Oceanographic Phenomena in the Arctic Basin

We wish to take exception to an interpretation of oceanographic phenomena in the arctic basin based on measurements for tritium content as presented by Giletti and Kulp [*Science* **129**, 901 (1959)]. On the basis of the agreement between the tritium value of one sample from 400 m (0.9 ± 0.1 tritium unit) and the value for average Atlantic surface water prior to Project Castle (1.0 ± 0.1 TU), Giletti and Kulp have concluded that the sample must be recent, perhaps less than 2 years from the surface. Then, by a series of assumptions, they suggest that this water came from the local continental shelf, and this suggestion is magnified in their conclusions to "(iv) a new source of the deeper water of the Arctic Ocean was found to be in the Canadian archipelago. . . ."

It is generally meaningless to try to interpret vertical relationships of water masses without considering the density structure. In order for water to sink with a minimum of mixing from the surface to depth, whether this sinking occurs more or less vertically as in a convection cell, obliquely as along a tilted isopycnic surface typical of the major oceanic convergences, or following the slope of the bottom in association with cooling and freezing processes as in the Weddell Sea, the water when it leaves the surface must be of a density equal to or greater than the ambient water through which (or under which) it sinks. Ice formed in the freezing of sea water generally contains less salt than the sea water from which it was frozen. Accordingly, as continued freezing occurs, the ambient water is enriched in salt.

For sea water within the commonly occurring range of salinity and temperature, the density at atmospheric pressure, of which sigma-*t* (density at atmospheric pressure − 1) × 10^3 is a common measure, can be increased by decreasing the temperature (surface cooling) or by increasing the salinity, which in high latitudes is associated with freezing. Once ice forms, both these surface processes occur simultaneously, with further loss of heat, and this is the case in the Arctic Ocean and Canadian archipelago in the cooling cycle of the year. If compression due to pressure is disregarded, water, once it has left the surface, can increase in density only through mixing with ambient water of the same (second-order effect) or greater density. Although mixing with sublying water of greater density can be effected by wind and tide, or by the momentum of the moving stream, local density flow as apparently envisaged by Giletti and Kulp would result in water formed at the local sea water–ice interface sinking along the bottom and reaching the 400-m level with an equal or somewhat lesser sigma-*t* than the water had when it left the surface.

The density of the water at 400 m [0.35°C, 34.89 $^0/_{00}$ (note that salinity is given in parts per mill and not percent, as in the article by Giletti and Kulp; this was presumably a typographical error)] expressed as sigma-*t* is 28.02; that at 10 and 80 m is 25.47 and 26.08, respectively. The salinity of water which would have a sigma-*t* of 28.02 at its freezing point is 34.77 per mill, a value much higher than any ever reliably reported for surface waters in the area under consideration. The values of 31.64 and 32.40 per mill at 10 and 80 m, respectively, as given by the authors, are in agreement with other reported values for nearby parts of the arctic, and this is about the range to be expected at the corresponding depths along the fringe of the archipelago. It may be assumed that in peripheral arctic areas of restricted circulation the salinity of the water may be increased locally as a result of freezing, but such an explanation would hardly account for the great quantity of water of high salinity found widespread in the Arctic Ocean at depths greater than about 200 m.

Further, if the salinity was increased by freezing in the Canadian archipelago and the water then moved down the continental slope without mixing, it would have, at the depths being considered, a temperature of about −1.9°C, corresponding to the freezing point of water of salinity 34.89 per mill, instead of +0.35°C as reported. The average salinity of the water in the upper 100 m in the Canadian archipelago is considerably less than 33 per mill, and the average temperature of this water column is considerably less than 0°C (*1*). Even if enough local freezing should occur to raise the salinity from less than 33 to more than 34.89 per mill, there is no local source of heat in the archipelago waters or on the local Arctic shelf that could raise the temperature of locally sinking waters from the freezing point to +0.35°C.

Giletti and Kulp make special reference to conditions at station 11. The bottom water at this station cannot be distinguished from other water in the area through tritium concentration, as apparently no tritium analyses were made for this particular water. Moreover, there is no indication of the number of temperature measurements used to establish the temperature curve shown in their Fig. 1 (the curves in this figure do not agree with the reference cited). Since salinity values are lacking as well, the possibility that malfunctioning equipment led to an erroneous temperature picture cannot be ruled out. However, let us assume that the value of 0.1°C for the bottom water, as read from the curve given by Giletti and Kulp, is correct, and that this water left the surface at −1.9°C and mixed, in sinking along the archipelago slope, with water of Atlantic origin having a temperature of +0.5°C. The resulting mixture, on reaching the bottom at the indicated 480 m, would consist of about 80 percent Atlantic water and 20 percent locally formed water. The possibility that this or a similar sequence of events occurred cannot be ruled out at this time, but adequate temperature and salinity data would be required to prove it. At best, tritium analyses will afford only supporting information.

The view commonly held is that water such as that which appears at 400 m in the portion of the arctic in question comes from the Atlantic Ocean via the Norwegian Sea, suffering some loss of temperature and some decrease in salinity by subsurface mixing with ambient water en route (*2*). This is in line with what we found from examination of unpublished data from a number of oceanographic stations from the arctic basin, in which we considered not only the temperature, salinity, and density but other variables, such as inorganic phosphate. The water in the upper 50 or so meters enters into convective turnover each winter and is an entirely different water mass from that occurring at depths below 200 or 250 m. Between this surface convective layer and the deeper water is a transition zone in which the density increases rapidly with depth below the surface. The sample taken at 80 m lies in the upper portion of this pycnocline, and from our present limited knowledge of winter convection in the area it would seem that the water from this depth may possibly enter into the vertical turnover in some years but not in others. In other words, it may be one or many years away from surface contact. Additional tritium data from this transition layer might help greatly in clarifying the processes involved. This would require samples from various depths from a network of stations. The one sample from 80 m mentioned in the article, although providing food for speculation, of itself tells nothing of the processes.

We suggest that the agreement between the tritium values for the 400-m sample and for average Atlantic water is not at variance with the accepted view of this circulation. We offer the following two alternatives as to why this value is not lower than 0.9 ± 0.1 TU.

1) Giletti and Kulp have assumed that any Atlantic water which sinks to

become deeper basin water circulates cyclonically, requiring a travel time of more than 2 years to reach the sampling location. Actually, the pattern of circulation of the deeper waters has not been definitely established. If the water was formed near Jan Mayen Island (a distinct possibility) and traveled the most direct route to the sampling location in 8 months to 1 year, its rate would be only 2 to 3 mi per day, and it would arrive with a tritium content approximately that of average Atlantic water.

2) The measurements, on which the value of 1.0 TU for the average Atlantic water prior to the spring of 1954 are based, vary from 0.62 to 1.5 ± 0.4 TU (*3*). Let us suppose that the 400-m sample had an original value at the surface of nearer 1.5 than 1.0 TU. Then this sample would have had several years in which to traverse whatever portion of the polar basin lay between and still arrive with a tritium content of 0.9 ± 0.1 TU. In other words, the results of the tritium sampling as reported are too sketchy to serve as the basis of any real conclusions concerning the water circulation of the area.

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References and Notes

1. M. J. Dunbar, *Bull. Fisheries Research Board Can.* 88 (1951).
2. H. U. Sverdrup, M. W. Johnson, R. H. Fleming, *The Oceans* (Prentice-Hall, New York, 1942); *Recent Soviet Scientific Investigations in the North Polar Regions* [a translation by K. R. Whiting of A. F. Laktionov, *Severnyy Polyus* (Sea Transport Publishing, Moscow, 1955), pp. 347-426] (Documentary Research Division, Research Studies Institute, Air University, 1956).
3. B. J. Giletti, F. Bazan, J. L. Kulp, *Trans. Am. Geophys. Union* 39, 807 (1958).

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The careful study by Barnes and Coachman of our paper, which attempted to show the potential application of tritium as a natural tracer in arctic problems, is appreciated. They correctly suggest that the heading over the column giving the salinity data should read "per mill" rather than "percent."

We agree that interpretation cannot be carried too far on the basis of such a limited number of measurements; nevertheless, these data are pertinent to the problem discussed and help to limit speculation. Moreover, a sufficient number of tracer data could, alone, lead to solution of this problem, contrary to the statement of Barnes and Coachman that these can only supply "supporting information." The practical difficulties are many, however, and obtaining these reconnaissance samples was a considerable achievement on the part of Crary and Goldstein.

It was not suggested in our paper that the earlier interpretation of most oceanographers—that Atlantic-derived water occupies the Arctic Ocean at depths greater than 200 m,—be discarded. From the tritium data, however, it appeared that another source might contribute to the total influx. To quote from our article (page 903): "In this case the amount of water is small and affects only the bottom 100 m. This sinking water mixes with the normal Atlantic-derived water. . . ." Since Barnes and Coachman agree that it is possible to have a mixture of 80 percent Atlantic water and 20 percent surface water at the station in question, there would appear to be no real disagreement on this point.

The two explanations which Barnes and Coachman suggest from comparison of the tritium concentrations of the 400-m sample in the arctic and of average North Atlantic surface water do not appear to be substantially better than that suggested by us. Our suggestion is that the high tritium content resulted from the sinking of surface water during times of freezing and the mixing of this water of higher tritium content with the Atlantic-derived water.

Their first hypothesis requires the assumption that the water at 400-m depth north of Ellesmere Island comes from the vicinity of Jan Mayen Island by a direct route at the rate of 2 to 3 mi per day. This would indeed account for the tritium content, within the range of experimental error, but such westward movement around the north coast of Greenland and the Canadian archipelago would be in conflict with the pattern of flow proposed by Worthington (*1*). It will be recalled that the area studied by him included the Chuckchi Sea and Beaufort Sea and the area of collection of the sample in question.

For their second hypothesis, based on the data of Giletti, Bazan, and Kulp (*2*) for North Atlantic surface water, they base their arguments on the sample with the largest error. Owing to the small size of this sample, the counting statistics gave a standard deviation of ± 27 percent, which essentially overlaps the mean of all values. This large error must be compared with the standard deviation of ± 10 percent obtained for each of the remaining eight analyses. Of the better measurements, the highest value is 1.34 ± 0.1 TU. If this value is used, the time allowed for the cyclonic gyre of 3500 mi is 7 years. This is based on comparison with the value observed at the station in question (0.9 TU) and implies a mean flow rate of 1.3 mi/day. This rate is probably less than the true rate, which should be inferred from the *average* North Atlantic value; calculation on this basis gives 4.8 mi/day.

Not only is this a rather high rate but it requires a high tritium concentration throughout the Arctic Sea. It would be reasonable to expect this layer of higher tritium content to be at least 300 m thick. The cosmic-ray tritium production required to maintain this level in a steady state would be at least equal to that required for the entire North Atlantic surface water. If this arctic water all came from the Atlantic, the natural tritium production would have to be almost double the presently assigned value. If anything, the production rates calculated from observations on tritium reservoirs are already too high (when compared with calculations of production based on cosmic-ray reaction rates), and this makes the hypothesis of doubtful validity.

Further study of tritium in selected localities could easily support or disprove either of the suggestions by Barnes and Coachman. Far more sampling would be needed to "prove" any of the hypotheses discussed.

It is concluded that although more information is required to settle the matter, the natural tritium tracer may provide the means for doing so. Indeed, it has already focused attention on the areas where the critical data are likely to be obtained.

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References and Notes

1. L. V. Worthington, *Woods Hole Oceanog. Inst. Tech. Rept. 53-92* (1953); *Trans. Am. Geophys. Union* 34, 543 (1953).
2. B. J. Giletti, F. Bazan, J. L. Kulp, *ibid.* 39, 807 (1958).

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Absorption of Iron from Iron Chelate by Sunflower Roots

Abstract. Roots of decapitated sunflower plants absorbed iron from the ferric chelate of ethylenediamine di(*o*-hydroxyphenylacetic acid), leaving most of the acid in the nutrient solution. The chelating capacity of the nutrient solution increased as iron was absorbed by the plants. Most of the absorbed iron was found in the plant exudate.

Early investigators (*1*) suggested that synthetic chelating agents delivered iron to roots but were not themselves absorbed. Later research (*2*) led to the view that both components of the metal chelate were absorbed. Some emphasis has been placed on equivalent uptake of metal chelate components by plants (*3*), but later reports (*4*) suggest a nonequivalent uptake. The experiments reported here show that sunflower plants