of vital interest to the descriptive oceanographer.

Thus the formation of North Atlantic Deep Water follows a classic pattern—cold, dense water is formed at high latitudes, and warm surface water flows poleward to replace it. In this pattern, the process of water-mass formation changes the water characteristics radically. Much of the replacement water must come from the tropical South Atlantic since the North Atlantic Deep Water flows into the South Atlantic at a rate of $9 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ according to Sverdrup et al. (1942), or $7 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ according to my box model (Worthington, 1976, figure 11). As this tropical surface water flows north, it gradually transfers heat to the atmosphere and undergoes enormous changes in its T - S characteristics before it finally becomes North Atlantic Deep Water.

This pattern is clearly consistent with the Stommel (1958) theory since upwelling of the North Atlantic Deep Water must take place—provided that the volume of the North Atlantic Deep Water remains constant. One could conceive of a water mass that refuses to mix with its neighbors—in this case the Antarctic Intermediate Water, the Circumpolar Deep Water, and the Antarctic Bottom Water—but, instead, pushes them bodily aside, preserving the purity of its original T - S characteristics and increasing its own territory. In the case of the North Atlantic Deep Water, it seems unlikely that such behavior has taken place. The area of the Atlantic Ocean is $82.4 \times 10^6 \text{ km}^2$. The North Atlantic Deep Water is found throughout two-thirds of this area. Its volume is $89 \times 10^6 \text{ km}^3$ — $62 \times 10^6 \text{ km}^3$ in the North Atlantic according to Wright and Worthington (1970) by their definition, and the remaining $27 \times 10^6 \text{ km}^3$ in the South Atlantic according to this census (same definition). The mean thickness of the North Atlantic Deep Water is thus about 1500 m. The width of the South Atlantic at the southern limit of more or less pure North Atlantic Deep Water (35°S) is 6400 km. To advance 1° of latitude (110 km) southward, the North Atlantic Deep Water would have to increase in volume by $1.06 \times 10^6 \text{ km}^3$. If $7 \times 10^6 \text{ m}^3 \text{ s}^{-1}$ cross the equator, as I have suggested, this flux should result in an advance of 1° of latitude every 4.5 years if no upwelling were taking place. Since the *Meteor* expedition was made, over 50 years ago, the southern limit of the North Atlantic Deep Water would have had to advance 11° of latitude; it plainly has not done so. We should, however, be cautious even in crude calculations like these, since our numbers for the amount of water formed may not be even approximately correct. For example, most of the measurements made in the deep, dense overflows from the Norwegian Sea were made in the 1960s—a cold decade in the northern North Atlan-

tic relative to the decades that preceded it (Colebrook, 1976). This could easily have resulted in an overestimation of the climatic mean flow of the dense overflows.

We should probably not regard the Iselin (1939a) theory of the thermocline-halocline formation as exclusive of the Stommel (1958) theory, or vice-versa. The evidence that water sinks along density surfaces that have outcropped at the sea surface seems inescapable, yet there is equally strong evidence that upwelling of deep water through the thermocline must take place, at least in the Atlantic.

Recently (Worthington, 1977a), I have questioned the Stommel-Arons (1960b) model as it applies to the Pacific Ocean. The argument is a simple one; the deep water in the Pacific contains not less than 7 g of dissolved silicon per cubic meter, and the upper layers are very nearly silicon-free. If the deep water (formed in the Weddell Sea) upwells through the thermocline throughout the Pacific and flows southward in the upper layers, two processes must take place. First, the dissolved silicon must be removed from the deep water before it can return southward to the formation area, free of silicon. Second, an equal, or nearly equal, amount of silicon must be replaced before the surface water sinks below 2000 m in the Weddell Sea. The removal and replacement rate necessary to accommodate the model can be calculated to be about 30×10^{14} g of silicon per year. I believe that to ask the ocean to perform feats like this is unreasonable and that the most logical conclusion to be drawn is that the deep waters in the Pacific are not being renewed at the present time. One could guess that they might not be renewed until the time when the rate of accumulation of ice on the Antarctic continent begins to exceed that of ablation and runoff.

The *surface* waters around Antarctica are fairly rich in silicon, as are those of the northern North Pacific. Reid (1973b) has attributed this situation in the North Pacific to upwelling, and this also is the most reasonable explanation for the state of affairs around Antarctica. Nutrient-rich upwelled water, augmented by runoff from Antarctica, flows equatorward until the amount of sunlight is sufficient to cause photosynthesis; then the silicon (and other nutrients) is immediately used for plant growth—consistent with the rich biota in the zone around Antarctica and with the high rate of accumulation of siliceous sediments beneath this zone (Lisitzin, 1972).

By comparison, the North Atlantic surface waters (except in the coastal zones) are a desert because they consist of nutrient-stripped water from the middle and low latitudes, and Metcalf (1969) has shown that newly formed North Atlantic Deep Water is also silicon poor. The subject clearly invites further investigation, but, as a first approximation, one might suppose that in

silicon-poor oceans such as the Atlantic, the Stommel (1958) theory holds true, but in silicon-rich oceans, such as the Pacific and Indian Oceans, the process of deep-water formation has been suspended and the Stommel mechanism awaits application for the next cold, climatic variation in the southern hemisphere. If such a cold variation should take place, and if the vast store of nutrients that has (I believe) been accumulating in the deep Pacific through the years should be brought up into the sunlight, the results, in terms of biological productivity, might be staggering.

If this hypothesis is even approximately correct, we are faced with the problem of how the thermocline-halocline is maintained in the Pacific and Indian Oceans. A clue to this has been provided by McCartney (1977). He has identified a family of water types in the South Atlantic, South Pacific, and Indian Oceans that he has termed Subantarctic Mode Water. These layers appear to originate by deep convective overturning immediately equatorward of the Antarctic Circumpolar Current. They can be identified at great distances from their formation regions by thermostads with *T-S* characteristics identical to those of the deep overturned waters, and usually by an oxygen maximum in these thermostads. These layers are strongly reminiscent of the 18°-Water thermostad formed south of the Gulf Stream (Worthington, 1959) and of the Subtropical Mode Water (Masuzawa, 1969) south of the Kuroshio and the Kuroshio Extension. They differ from 18° Water and the Subtropical Mode Water in that they are found at much greater distances (up to 3000 km) from their points of origin and that they occur over a wider range of temperature (14–4°C).

Although the matter is still under investigation (McCartney, 1980), it appears that most of the southern hemisphere thermocline waters (in terms of volume) originate in these thermostadal layers by means of convective overturn. Certainly, all the southern, high-volume classes between 4 and 14°C that appear at the base of the central prong in figure 2.9 can be traced to the deep mixed layers described by McCartney (1977).

Water-mass formation is a complicated process. There seems to have been no major hypothesis on the subject that can be entirely accepted or entirely rejected. The earth's geography and the atmospheric circulation encourage the production of a wild variety of water masses, and evidently these water masses are formed in a number of different ways. I have attempted to describe some of these. The study of water-mass formation is intimately tied to the study of the general ocean circulation. The most promising method for the future will be to combine these two studies by means of direct current measurements and the rigorous observation of the distribution of variables in the oceans and of the atmospheric processes that bring about these distributions.