DEEP CIRCULATION IN THE OCEANS (*)

by

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I. *PROBLEM OF THE DEEP CIRCULATION.*

For some fifteen years past, physical oceanography has been particularly devoted to problems of deep circulation. New data in great number, collected by means of the most recent methods and instruments, have been remitted by oceanographic expeditions, especially by those of the *Meteor*, of *Discovery II*, of *Dana*, of the *Carnegie*, of the *Willebrord Snellius*. Prior information, collected since the time of the *Challenger*, has been subjected to meticulous examination permitting its utilisation ; on this subject the works of G. WÜST are noteworthy (1).

A question of principle first arises : does a deep circulation exist which may be detected by available methods which will allow its mechanism to be analyzed and permit its representation ?

For a long time there predominated the concept of an almost complete stability of the deep layers of the ocean beginning with the *discontinuity layer, plan de cassure, Sprungschicht* or *thermocline*, in which the thermal index, after a rapid fall at the surface and in the subsurface, thereafter decreases only slowly (at a temperature of + 4° C and towards 1000 metres' depth) : this concept excluded from the regular circulation three-quarters of the oceanic mass. J. THOULET hoped to have found a proof of this stability in the vertical fall of the globigerina frustules. Densimetric measurements revealed only a slow and regular increase of the density towards the deep layers. At most, J. THOULET conceded slight turbulent motions on the bottom due to submarine eruptive action (2).

These views concerning oceanic immobility in the depths are to-day abandoned. However, they have left, at any rate in France, a sort of residue in the principle of the immiscibility of the waters in deep layers attached to the theory of *Transgressions* by Ed. LE DANOIS. Indeed, to consider the deep waters of the ocean as compounded of a sort of puzzle of liquid masses impenetrable the one to the other, a large number of which would appear to be as much fossilized as terrestrial geological layers, means implicitly an affirmation of the immobility of at least a great part of those bodies of water (3).

In fact, the progress of oceanic observation forbids us to doubt the existence of the deep circulation. Answers to the questions offered for consideration do not aim at proving the existence of such a circulation — nowadays admitted. The problems are as follows :- does the circulation act solely in *large liquid masses* in a vertical or in a lateral direction ? Does it occur solely by turbulence movements of small radius with intricate combinations in one case or the other ? Or again, does it combine, in a sort of algebraical sum with indefinitely changing factors, *mass movements* and *turbulence movements ?*

II. *METHODS OF OBSERVATION AND CALCULATION.*

It is evident that for determining the regular, periodical or occasional, movements of the sea, methods differ for the surface, the subsurface and depths :- at the surface or subsurface direct instrumental observation ; in depths, indirect instrumental observation and calculation. Such a distinction is fully justified by the extreme difference in speed of the observed movements and also the very difficult practical problems involved in correcting depth data.

Let us briefly recall that at the surface several systems of direct observation have been and are still in use : first the difference between the D. R. position of the ship and the astronomical fix taken at 24-hour intervals ; this is the principal source of information required by navigation as plotted on British and American Pilot Charts ; then we have the *floats* dropped at a determined point and recovered at a greater or less distance from that point; next we have observation of the drift of *derelicts* or wrecks abandoned by their crews and drifting, often for considerable periods of time, under the impulse of winds or currents; lastly, on the edges of frozen seas, there is the drift of icebergs and icefields.

These empirical methods are not satisfactory for a clear understanding of ocean dynamics, even at the surface, because, with the exception perhaps of icebergs, the majority of which are submerged, all surface floats are influenced by the wind and respond to the latter as much as to the particular movement of the water mass; they do not in any way help to solve the problem of the action peculiar to each of the two fluids. We mention them only as a reminder, since none of these processes can be applied to subsurface circulation, still less to deep-water circulation.

This is not the case with regard to *current meters*, among the numerous existing models of which I shall mention only the EKMAN meter and the IDRAC current-meter. They are located at subsurface at a known depth; consequently they escape the immediate influence of the wind. They record this influence only when it is, so to speak, incorporated in the water mass in motion. Subsurface circulation must be considered as the true superficial oceanic circulation; for a predetermined point and time, current meters enable an accurate estimate of it to be made.

So far, however, the subsurface only has been concerned and not the deep oceanic mass. To what level is it possible to use current-meters? The most recently devised, the IDRAC *moulinet*, has brought us a great step forward in this direction. It appears that this instrument can be used with efficiency to depths of 500 metres. This is still a level pertaining to the subsurface; by which we mean that, according to all oceanographers, there are still revealed thermal and haline (salinity) variations in relation, if not with diurnal variations, at least with seasonal variations of supraoceanic atmosphere (4).

The problem therefore remains in its entirety, unsolved for deep layers, above all for those below the *thermocline*, i. e. for three-quarters of the oceanic mass.

In order to detect the movements, of large or small amplitude but always very slow, which occur in the depths, we have first the temperature and salinity indices (*thermal* index and *haline* index).

From the thermocline towards the great depths, temperature continues to decrease slowly but irregularly; the inequalities, although they may be only of the order of one-tenth degree, disclose breaks in the equilibrium and, consequently, motion.

The same holds good for inequalities of the haline index; in depths these are of the order of a decigram or centigram of the salt weight; low as they may appear, they also disclose equilibrium breaks which imply movements.

Is it, however, sufficient to observe what Americans call the *T. S. correlation*, i. e. the agreement or disagreement of the two indices, in order to obtain the empirical determination of the deep circulation, as was already done some time ago by BARTLETT and PILLSBURY? W. BJERKNES did not think so. Although the empirical method may very well give the direction of the deep-sea movements, it does not give the intensity — in other words the degree of velocity — of these movements.

In his *Hydrodynamique* (1910), therefore, W. BJERKNES proposed a method of calculation by means of which one may rapidly determine not only the direction of deep currents but their velocity, their limits and, consequently, the volume of water which they transport (5).

BJERKNES is of the opinion that for the mathematical solution of the problem of the motion of waters both surface and deep-sea, it suffices to consider the *pressure field* and the *mass field*, in which the liquid bodies evolve. The pressure fields are represented by *isobar* surfaces where the pressure, as a function of the depth, is calculated in decibars, which correspond roughly to one metre. The mass fields are represented by surfaces of equal specific gravity or isosteres surfaces; the specific volume, or the mass unit, is the reciprocal of the density; like the latter, it is given by observation of the thermal and haline indices. Isobars and isosteres intersect along planes of intersection called solenoids, a name borrowed from the physics of electricity.

Taken as a whole, the motive forces considered by BJERKNES appeared too simple to W. EKMAN who proposed to develop BJERKNES' mathematical representation by introducing new symbols to represent the influences of the wind, of friction (viscosity), and of deviation due to earth rotation. A thorough analysis of these variables enables EKMAN to determine seven influences, the combinations of which, at the surface and subsurface, form the *turbulence*. According to EKMAN, the addition of these variables to

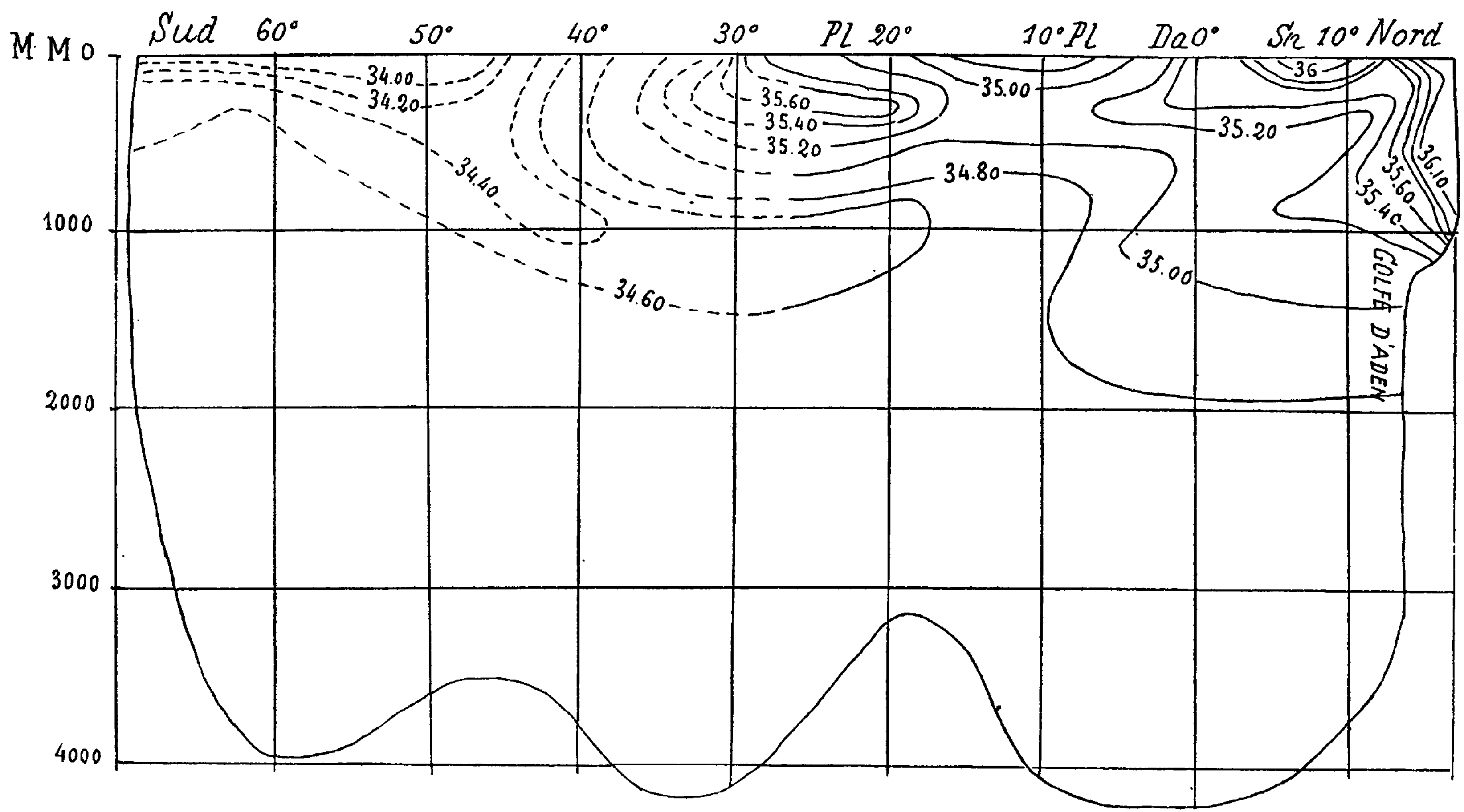

Fig. 1

L'Indice d'Oxygénation du S. au N. de l'Océan Indien, d'après le Discovery II
Pl = Planet ; Da = Dana ; Sn = Snellius. — *Isohalines incertaines* — — —
Oxygen Index from S. to N. of Indian Ocean from *Discovery II*
Pl = Planet ; Da = Dana ; Sn = Snellius. — Isohalines uncertain — — —

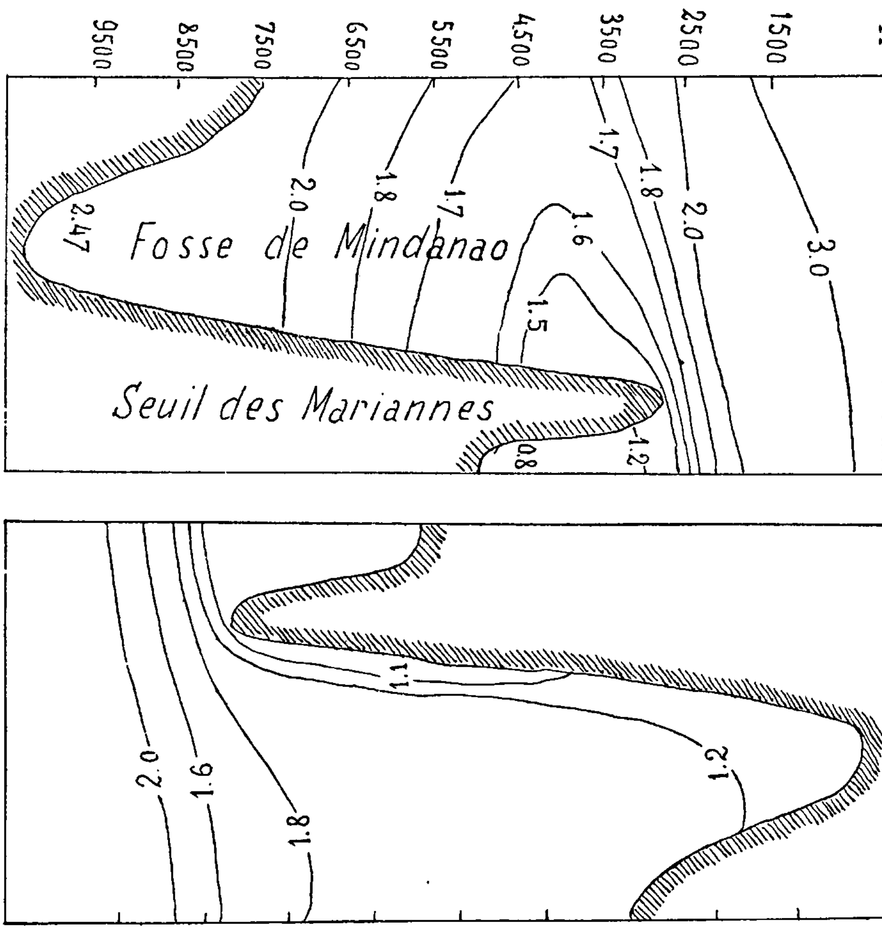

Fig. 2

La température in situ et la température potentielle dans la fosse de Mindanao d'après les mesures du Tuscarora *et du* Willebrord Snellius

Temperature *in situ* and potential temperature in the Mindanao Deep from Measurements by the *Tuscarora* and the *Willebrord Snellius*

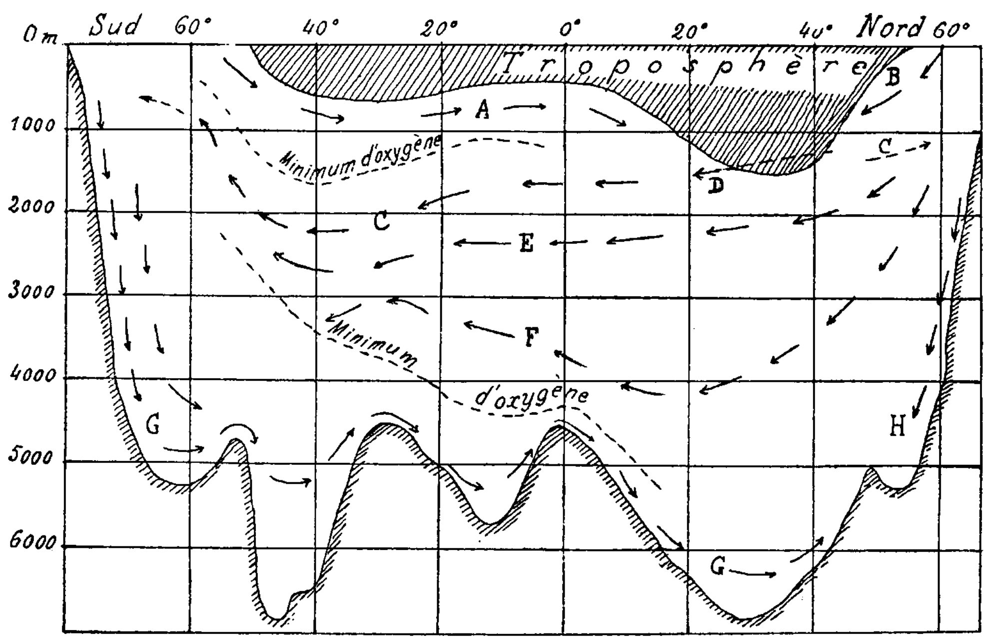

Fig. 3

Courants de la « Stratosphère » atlantique, d'après G. Wüst
A = *Eau intermédiaire subantarctique.* B = *Eau intermédiaire nord-atlantique.* C = *Eau de fond supérieure nord atlantique.* D = *Eau méditerranéenne.* E = *Eau de fond moyenne nord-atlantique.* F = *Eau de fond inférieure nord-atlantique.* G = *Eau de fond antarctique.* H = *Eau de fond subarctique.*
——— *Courants de la partie Ouest. Courants de la partie Est.* — — — —

Currents in the Atlantic « Stratosphere », according to G. Wüst
A = Subantartic intermediate water. B = North Atlantic intermediate water. C = North Atlantic Upper bottom water. D = Mediterranean water. E = North Atlantic Mid-depth bottom water. F = North Atlantic Lower bottom water. G = Antarctic bottom water. H = Subarctic bottom water
——— Currents in Western portion. Currents in Eastern portion — — — —

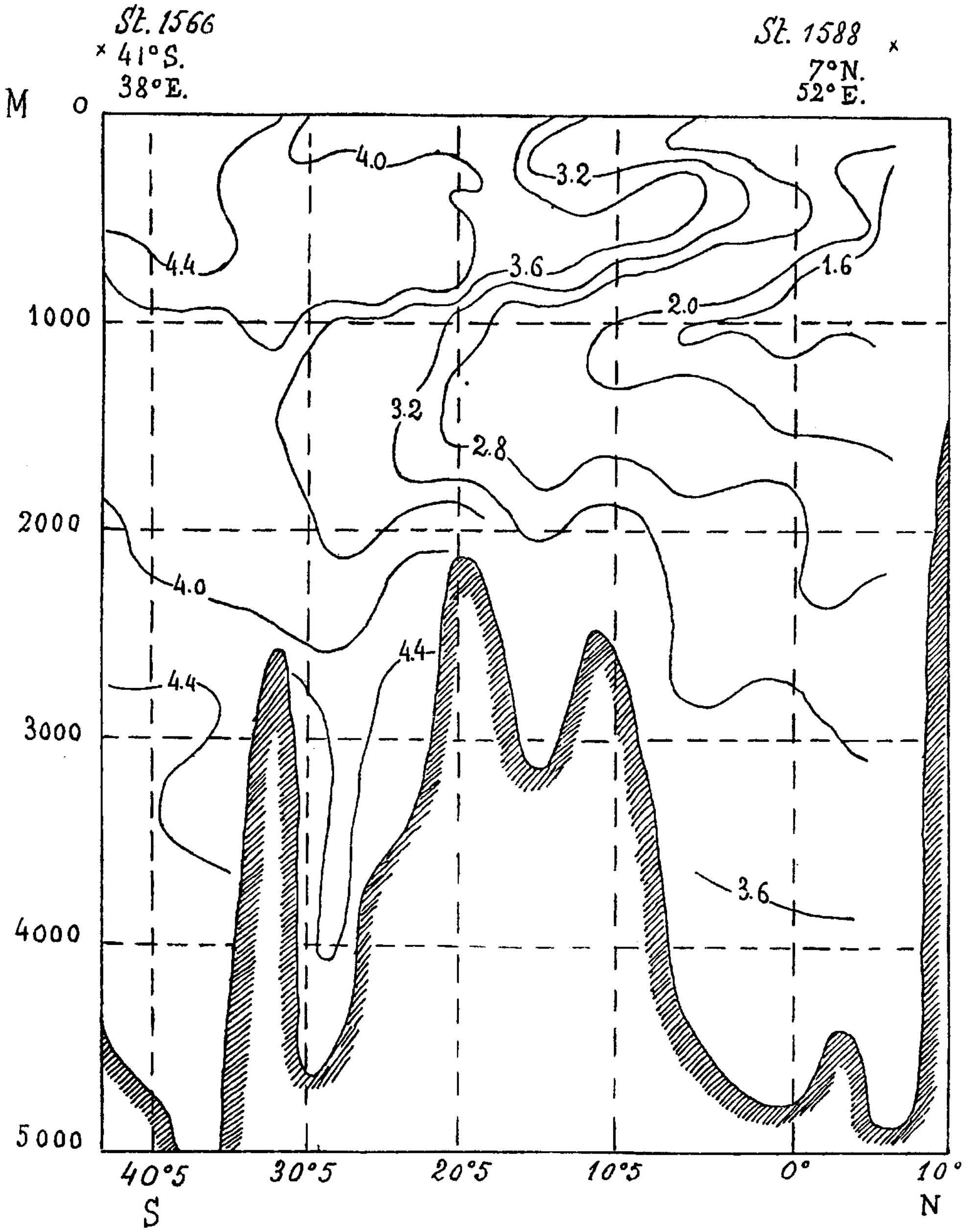

Fig. 4

Indices de salinité dans la partie occidentale de l'Océan Indien

Salinity indices in the Western portion of the Indian Ocean the BJERKNES symbols affords a complete representation of current direction and intensity, at surface and subsurface (6).

This may be admitted. It may be conceded that the BJERKNES and EKMAN equations have almost grasped the superficial phenomenon (i. e. the phenomenon occurring above the thermocline). In comparing, for the Florida current, the calculations of BJERKNES with the empirical observations of PILLSBURY, G. WÜST has demonstrated by their concordance the validity of the method (7). On the Grand Banks of Newfoundland, in the surrounding waters and in the sea of Labrador, the International Ice Patrol and American scientific expeditions constantly and successfully use the BJERKNES-EKMAN method in places where liquid masses of very different signs confront each other (8).

It may be asked, however, whether the method is equally valid for the masses which form the subject of this paper, that is to say, for depths below the thermocline ? This appears doubtful.

Indeed, the validity of the BJERKNES method even with the addition of EKMAN's *turbulence* definition, is based principally on the presence of defined liquid bodies occupying a greater or less volume and clearly separated from each other. In deep waters, where almost inappreciable changes occur over great expanses, things are far from presenting this appearance. On the other hand, the idea of *turbulence* does not appear to be sufficiently developed by EKMAN ; as we shall presently see, it involves other factors which it appears difficult so far to submit to quantitative analysis. Besides, neither the BJERKNES nor the EKMAN method of calculation takes into account a factor which reveals itself of more and more importance in the study of deep circulation — I mean the *oxygen index.*

Apart from the combined oxygen, oceanic waters contain at all depths a quantity of oxygen in solution which may be measured accurately, especially since the application of the WINKLER method in 1901, and can only result from the absorption of the atmospheric oxygen at the surface, or from the production of oxygen by chlorophyllian vegetation (the American processus photosynthetic). Oxygenation, although difficult to interpret, certainly proves regular communication, by turbulence or otherwise, between the surface and the deep layers (9).

It is comprehensible that in those conditions G. WÜST renounces, *for the moment*, as he says (for a long time, in our opinion) the BJERKNES dynamic method, "doubtless because the forces are often too small in proportion to the frictional perturbations" — a phrase which implicitly recognises, it would appear, the preponderant effect of *turbulence* in the deep layers (10).

The admission that it is not yet possible to apply regularly the BJERKNES method below the *thermocline,* seems to involve a complete and absolute generic distinction between surface and subsurface circulation on the one hand and deep circulation on the other.

Such a distinction has been made in Germany since the *Meteor* expedition ; German oceanographers make a clear separation between circulation above and circulation below the thermocline. By inverse analogy with the terms used in meteorology, they give to the first the name *troposphere,* to the second the name *stratosphere.* In the marine troposphere appear long or short period variations related to lower atmospheric conditions. The stratosphere, in which much slower motions occur, does not show anything similar.

This terminology, generally adopted by nordic scientists and even by those of England and U. S. A., is therefore based upon an analogy ; and offers the advantage of all comparisons : it conveys an image. It also, however, offers their inconveniences : it is inexact. "Comparaison n'est pas raison" says an old French proverb.

I have pointed out that between the *troposphere* and the atmospheric *stratosphere* is a fundamental difference in their nature arising from the presence of vapour in the first and its absence in the second, while the Ocean is practically homogeneous from the surface downwards to the greatest depths (11).

It may also be objected, as has been done in the United States, that while the entire atmospheric troposphere is agitated by large movements, the strong movements of the marine troposphere are wholly limited to the surface. In the marine troposphere the convection forces are probably less important than in the atmosphere. The limit between the two zones is fairly distinct in the atmosphere ; while in the Ocean the thermocline-limit, except near the Equator, occupies a thick layer of some hundreds of metres (12).

It seems to me that G. Wüst, in his profound studies on Atlantic deep currents, has brought a strong argument to bear against the troposphere-stratosphere concept when he states that, on the one hand in the Gulf Stream zone — the only one for which we have very numerous depth data — the troposphere extends to isobaths much deeper than usual and, on the other hand, there exist remarkable analogies between the tropospheric and stratospheric circulations of the Ocean : strong and regular in the western part, weak and sometimes irregular and uncertain in the eastern part ; which suggests a close structural identity of the two circulations in spite of their extreme difference in intensity (13).

In 1929 also, A. Defant appears to me to have singularly altered the scope of the German terminology when he stated that, between the 40th and 50th S. parallel, the stratospheric waters reach higher levels up to contact with the atmosphere which, he declares, is decisive for the circulation of the marine stratosphere :- "...it would stop almost completely if it were everywhere covered by the troposphere". Consequently the so-called marine stratosphere is not a self-contained body as commonly represented up to the present ; comparison with the atmosphere becomes decidedly misleading since it is evident that nowhere does the atmospheric stratosphere traverse the troposphere ; nowhere does it come into contact with the soil (14).

In such conditions it would seem preferable to cease using the terminology in favour in the nordic countries. Let us say that on the one hand there is a *surface and subsurface circulation* and, on the other, a *deep circulation* : that, without prejudice to causal explanations, these circulations differ widely in intensity, which fact, no less than the position occupied by man on the planet, necessitates very different methods of observation and calculation in one and the other case : that the true laws of surface circulation must be sought for at subsurface ; and lastly, that subsurface circulation laws are more or less definitely connected to those of deep circulation.

III. *DOCUMENTATION AND WORKING UP THE DATA.*

Having submitted the preceding, we can now try to synthetize and interpret the knowledge at present available.

Such knowledge is derived only from deep-sea soundings obtained in the Oceans during three-quarters of a century and from the measuring instruments used in such soundings for obtaining temperature and density.

In spite of numerous explorations, the assiduity of explorers and the useful data contributed by cable-ships, the observations collected represent but an infinitely small total when compared with the immensity of the field to be explored. Deep circulation observations can be made only by means of wire sounding — always a long and troublesome operation.

From this point of view a few figures are significant.

According to G. Wüst, there have been made from 1869 to 1934 in the Atlantic (including the Atlantic area of the circumcontinental ocean which I call the Southern Ocean ; same remark for the other oceans) only 852 temperature measurements, 537 of which were made by cable-ships, to depths exceeding 4000 metres (15).

For the Pacific, two and a half times larger, the proportion is less : 1501 temperature measurements at depths exceeding 4000 metres ; 122 of these having to be discarded there remain 1379 (16). Up to 1929, almost complete ignorance prevailed with regard to the Indian Ocean ; according to Lotte Möller, at that date there were only three stations to supply complete series of temperatures at depths exceeding 3000 metres (17).

Are measurements so sparse and, so to speak, lost in the ocean immensity sufficient — I shall not say to form the basis of a certitude — but at least to give some appearance of soundness to the assumptions ? For the Atlantic, G. Wüst is affirmative. With regard to the Pacific and also, I imagine, for the Indian Ocean, still less known, he makes reservations. In our opinion reservations must be made for the whole of the oceanic deep circulation. As yet we are only in the period of working hypotheses ; to these we must limit ourselves and not yet talk of certitudes — even approximate.

Let it be added that, because of the imperfect character of instruments used in the first era of oceanography, observations collated can be compared and used only after thorough analysis and correction. This work has been accomplished with great care by G. Wüst and his followers : conversion of density measurements to salinity measurements, reduction of temperature measurements taken with maximum-minimum thermometers and with reversing thermometers.

By his critical analysis G. Wüst has been led to consider, in deep-sea motion, only the potential temperature and not the temperature observed *in situ.*

In fact, in consequence of increment due to pressure in deep layers, the temperature is always higher in those layers (by some tenths of a degree, occasionally by one or two degrees) than it should be. For the temperature *in situ* therefore, G. Wüst substitutes the temperature which would be observed if the water, reduced to surface, were affected by atmospheric pressure only — which is the *potential* temperature (18).

Thus G. Wüst excludes all purely static consideration of temperature increase in deep-sea layers; he takes into account only *dynamic* contingencies revealed by potential temperature differences.

Can such a separation between *statics* and *dynamics* be allowed ? Do not higher temperature differences, in which pressure has a role, exercise their influence on the lateral transfer of deep layers, depending on the depth ?

Here I simply submit the problem, without pretending to solve it.

The limited documentation, the sources of which I have just mentioned, was first used for the *Atlantic Ocean*, the least unknown of the great Oceans.

In 1925, for the *Meteor* Expedition and based on prior observations, A. Merz prepared a diagram of Atlantic deep circulation; this hypothesis was to be confirmed or corrected by the *Meteor* observations (19).

In the zone which a little later was named the *stratosphere*, Merz identified three great lateral movements of the waters :- from 600 to 1200 metres, relatively light and cold waters coming from southern high latitudes (antarctic intermediate current); from 1200 to about 4000 metres, waters from the North Atlantic (Sargasso Sea), with a rather important salinity increment and a smaller temperature increase (North Atlantic deep current); from 4000 metres to the greatest depths, very cold and slightly less saline waters from southern high latitudes where their temperature gradient causes them to be propagated in the depths (antarctic bottom current). Hence large sheets of water diffused in the direction of the parallels without noticeable appearance of vertical convection and mixing except in zones of origin.

This system of figuration appeared almost confirmed by the first results of the expedition as published in 1926 and 1927 by the *Zeitschrift des Gesellschaft für Erdkunde.* These results appeared sufficiently general and sound to allow of seeing in them general laws of ocean circulation, applicable to the Indian and Pacific Oceans as well as to the Atlantic.

Soon, however, the vast simple concepts of Merz were put to the test of reality.

An attentive study of *Meteor* results as given in the Reports of the Expedition bring to light irregularities and new facts irreconcilable with the primitive diagram.

G. Wüst acknowledged that it had been a mistake to neglect the *vertical circulation* to which the old Buchanan theory on the convection of warm tropical offshore waters attached great importance and the existence of which seemed to be proved by the cooling of the waters observed along coasts and on shoals (20). The new fact of *internal tide waves* discovered by O. Pettersson in the Kattegat and the North Sea was confirmed by the *Meteor* in the tropical Atlantic. A. Defant and, to a greater degree, Ekman hesitate to connect these vertical oscillations to tide generating forces; in any case they have scarcely any ascertained relationship with surface tides. One thing, however, can not be refuted :- the pressure in the vertical sense which must be integrated into deep circulation (21).

The new role played by the oxygen index, which was revealed by *Meteor* investigations led also to an alteration of the primitive diagram. G. Wüst used the new index not only to determine the boundaries between the deep-lying sheets of water, as the *Meteor* observations had done, but to define a few of those sheets themselves.

For the general definition of the strata of the *stratosphere*, G. Wüst, temporarily abandoning the Bjerknes method, groups the temperature, salinity and oxygen observations, which enables him to determine the liquid control layers (*Kernschichten*) and the principal set of the deep currents (22).

These directions, whether N-S or S-N are eight in number; as we have seen, there were only three in the original Merz diagram (23).

Without taking up in detail the determinations of G. Wüst we draw the attention of the reader to the following points :-

1. As already indicated, G. Wüst acknowledges that in the Gulf Stream zone the

circulation of the *troposphere* extends to deeper levels than anywhere else (to 1500 metres at least). In other words, the independent circulation of the *stratosphere* would appear to be moved down lower than this isobath which practically means that there appears to be undeniable connection in this area between troposphere and stratosphere. Now, the Gulf Stream area is the best known of all oceanic areas; it is the only one for which we possess fairly numerous depth measurements. Is it not possible that, in proportion with our increase of knowledge, facts of the same nature may be disclosed in other parts of the Oceans ?

2. G. WÜST rightly indicates that in the western part of the Atlantic, the bottom currents are much more marked than in the eastern part. In the West, a precise and definite set; in the East, weak and uncertain diffusions. He attributes those differences either to bottom morphology or to deviation resulting from earth rotation (Coriolis component). However, the case is exactly the same at surface and subsurface, where the currents are much stronger in the West than in the East. Do we not find there an indication of a fundamental structural identity, in spite of the extreme difference in intensity of the phenomena ?

3. According to G. WÜST, the waters of Mediterranean origin are the principal cause of the salinity excess in depth, to 65° W. and 20° N.; also, the intrusions of those Mediterranean waters are recognisable to within the Southern Ocean. Such opinions appear to us exaggerated. Not only can the discharge from Gibraltar give, according to NIELSON, no more than 56200 km^3 yearly of saline water at 38‰ (which is a small quantity for the volume of the Atlantic waters) (24), nor is it that these waters (38‰ at 300 metres) fall rapidly in the Eastern North Atlantic to 36‰ at 1200 metres, which, says C. O'D. ISELIN, puts these waters "in a very diluted state", but the Coriolis component, the importance of which WÜST acknowledged for the deep as for the surface currents, can only transport the deep Mediterranean waters towards the North.

4. G. WÜST is fully justified in recognising the importance, hitherto too much neglected in Germany, of vertical exchange; he is right also when he rejects in this connection the theory of the *immiscibility of liquid bodies* upheld in France by Ed. LE DANOIS. It seems to us, however, that it is not only for the vertical exchange of water but also for the lateral exchanges that the phenomenon called by the Americans *internal mixing* occurs on a greater or less scale and according to variations the laws of which are as yet wholly unknown to us. Will it be possible to represent this phenomenon by diagrams ? For the moment, WÜST thinks not — and we agree with him. The study of these phenomena must be made under the sign of the idea of *turbulence* of which we shall speak presently. The future, however, must not be despaired of: in many cases the *secondary surface and subsurface gyrations* which we have studied and which are merely turbulence phenomena, can already be represented (25).

For lack of sufficient documentation it is scarcely possible at present to trace, for the *Indian Ocean* and for the *Pacific Ocean*, a representation of the deep currents.

Still, it can already be stated that between the laws of their deep circulation and those which govern the Atlantic circulation, the differences are great.

There exists only one feature common to all three Oceans, namely, the diffusions of cold deep waters from the Antarctic or rather from the borders of the Antarctic continent. There are certainly, however, great inequalities, either with regard to the volume or to the set of the waters, between the contributions diffused towards the three Oceans, and concerning those inequalities we as yet know nothing.

After the *Meteor* Expedition, however, the diagram of the three superposed deep currents drawn up for the Atlantic by A. MERZ appeared to many minds, especially in Germany, a planetary fact of general application verified by the few observations made in the other two Oceans (26).

Reservations and contradictions were, however, soon formulated.

For the Indian Ocean, in which was conceded the existence of a warm N-S middle-depth current from the Red Sea comparable to the warm current issuing from the North Atlantic, Helge THOMSON proved by means of the *Dana*, *Planet* and *Snellius* observations that those waters did not go beyond Lat. 20° S. (27); the *Discovery II* observers reached the same conclusion and advanced the opinion that the waters with salinity excess found farther South originated in a deep-sea diffusion from the Atlantic (28).

With regard to the Pacific, G. WÜST acknowledges that it is difficult to arrive at determinations, not only because of the scarcity of observations but also because of the less clearly defined characteristics distinctive of this vast Ocean — a perfectly just remark: as a necessary corollory we shall add that the rhythm of the deep circulation is doubtless much slower in the Pacific than in the Atlantic. In the South Pacific there exists no *thermocline*, but a regular fall in temperature. The salinity values are more uncertain than in the Atlantic, the Pacific is relatively poor in salt and becomes more and more so towards the North. It is not astonishing that G. WÜST finds the principal articulations of the deep circulation weak. I cannot, however, explain why he states that in the Pacific there is a greater influx of northern polar waters than in the Atlantic since the Pacific is almost closed to northern polar intrusions both surface and deep. G. WÜST regards those waters as coming from the Sea of Okhotsk. In my opinion in doing so he assigns an exaggerated role to this medium sized sea as in the case of the Mediterranean in the Atlantic (29).

The observation made by A. G. CLEWES of *Discovery II*, namely, that the middle-depth salt waters of the South Atlantic come from the South Pacific, appears to me of much more interest. Let us examine this observation side by side with the question of the Atlantico-Indian deep current already mentioned. That gives us a W-E depth movement over the greater part of the circuit of the Southern Ocean. Now, this set in depth is exactly the same as that at surface and subsurface — hence a new reason why the two circulations should not be too sharply separated.

Remarks of capital importance are also due to SVERDRUP of the *Discovery II* Expedition with reference to the subantarctic convergence line where, towards Lat. 50° S., A. DEFANT states the *stratosphere* to be level with Ocean surface. SVERDRUP remarks that the deep circulation features on this area do not correspond well with the system of lateral currents (intermediate antarctic current), established according to *Meteor* observations. SVERDRUP observes that this system must be developed. Towards 900 metres, the subantarctic intermediate current merges with the complex vertical spiral movements, when it becomes very difficult to determine the limit between the troposphere and the stratosphere (30).

The movements reported by SVERDRUP are turbulence movements; in areas where they are observed, they appear to constitute the predominant part of the circulation.

IV. *THE IDEA OF TURBULENCE.*

The idea of turbulence and its integral facts have already attracted the attention of oceanographers. EKMAN, JACOBSEN and FJELDSTAD have tried to define it by mathematical symbols. Recently a young Russian writer, W. STOCKMANN, continued this work on the basis of the vertical stability of the liquid layers, and adding to it a mixing coefficient and a coefficient of virtual viscosity (31).

It would seem to me a better method, before calculating the *theoretical causes* of turbulence, to search for all of its observable indications; by that I mean the observational facts which are the proof of molecular perturbations in the liquid layers; for those are the perturbations which constitute turbulence. They are numerous, and embrace equally submarine relief, oceanographic chemistry and marine biology.

From the point of view of submarine relief, echo sounding more and more proves the existence of a sea bottom as uneven as dry land. It is scarcely to be doubted that those protuberances may cause eddies in the slow movements of bottom waters. In this connection a certain analogy may be drawn between the atmospheric *troposphere* and the so-called marine *stratosphere*; in both eddies are caused by the unevenness of the soil. Practical meteorological studies in the interests of air-navigation and numerous observations and experiments made by aeroplanes themselves have shown the magnitude of the eddies (upward-flowing and gyratory currents) due to the relief of the soil (32). Certainly the same phenomenon occurs in the great sea-depths, taking into account always the differences of fluids and of pressures. It must even be believed that the influence of submarine relief is not limited to deep-lying turbulence eddies; its repercussion extends to the great middle-depth liquid diffusions, as the investigations of the *Armauer Hansen* have proved for the North-eastern Atlantic (33) and those of the *Atlantis* in the North-west Atlantic.

The recent research work of oceanographic chemistry, by revealing the magnitude of the concentration or deficiency of certain bodies in solution in sea water, such as phosphates and nitrates, leads us to suppose that other indications, if not causes of eddying movements may be found, which escape all other process of observation, principally with reference to *secondary gyrations* (as we call them), to *complex spirals* (as

SVERDRUP calls them) which exist at the margin of large current-layers. Here, however, for the determination of turbulence movements, oceanographic chemistry is in close connection with marine biology. It is the different behaviour of the forms of organic life of the plankton and microplankton which determines the chemical transmutations of sea water, or which is determined by such transmutations. This transport of living to inert, and from inert to living matter is not accomplished as in a closed vessel, in a calm environment; it is accompanied by perturbations, by turbulence which, in all likelihood, are accentuated at the borders of the great currents. We find an interesting indication of this in the inequalities of bacterial life. In sea water, the number of bacteria for 1 cm^3. may vary from 0 to 29400; the highest numbers being found on the borders of ocean currents, in places where those currents come into contact with other waters of a different nature. "The high numbers", says Selman A. WAKSMAN, "are due either to the masses of plankton organisms which die on the borders of the currents, or to a mixing of the waters in a vertical direction at the contact point of two currents from which there results a rising of the bacteria from lower towards higher levels (34).

The extreme inequalities in density of deep-sea life also without doubt influence the turbulence movements of deep-sea waters: it is scarcely conceivable that a *liquid body* composed almost entirely of a living mass and a contiguous body almost azoic should remain in a state of equilibrium without eddying perturbations. Since W. BEEBE's submarine explorations at the Bermudas, we begin to suspect that, even in middle-depth waters, there are submarine deserts and islands of living organisms separated from each other (35). We believe therefore that the determination and the representation of turbulence motion, if ever they become possible can be made only with the co-operation of all sciences pertaining to the sea.

V. *CONCLUSION.*

To summarize, recent investigations on deep ocean circulation tend towards the constant improvement in the means of observation and determination now available. Nevertheless our information even for the Atlantic is by far too scanty; so far it justifies no more than working hypotheses. That concerning large layers spreading out to great distances, formulated for the Atlantic in particular, has already called and will doubtless call for more and more limitations and reservations — for instance with regard to the role attributed to the Mediterranean and to antarctic waters. The vertical circulation features and the turbulence features will take the place which properly belongs to them; it will perhaps be necessary to revert, with the appropriate changes, to BUCHANAN's theory on the convection of warm, saline, tropical waters and of the welling up of cold waters on coastal and shoaler areas. Doubtless the unity of the oceanic circulation from the subsurface to the greatest depths, will be recognized. The mathematical treatment of the facts of marine dynamics is possible only in the case of clearly defined bodies of water (corps liquides, Wasserkörper): this does not appear to be the most usual case.

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