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# Hypotheses connecting fluctuations in Arctic climate with biological productivity of the English Channel

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Summary—An explanation of variations in nutrient content and biological productivity in the English Channel in the last thirty-five years has been sought in terms of variations in Arctic climate. To provide the connecting links a series of interlocking hypotheses has been erected. These are summarized in the discussion.

For many years at Plymouth we have been concerned with the large variations in the phosphorus available for growth of plants and animals as represented by the amount of phosphate present at the midwinter maximum (Fig. 1). This was high during the nineteen-twenties, fell to a little more than half during the nineteen-thirties and forties, and is now tending to increase again. There have been large associated changes in the abundance and nature of the zooplankton in the English Channel.


Fig. 1. Annual variations in inorganic phosphate available for plant growth at a position in the English Channel, 22 miles south-west of Plymouth, as measured at the winter maxima. In 1922 and 1923 the maxima were probably missed; they were probably greater than shown.

For some years we sought an understanding from studies on the continental shelf within 200 miles of Plymouth (the Celtic Sea) and found none. The next step was to examine the conditions over the continental slope of the Celtic Sea (Cooper, 1952 b). Upwelling on the scale which occurs along the coasts of California, Peru and South-west Africa has never been recognized, since the prevailing winds are unfavourable for this. Even when winds have been favourable for upwelling of nutrients by the classical process, as in February, 1947, this has not occurred.

The phenomenon of cascading was first examined (COOPER and VAUX, 1949) and is now well authenticated (LEE, 1952; BODEN and KAMPA, 1953). It is primarily a process of impoverishment, stripping from the shelf whatever nutrients happen to be there. Work now in progress (1955) shows that this concept is too simple, and that there may be an associated mechanism which leads to enrichment of water overlying a cascade, though not of the area from which the cascade has come.

Other hypothetical processes of enrichment, such as capsizing and submarine eagres (COOPER, 1952 A, D) have also been proposed, but still lack observational basis.

At this point in the investigation, a satisfactory explanation of the fluctuations in phosphate and biological productivity was still not in sight. There was no escaping the conclusion that an answer would never be found whilst study was confined to the shallow shelf.

Then an investigation was undertaken which seemed at the time to be a false scent, viz. the search for an explanation of the sudden appearance off Plymouth in the autumn of 1950 of large numbers of boar fish, *Capros aper*. The hypothesis was erected that these had been ejected about six weeks earlier from their normal habitat in a coral-encrusted submarine canyon by a submarine eagre. A rider necessarily followed, that the conditions in the Atlantic abreast of the slope between depths of 200 and 500 m must have been very different in the nineteen-thirties and forties from what it had been in the nineteen-twenties. The *Capros* argument (COOPER, 1952 C) was a logical one, but the threads were as thin as gossamer. It may well prove utterly wrong. None the less it was this study which led to the decision that it was useless further to pursue studies in shallow water, but that the deeper Atlantic ocean must hold the key.

There is no need to invoke hypothesis to establish that—whatever the mechanisms may be—vertical mixing down to about 400 m occurs in winter south-west and west of the English Channel. The conclusion is evident from every station that has ever been worked there. There seems no good reason for believing that there has been any major change in the nature of the physical processes occurring in the upper 400 m during the last 40 years. However, the chemistry of these waters has changed, and we are forced to seek an explanation in terms of physical processes occurring at a greater depth. The enrichment with nutrients of the upper 400 metres can have come about only by some form of overall upward displacement of the deeper water. The idea is that, first, during the rich period, the layer of water containing, say, 0·8 to 0·9 μg-atom/l phosphate-P had been displaced upwards by some hundreds of metres, so that the processes of vertical mixing always operative in the upper 400 metres can bring them right to the surface, and that, secondly, during the poor years from 1931 to about 1950, this layer had subsided so that the mixing processes could no longer bite into it. Nutrient evidence to establish this argument beyond doubt does not exist. Due to internal waves, the observed depth of a nutrient sample obtained by snap sampling may differ by scores of metres from the mean depth. Consequently the sparse data available from earlier years are hard to interpret. Argument can proceed only *ex hypothesi*.

To achieve such a large-scale upward displacement of water, an equivalent volume at a greater depth, perhaps at the very bottom of the Atlantic, had to arrive from somewhere else. Such deep water is always cold and has sunk from the surface in polar regions.

Two events are necessary for the production of deep, bottom water in polar regions: one is that there must be present water of sufficiently high salinity, the other is that air temperatures must be low enough to cool this saline water sufficiently for it to become very heavy and so able to sink deep in the ocean.

Deacon (1937) has shown that this happens in the Weddell Sea and around the South Orkney Islands. Taking the world as a whole, this part of the Antarctic is by far the most important centre for creation of oceanic bottom water. As this water moves away from the Antarctic it may become much modified by mixing processes, particularly over the deep submarine ridges which separate the deep oceanic basins.

In the North Atlantic there are two areas of sinking of cold saline water. One is over the ridge which joins Faeroe to Iceland (Cooper, 1955) and the other is in the Denmark Strait and around East Greenland. In neither case can it be said with strict truth that the cold, heavy water which sinks deep in the Atlantic is formed in a localized area. Rather are we concerned with a continuous process which takes place all the way from Jan Mayen to the Southern tip of Greenland. It so happens, however, that events which happen between 300 metres depth and the sill of the two ridges connecting Iceland with Faeroe and Greenland stand out in high relief.

In the development for the Faeroe–Iceland area (Cooper, 1955) it was suggested that the density distribution associated with the Iceland–Faeroe current acts as a dynamic dam, parting the waters of the Atlantic from those of the Norwegian Sea. Whilst it runs steadily, little water need flow over the ridge. Our increasing knowledge of such currents (cf. Fuglister and Worthington, 1951) suggests that they rarely run steadily for very long and tend to meander all over the place. A meander to the south will carry cold Norwegian Sea water to the Atlantic, where it will lie above much lighter water through which it must sink. Again, if the current weakens, the isopycnals will flatten out. Saline surface Atlantic water will flow into the Norwegian Sea, and in compensation Norwegian Sea water below about 250 m will spill over into the Atlantic. In the neighbourhood of 62° N, 13° W, this cascading water sometimes has a salinity exceeding 35·0‰ and a temperature of less than 2° C. It is responsible for the salinity maximum which can be followed south-west for many hundreds of miles along the Reykjanes Ridge (Cooper, 1955) and round into the Western Basin.

A similar process in the Denmark Strait may differ in detail. The cold but brackish East Greenland current flows south along the coast, whereas the warm saline Irminger current flows from the south of Iceland round by the west to the north coast of Iceland. These two currents throw off spurs which coalesce with their opposite numbers, but in the centre there is a region where the two main currents must be moving more or less side by side and in opposite directions. This is an unstable situation, and must lead to a lot of eddying, swinging about of the main water masses, and mixing. The sill depth of the Denmark Strait is rather more than 500 m. At depths shallower than this north of the Strait there is much water heavier than 27·9 or 28·0 sigma-*t* and able, if it can escape, to sink into the Atlantic. It comes through the strait banked against the west side. South of the sill it does not fall straight into the trough, but is held against the terrace at depths less than 600 m. It travels south banked against the continental slope of Greenland, and does not reach the bottom until 60° N Lat. The evidence for these statements is being marshalled and will shortly be published. A great debt is due to the workers on the research ships, *Meteor*, *General Greene*, *Dana* and *Heimland*. Full acknowledgements will be made later.

On all sections this piling up of heavy water to the right against the slope is to be seen, but there is much variation in temperature; in some sections there is an abundance of water having potential temperature less than 2° and salinity near to 34·91‰. In others there is no water colder than 2·5°, or, in one "Meteor" section, colder than 3°. There is always a deep current towards the south-west, but the presence of very cold Denmark Strait water is intermittent.

Such intermittent bursting of the dynamic dam, due to either process, would result not in a continuous stream of water flowing off the ridge, but in a series of self-contained, cold, heavy balls or boluses of water. The calving of a large bolus of water from the ridge is likely to be completed in a matter of days or weeks at most, and is likely to occur most often in late winter or early spring. The descent of a bolus from 400–500 to 1,500–2,000 metres through much lighter water is likely to need a very short time. It is not surprising, therefore, that no research ship has ever recognized the birth process for what it may be.

This water does not sink by the shortest route to the bottom of the Atlantic, but is held to the right by the force of the earth's rotation, and so traverses the side of the continental slope for 300 miles to Cape Farewell, and continues in this way around the bottom of the Labrador Sea at least as far as Newfoundland.

The evidence I have presented gives no idea of the frequency with which these cold water masses or boluses are born or calved in the Denmark Strait. It could be a seasonal event, once a year. It seems more likely that the complex water movements there are constantly meandering, and that the thrust of a cold bolus to the south over the sill may be a rather irregular affair. If so, the sinking of boluses along the Greenland coast may also be an irregular affair.

It is helpful to think of one of these boluses as behaving like a solid but elastic object. As it thrusts forward and downward, it must displace the water already there.

Moreover, when passing over an irregular slope and bottom, the under surface of such a bolus of water must be expected constantly to adjust and re-adjust itself to the solid topography. Its free surface against the enveloping water must needs also constantly re-adjust itself. The picture one has is of a vigorously writhing interface which should initiate internal waves in the adjacent ocean, even if this is only weakly stratified.

Shortly after calving, whilst it is sinking rapidly from sill level at about 500 m to 1,500 m, the internal wave pattern should be intense but short lived. After a depth of 2,500 m has been reached, a number of such boluses are likely to be present and in some degree to have coalesced. The writhing will be gentler but unceasing. This writhing of boluses in the North Atlantic provides an origin for internal waves down to at least 4,000 m. In the North Pacific no such possible origin for internal waves has been recognized deeper than the bottom of the sub-Arctic water around 1,500 m. Deeper waves might however arrive in the North Pacific from the Antarctic.

It is commonly believed that the attenuation of such waves in the open ocean is not severe. They may travel great distances. If there is no frictional loss on passage, for a few hundred miles, the energy in a wave propagated in an equipotential surface from a point source will be inversely proportional to the distance run. For greater distances such waves, not accompanied by transport of water, should follow great circles, so that the law of attenuation would need to be worked out not from plane but from spherical trigonometry. However, in the Eastern North Atlantic the source of internal waves is postulated not as a point but as ridges or lines more than 600 miles long. Consequently the energy in waves from these lengthy sources arriving at the European continental slope may be considerable.

Let us now consider what may happen when such a wave system meets a continental slope. If it approaches head on at a smooth gentle slope, it is likely to run straight up it and to be reflected back. A standing wave might well develop off slope, but the conditions are unlikely to lead to a lot of mixing. From the usual snap oceanographical observations such a standing wave might be interpreted as evidence for a strong along-slope current. If the wave system approaches a smooth slope at a wide angle, the waves may swirl along it, and somewhat more mixing would result.

The really interesting case is when an internal wave system approaches a highly dissected continental slope or borderland at a glancing angle. The slope abreast of the English Channel is likely to provide an excellent site for this process, while the Southern Californian continental borderland may provide an even better one. Then, as I see it, there should be a very great deal of sloshing about indeed, much vertical mixing and a tendency for homogenization of all properties as far down as the internal waves occur. This means that nutrients will be brought up to such depths that the classical methods of wind-driven upwelling can bite into the enriched water and bring it right up to the surface. If, and when, cold Arctic winters produce large boluses and consequent internal waves of large amplitude, then upper-water enrichment with nutrients over continental slopes would be favoured.

Furthermore, we may be concerned not only with the conventional nutrients, but with associated organic growth accessory factors which we only dimly understand. Much dead plankton, faeces and organic detritus sink in the sea and decompose. This should happen to the greatest extent in the oxygen poor layer. Oxygen defect (saturation value less content of oxygen observed) provides a very rough but ready measure of the extent of decomposition products. During periods of minimum internal wave activity the maximum oxygen defect may well be more than in periods of great activity. But paradoxically, the depth range of the oxygen poor layer should be less. The inflexion should be more pointed. This is because the homogenizing action of internal waves beating against a dissected slope should spread the oxygen defect both above and below the point of inflexion. The upward spread would bring not only the oxygen defect but also the accompanying organic substances nearer the surface.

In this group of interlocking hypotheses, some may be rejected without gravely imperilling the rest. The concept of homogenization is not one of these for, if it falls, the others become useless. To illustrate the argument, let us assume that some property such as phosphate content is linearly proportional to depth, and is subject to a uniform process of homogenization from surface to an inert bottom. In mid water, exchanges of phosphate will occur, but the later state will remain analytically indistinguishable from the earlier. If a layer was labelled with radiophosphorus, homogenization would spread this up and down so that the result might be seen as a form of eddy diffusion. At the surface, exchange with the air being impossible, a homogeneous layer enriched from below would begin to build up. As the process proceeds, both depth and phosphate content of the homogeneous layer would increase. Similarly at the bottom, homogenization will increase the thickness of a uniform bottom layer containing phosphate equal to that at the point of inflexion. The defect in phosphate content which would seem to have appeared near the bottom has actually been moved upwards, and equivalent enrichment has appeared near the surface. It should be clear that the molecules of phosphate which appear in the surface waters are not those which have left the bottom. The process is akin to water moving up a pipe. If the process went on long enough and unhindered, the whole ocean would become homogenized from top to bottom. At an intermediate stage, it might seem that phosphate was being transported from the bottom to the surface waters, without the middle layers being affected in any way that could be recognized by chemical analysis.

Homogenization is a two-way process. In an ocean in which a nutrient salt increases downwards, the shallower and poorer waters are enriched at the expense of the deeper. Temperature or heat content, on the contrary, decreases downwards so that the same process causes the deeper layers to warm up at the expense of the shallower. Now let us consider an area of the sea near a continental slope: (a) where the broad pattern of currents and winds as well as the amount of radiant energy received from the sun remain unchanged but (b) where the intensity of deeper internal waves derived from distant sources increases. Homogenization should increase so that the surface layers would become colder but richer in nutrients. If then for some years the process is reversed, internal waves from a distance would become weaker so that homogenization against slopes would decrease. Loss of nutrients and accessory growth factors from surface waters by sinking of faeces and detritus would not be fully made good. In consequence the surface waters become poorer. At the same time, radiation received from the sun would be retained in the upper waters, instead of being homogenized by mixing with deeper colder water. The sea surface would warm up. Something very like this seems to have happened in Western European waters in the last thirty years.

The changes in heat balance would be expected to produce some local changes in winds, weather and currents, so that in places this generalization may not apply. Averaged over a large ocean, however, and granted the premises, it seems inevitable.

Again, when very cold winters prevail in the Arctic, more cold heavy water will be produced, and will spill over the ridges between Greenland and the Faeroes into the Atlantic. The scale of everything will be increased, so that the amplitude of the system of deep internal waves in the ocean should also increase.

Let us now consider two oceans. In one, writhing boluses of newly arrived water may initiate internal waves and subsequent homogenization right to the bottom in 4,000–5,000 m. In such an ocean, combination of the two separate processes of upward displacement and homogenization will cause deep capital reserves of nutrients to be made relatively quickly available for biological production in illuminated waters. In the second ocean, sinking of cold water does not go deeper than, say, 1,500 m depth. Below this depth there can be no writhing surfaces, no appreciable internal waves and no homogenization against continental slopes. There is then no way by means of which nutrients below this depth can be got into circulation. Vast resources of nutrient capital remain unused for long periods. In such an ocean only the resources above, say, 1,500 m would be in effective circulation.

In this parable the oceans in mind are the North Atlantic and the North Pacific. The North Pacific is much the larger, so that for it the ratio of length of slope to volume of water is much less than for the North Atlantic. This also militates against the efficient use of the capital resources of the North Pacific by a process of homogenization.

The North Atlantic seems on the whole to be subject to impoverishment by exchange across the equator. Loss of relatively rich deep water, tends to be compensated by inflow from the south of relatively poor surface water. This is a process similar in nature to the impoverishment of the Mediterranean by exchange of waters in the Straits of Gibraltar. Consequently the deep phosphate resources of the North Atlantic are only about one-third of those of the North Pacific. In spite of this, the North Atlantic contains many of the world's richest fisheries. It is true that the North Atlantic contains an undue proportion of the shallow shelves well suited to the growth of fish, and that it is bordered by enterprising communities who make the most of what nature offers. But when all allowance is made for these important considerations, it still seems that the biological productivity of North Atlantic waters is greater than it ought to be. The nutrient capital of the North Atlantic seems to be more efficiently used than that of the North Pacific, except around Japan. The interlocking hypotheses offered here provide a possible explanation of this.

Again, judged by its phosphate resources the Mediterranean should be a near-desert sea. Though not rich, it is certainly not a biological desert. Here again one is forced to the conclusion that the slender phosphate capital of the Mediterranean is put to maximum use. It might be worth enquiring whether the present hypotheses could be usefully applied there.

Attention has been focussed on the events possibly initiated by calving of boluses of heavy cold water over ridges in high latitudes. There is another and different mechanism which may produce similar results on a smaller and shallower but not negligible scale. It may be illustrated from the Straits of Gibraltar. Here, light Atlantic surface water flows into the Mediterranean, and heavy, relatively cold and very saline deeper Mediterranean water flows out. In nature it is becoming ever more evident that fluid motions such as this tend to be irregular or gusty. It is reasonable to suppose that the outflow of Mediterranean water may not be as a steady stream, but as a series of boluses separately calved. This would initiate a chain of events similar to those already described for Faeroe–Iceland water, and augment the efficient use of the nutrient capital of the Atlantic. It is less easy to see how this calving process into the Atlantic might influence the Mediterranean regime, but some effect there is possible.

Two distinct speculative mechanisms have been described to transfer energy through deep water from one part of an ocean to another and, by so doing, to facilitate enrichment of surface waters with nutrients. These are (1) the hypothesis of upward displacement due to the intrusion of colder and heavier waters at a greater depth, and (2) the hypothesis of transfer of energy by internal waves, initiated by the sinking of boluses of cold, heavy water calved from ridges in high latitudes. These two hypotheses need not be mutually exclusive, but may merely express facets of a complex system of energy transfer at depth. This, in the words of Otto Pettersson, might be described as the systole and diastole of the ocean. Only many observations well placed in space and time may show what weight should be given to each or any of the component hypotheses.

Finally, the whole argument suffers from a grave thermodynamic weakness. Thermodynamically, energy can be obtained only from a source and never from a sink.

The areas around the Faeroe–Iceland Ridge and the Denmark Strait are sinks in every sense of the word. Consequently, not until the argument is geared to a thermodynamic source can it be fully satisfactory.

A chain of hypotheses has been constructed to link fluctuations in Arctic climate with fluctuation in the biological productivity of the English Channel. The author is under no illusions as to their vulnerability to attack from many directions. He can hope only that, by offering a target, something more substantial may emerge.

There is climatic evidence in support of the hypotheses.

SMED (1947, *et seq.*) has collected and summarized all the records of surface temperature which have been collected in the North Atlantic. Fig. 2 shows the areas into which he has divided the North Atlantic. His areas B, C, D are those in which we are most interested. A series of cold winters was experienced in the 'teens of this century, and

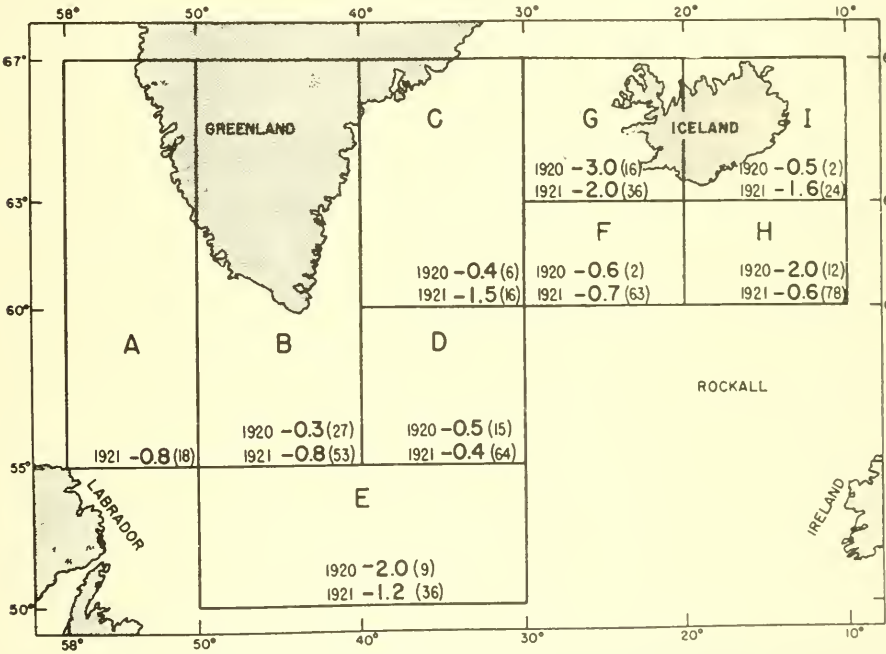


Fig. 2. Northern North Atlantic. Average departures of sea surface temperatures (°C) from long term means in the months of April 1920 and 1921. The number of available observations averaged is shown in brackets (after SMED).

particularly in 1918, 1920 and 1921. In the same Figure the departures from the long term average of the sea temperatures in April, 1920, and April, 1921, are presented. These were the culmination of a series of cold winters when a great amount of North Atlantic bottom water must have been formed.

Air temperatures between 60° and 70° N have been recently summarized by BROWN (1953). Fig. 3 shows the area studied divided in Marsden squares. Unfortunately, for squares 220 and 221 (in which we are most interested) there are insufficient data for evaluating means. His decadal means for areas 217A and B and 218A and B are shown in Fig. 4. It will be seen that the decade 1910–1919 in December–March was

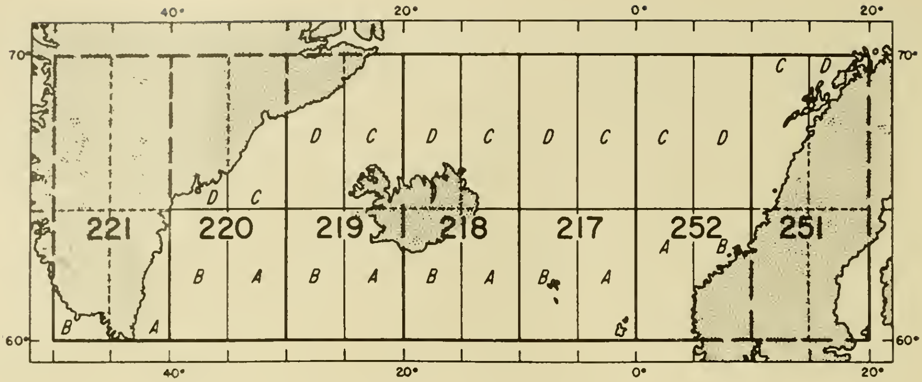


Fig. 3. Chart showing position of Marsden squares 217 and 218.

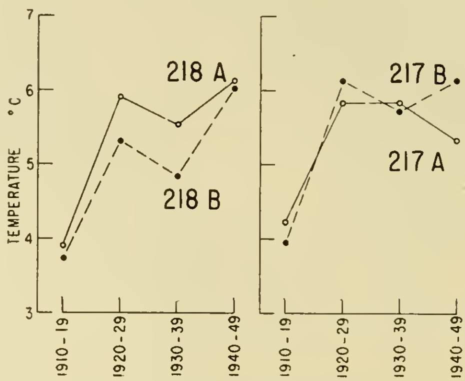


Fig. 4. Decadal mean air temperatures for Marsden square areas 217A and B and 218A and B for the season December to March (after P. R. BROWN).

$1\frac{1}{2}°$ colder than the three decades 1920–49. That is on a decadal basis the years 1910–19 were more suitable for the formation of oceanic bottom water than the thirty years since. It is probable that in these decadal means the coldness of the years 1920 and 1921 was swamped by the eight following warm years. Nothing comparable was experienced for the next thirty years. In these years we had precisely the conditions which we need to explain the fluctuations in the English Channel. In each winter an amount greater than average sank in the Greenland area. In the first winter the excess sinking would have displaced the strata of water upwards by, perhaps, a few tens of metres. This would not have brought much nutrient rich water within reach of the processes of vertical mixing of surface waters which are always operative. The next winter would have been more effective and the third winter most effective of all. By this time the nutrient table would have been lifted by perhaps 100 metres or more, to a level at which processes of vertical mixing would readily and continually draw upon the upwardly displaced water to enrich the surface waters. The deep waters of the ocean move slowly, so that there may be a considerable time lag before events in polar regions come to influence upward displacement in temperate and tropic latitudes.

On this line of argument, the maximum enrichment of the surface waters of the temperate Eastern North Atlantic with nutrients occurred in or shortly after the year 1921.

If unusually large amounts of water sank in the North in these winters, they had to be compensated by an equivalent amount of surface water, which could have been supplied only by increased intensity of the North Atlantic Drift system. One event associated with this increase in the North Atlantic Drift may be the incursion into the English Channel of much unseasonably warm water in the autumn of 1921 (HARVEY, 1925).

We have long had the suspicion that this incursion of warm water was in some way associated with the rich phosphate observed by ATKINS in his first analyses in the English Channel on 7 March, 1923. I, for one, have always had great difficulty in accepting this explanation, since warm surface water is nearly always poor in nutrients. Today there is no such warm water anywhere in the Bay of Biscay or Eastern North Atlantic carrying an equivalent supply of nutrients. This objection now vanishes.

This water in some area well to the south of the British Isles had been enriched with nutrients by upward displacement following a period of cold Arctic winters and the three winters of excessive polar sinking. During the warm summer of 1921 there was time for the water to warm up by solar heating, but no considerable redistribution of nutrients with the deeper water had taken place. The nutrient properties of this particular warm water may well have been very far from the equilibrium state that we commonly observe.

## DISCUSSION

During the last thirty-five years there have been large changes in the distribution of phosphate in the Western English Channel whilst the zooplankton has changed in species and abundance. No explanation has been found locally, so that attention has been extended to events in the Atlantic Ocean.

A number of associated hypotheses have been erected to explain these changes in the English Channel:

(1) That in cold Arctic winters saline surface water is cooled further and made heavier than in relatively mild ones.

(2) That this leads to a greater recruitment of fresh deep water in the North Atlantic after cold Arctic winters.

(3) That to make room for this fresh deep water, an equal volume of water has to be displaced upwards, i.e. a supply of nutrient-rich deep water is displaced towards the illuminated surface layers. This process may be a diffuse one over the whole ocean basin.

(4) That the origin of the North Atlantic deep water should be considered not in terms of a localized area, but in terms of physical processes which are occurring all the way from Jan Mayen (72° N) to the southern tip of Greenland (58° N), and even further south.

(5) That in so far as special areas stand out, these lie over the ridges between Iceland and Greenland on the west, and the Faeroe Islands on the East. Over the two ridges the physical processes which yield heavy water seem to differ in detail, but in both are

confined to depths between about 300 m and the sill depth of about 550 m. Local surface waters seem not to be overmuch concerned.

(6) That the cold heavy water flows away from the sills of the two ridges not as a smooth continuous current, but intermittently as discrete large boluses.

(7) That due to the effect of the earth's rotation these boluses are held strongly to the right against the eastern slope of the Reykjanes Ridge, and against the eastern continental (or insular) slope of Greenland.

(8) That where the boluses are constrained by restricted topography, they move fast horizontally and sink slowly.

(9) That where narrow straits open out to oceanic dimensions and constraints are removed, then horizontal velocity decreases and the boluses sink rapidly.

(10) That where boluses of heavy water pass over a dissected bottom or slope, they mould themselves to the rock configuration, and that in consequence the free interface against the enveloping water writhes vigorously.

(11) That such a writhing interface, at what is effectively a strongly developed discontinuity layer, initiates strongly developed internal waves which may travel great distances through the open ocean with little attenuation.

(12) That when such waves meet a continental slope vertical mixing should result, and that when they meet a highly dissected continental slope at a glancing angle, the mixing should be very vigorous indeed. Homogenization of all properties would be much favoured, i.e. shallower layers would be enriched with nutrients at the expense of the deeper and, conversely, deeper layers would, in most places, be warmed at the expense of the shallower.

(13) That the nutrients are finally brought to the surface by thermal and wind-driven mixing processes, and by upwelling processes which may be always operative. It is suggested that these processes are much more effective in producing surface enrichment when a series of cold Arctic winters has produced a situation in the deep Atlantic which favours upward displacement and homogenization of nutrients, especially against continental slopes.

(14) That in the North Pacific there is no exact parallel to events in the North Atlantic. The only comparable process would seem to be unable to affect the North Pacific waters deeper than about 2,000 m at most. Consequently the nutrient resources of the deep North Pacific seem to be much less effectively used than are those of the deep North Atlantic.

Some evidence for hypotheses 1–9 has been presented elsewhere (COOPER, 1955). Further evidence for hypotheses 1–9, applying to the Denmark Strait and East Greenland, is being marshalled for publication.

Hypotheses 10 and 11 are intuitive and no supporting evidence is known to the writer.

Direct observations in support of hypotheses 12 and 13 are now being sought in the Atlantic near Plymouth. Further evidence in support of hypothesis 14 is still being sought.

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