I have chosen a particular case, but it is manifest that the equation

$$(\alpha+\beta x+\gamma^{x}+\zeta x^3)\frac{d^2y}{dx^2}+(\alpha'+\beta' x+\gamma' x^2+\zeta' x^3)\frac{dy}{dx}+$$
$$(\alpha''+\beta'' x+\gamma'' x^2+\zeta'' x^3)y=0$$

could be treated in the same manner. There will be eight possible forms of solution of the class we have here considered, but in practice the number of trials will be much reduced if we do not consider the incommensurable roots of (n).

XI. "On the Undercurrent Theory of the Ocean, as propounded by recent explorers." By Captain Spratt, C.B., R.N., F.R.S.

Received June 15, 1871.

The universal undercurrent theory so fascinatingly advocated by Maury and others, and more recently by the late Dr. Forchhammer, has now been so remarkably supported and maintained in the enlarged views pronounced by Dr. Carpenter in his recent papers and lectures before the Royal and other societies, and is of so interesting and important a nature in connexion with the study of the laws regulating the natural history and geological results of the past, as well as of the phenomena in progress in the ocean and seas in communication with it, that the assumed facts and data upon which they are based deserve, and indeed require, in the interest of sound science and philosophy, to be carefully considered and analyzed before they can be accepted as a grand law such as is implied in the views or theory.

Dr. Carpenter has put forward certain axioms as "propositions" or fundamental principles, as necessary results from the influence of rivers and rain, temperature, evaporation, and density upon the surface and deeps of all seas that are in communication. I feel it necessary to give the more important of these, as being the basis of his theory*.

"No. III. That wherever there is a *want of equilibrium* arising from *difference of density* between two columns of water in communication with each other, there will be a tendency towards the restoration of equilibrium by a flow from the *lowest stratum* of the denser column towards that of the lighter, in virtue of the excess of pressure to which the former is subjected.

"No. IV. That so long as the like difference of density is maintained, so long will this flow continue; and thus any agency which permanently disturbs the equilibrium in the same sense, either by increasing the density of one column, or by diminishing that of the other, will keep up a permanent flow from the lower stratum of the denser towards that of the less dense. This constant *tendency to restoration of equilibrium* will keep the actual difference of density within definite limits.

"No. V. That if there be at the same time *a difference of level* and an

* See *antea*, p. 211.

excess of density on the side of the shorter column, there will be a tendency to the restoration of the *level* by a *surface*-flow *from the higher to the lower*, and a tendency to the restoration of the *equilibrium* by an *under*-flow in the opposite direction *from the heavier to the lighter* column."

Dr. Carpenter then refers to the observations and opinions* of the late Dr. Forchhammer of Copenhagen, for the conditions and facts that are said or assumed to exist between the Baltic and German Ocean in regard to the interchange of a surface and undercurrent between them so as to restore the lost density of the former caused by a supposed continued outward surface-current, and thus maintain the equilibrium or normal conditions between them.

As I shall be obliged to refer to these opinions as assumed facts hereafter, I must here dwell more particularly upon what has been assumed in regard to the Black Sea and Ægean, regarding which Dr. Carpenter states as follows:—"The condition of the Euxine is precisely parallel to that of the Baltic; and a surface-current is well known to be constantly flowing outwards through the Bosphorus and Dardanelles, carrying with it (as in the case of the Baltic) a large quantity of salt. Now, as the enormous volume of fresh water discharged into the Euxine by the Danube, the Dneiper, and the Don would in time wash the whole of the salt out of its basin, it is obvious that its density can only be maintained at its constant amount (about two-fifths that of ordinary sea-water) by a continual inflow of denser water from the Ægean, the existence of which inflow, therefore, may be predicted on the double ground of *à priori* and *à posteriori* necessity."

The first consideration then is, whether these predictions, as necessary premises to his enlarged theory, are true both for the Baltic and Black seas, since upon them the soundness of the theory mainly depends.

I am therefore induced, as I have been frequently requested to do, to give my experience and knowledge of the conditions of the surface and deeps of the Mediterranean and Black Sea, and next analyze the observations of Dr. Forchhammer to see whether his opinions and conclusions are true in regard to the Baltic, viz. that the density of the Baltic is maintained by an undercurrent from the denser waters of the Kattegat and German Ocean. For it is through my knowledge and experience of the conditions existing at the Black Sea Straits and Ægean Sea, from repeated experiments in them on temperature and densities, &c., that, with all due deference to the judgment of Dr. Carpenter, I am obliged to regard those advanced by him as fallacious, and to maintain that, in adopting them, he is advocating a theory upon erroneous premises. It is therefore necessary for me to briefly show here the nature and extent of these investigations, either published or unpublished, in support of my reasons for so differing from Dr. Carpenter.

* Philosophical Transactions, 1865.

In August 1848 I read a paper at the Swansea Meeting of the British Association*, "On the Influence of Temperature upon the Distribution of the Fauna in the Ægean Sea," as an explanation of the zones of animal life in that sea, which had been discovered by Edward Forbes a few years previously, when we were employed together in the 'Beacon.' I then showed that the temperature in depths below about 100 fathoms, although sometimes in midsummer trenching down to between 200 and 300 fathoms, was always uniform, the minimum of $55\frac{1}{2}°$ Fahr. (as the thermometers then gave) being reached at that depth; and this fact I had discovered in 1845 to exist in both divisions of the Mediterranean, although in the latter the minimum was not so low as in the Ægean by about 3° or $3\frac{1}{2}°$. As the thermometers in use at that time were defective in construction for such observations by (as now found) a constant error in excess of about $3\frac{1}{2}°$ or 4°, the temperatures, with this deduction made from them, agree with the recent observations of Dr. Carpenter. Also, in a paper published in the 'Nautical Magazine' for January 1862, "On the proper Depths for Deep-sea Cables," as the result of my experience gained after conducting the laying of the Varna and Crimean Cables across the Black Sea, and others in the Levant, and also between Malta and Alexandria, when the temperatures were constantly taken, I stated the following interesting facts.

Extract from the 'Nautical Magazine,' January 1862.

"The Mediterranean temperatures are known to be not very low at great depths, but reach their minimum as a permanency in from 100 to 300 fathoms; and this minimum temperature seems to correspond with the average annual temperature of the locality itself. And as the Mediterranean is divided into a series of basins, with comparatively intermediate shallows, it is its surface waters, about the depth of 200 or 300 fathoms (being that of the barriers which separate them), that unite by their superficial and encircling currents. Thus, as the depth across the Strait of Gibraltar is under 200 fathoms, the very cold waters in the deeps of the

* British Association Report, 1848 (Swansea Meeting), Sections, p. 81. Also, see Edinburgh Philosophical Journal, 1848, in which are the following remarks:—

"It is temperature, and local conditions partially arising from it, that limits the elevation and existence of animal life. So does the same law appear applicable to marine animals, which breathe the medium they inhabit.

"As a law resulting from this influence, characteristic, tropical, and subtropical species will have a limited distribution in geographical space, whilst the boreal and subboreal characters will be found in every geographic position, where corresponding regions of depth are found with animal life existing, the limit of which I believe to be much lower than 300 fathoms, having examples from 390 fathoms. But I must notice that the Ægean deep dredges indicate generally a zero of animal life at 300 fathoms, as Professor Forbes was induced to assume. I believe, however, that in the deserts of yellow clay an occasional oasis of animal life may be found in much greater depths, dependent upon some favourable local condition or accident."

Atlantic, or of the Black Sea, do not intermingle, and exert their individual temperature in the depths of the central basins. The temperature of the deeper waters of the Mediterranean, Archipelago, Sea of Marmora, and Black Sea are consequently each dependent on local influences, namely from the solar or atmospheric temperature above them. Therefore the minimum temperature of their deeper parts corresponds nearly with the mean annual temperature over them.

"In the Grecian Archipelago I long since showed it to be constant at about 55° in depths from 100 fathoms downwards. In that sea the temperature of the intermediate depths between 100 fathoms and the surface in the summer season ranges from 55° to 76°, and, indeed, even up to 80° and 86° sometimes, in the littoral waters of enclosed gulfs and shallow bays.

"In the eastern and western basins of the Mediterranean it will have consequently a higher minimum temperature than that; and I find that it is about 59° in all depths from 300 down to 2000 fathoms. But between the depths of 30 and of 300 fathoms there is an increasing variation from that temperature to 73° and to 75° in the summer months, but confined more particularly to the depths between 100 fathoms and the surface.

"But in the winter months of December, January, February, and March, the upper depth is nearly at the minimum temperature of the deepest part below, namely from 59° to 62°, varying with the locality and *depths* of water there.

"Thus it is that in these months the surface and deep waters of the Mediterranean are at a constant temperature of about 10° or 15° above that of the atmosphere.

"After the month of March, however, the solar influence begins sensibly to raise both sea and atmospheric temperatures, so that in July, in the southern part of the Mediterranean, it is at its maximum of about 75° from the surface down to the depth of about 30 fathoms."

Having been thus brief in stating the facts obtained in regard to the distribution of the temperatures of the deeps, I shall also be as brief as will be consistent with the due illustration of the more important facts and results in describing the observations for ascertaining these surface- and under-currents in one or two of the localities in question, viz. the Dardanelles and Bosphorus, where Dr. Carpenter has assumed and predicted conditions as an absolute necessity, and upon which predictions he has mainly founded his enlarged views and theories.

Now it will easily be realized on consideration, that the testing of surface-currents for their various rates in different depths, or of under-currents of small amount where they exist, in proof of this theory, as a general fact, is an experiment that requires much delicacy and nicety in the mode of operation and the means by which it is attempted or effected. I therefore never attempted such experiments by the use of any bulky object, such as a boat, that offered both great resistance to the surface-

current, and that was also easily affected by the resistance it offered to the wind and swell, and consequently the counter drift resulting from them, and thus necessarily tending to mask the more delicate movements in the deeps and to vitiate the results.

I felt, too, that a fixed object as a point of reference was always necessary, such as a buoy or float attached to a sinker actually upon the bottom.

Such observations for testing ocean-currents, then, should only be made in connexion with a *fixed object attached to the bottom*, whether in 2000 fathoms or 20 fathoms, as I have before recommended both in the 'Nautical Magazine' and in the Appendix to 'Researches in Crete,' when treating on the question of undercurrents in the Dardanelles and Ægean, &c., from which latter work I shall have to quote a few passages in proof of the actual conditions existing there. I am therefore induced here to express my regret that the same means were not adopted in the Straits of Gibraltar, especially as two or three good opportunities offered when the dredge had got irrecoverably entangled at the bottom.

I now give the following extracts referring to the observations made in the Ægean Sea, Dardanelles, Black Sea, &c., to test the rate and depth of the current generally flowing from the latter into the former, as also the different densities of these seas and straits; and the results arrived at will be seen to have a very interesting and important bearing upon the theory and question at issue.

The densities were tested by an hydrometer, which, having a range from about $13\frac{1}{2}°$ as the normal condition of the surface of the Black Sea, to 29° as the normal of the Ægean and Mediterranean seas, served to show the varying densities in different depths and localities between them with sufficient accuracy, without need of more elaborate analysis, such as might be necessary in the Straits of Gibraltar, where the difference is only about $1\frac{1}{2}°$.

Having carried out these observations at different depths in the Sea of Marmora and the Dardanelles, from below the Dardanelles Castles, a very interesting fact was ascertained,—namely, that nearly in proportion as the descending superficial current from the Black Sea diminished, so did the saline density of the water increase; and where there appeared to be no current (that is, below 40 fathoms in the Sea of Marmora, and below 20 fathoms in the Dardanelles) the density remained the same at all depths, and was that of the Mediterranean density.

Thus, in the Sea of Marmora, the density of the water in 40 fathoms, and from the bottom at 400 fathoms, was the same, viz. 29° by the hydrometer, and about corresponded with my observations on the Mediterranean density in general down to 2000 fathoms, except in one instance, Crete and Lybia, when it was 30° at that depth, and at another only $28\frac{3}{4}$ at the depth of 1200 fathoms, with $29\frac{1}{2}$ above*.

Then in the Dardanelles the current was found to cease at 20 fathoms,

* See Crete, vol. ii. p. 332.

and the maximum, or Mediterranean density of 29°, was found to be constant from that depth downwards, whilst the surface showed the same density as that of the Sea of Marmora, viz. 20°, the Bosphorus being about 14°, and Black Sea surface $13\frac{1}{2}$°, and about 15° below 100 fathoms. Therefore in this part of the Dardanelles, between the surface and the depth of 20 fathoms, there was an increasing density from 20°–29° as the result of the intermixture in the deeps of the narrows and from the encircling eddy or return-current by the south shore in that part, as invariably occurs; and in proportion as the density of the water increased to that depth, so did the rate of the current decrease, as shown in the following Table:—

Depths.	Density.	Temperature.	Current.	Remarks.
Surface	20°	50°	Rate of $2\frac{1}{4}$ knots.	At twenty miles westward of the Dardanelles, in the Ægean, the surface-density was the same as the Mediterranean.
5 fathoms.........	22	50	Rate of $1\frac{1}{2}$ knot.	
10 fathoms.........	25	53	Rate of $\frac{3}{4}$ knot.	
15 fathoms.........	27	55	Rate of $\frac{1}{4}$ knot.	
20 fathoms.........	28	55	Rate very slight.	
40 fathoms.........	29	54	No current.	

The plan I adopted for ascertaining whether any such undercurrents existed, as well as of the rate of the surface-current in descending depths, was as follows:—

A suspended sinker, or current-anchor, was made in several forms to test the most simple and effective form. Sometimes a vertical cross was used, which was formed of boards, so as to offer a resisting surface every way when hanging vertically with the weight or lead attached to its base. More generally a large weighted disk or enlarged log-ship was used, and when weighted with a weight consistent with size of the disk, and also of the line and float, was slung like a kite, the weight being in the place of the tail. When *thus slung* it would of course hang nearly vertical in all depths, and offer a sufficient resistance to prevent its being moved by the friction of the surface-current upon the float used to suspend it. It will be thus seen that the operation of testing slow currents in the deeps is one of great delicacy, and therefore requires great nicety and care in the mode and apparatus for doing it, for scientific dependence and aims.

The float that, after many experiments, I found to answer best was one made of thin copper or block-tin, in the form of an elongated air-tight cylinder, 4 or 5 feet in length, and pointed at *one* end to offer least resistance to the surface-current passing it. The other end was truncated or flat, where two loops were fixed in which a small rod or staff with a flag could be placed, to render its position conspicuous, without adding to its resistance or weight. The suspended kite, or current-anchor, was then weighted to about one-third of what the float would bear in perfectly still water, without being wholly immersed.

*Remarks and Experiments on the superficial and supposed undercurrents of the Mediterranean, &c.**

It can be easily understood that, if a superficial current of 1 knot is observed to pass a float attached to a line which has a sinker or anchor at the bottom, and also if the same amount of surface-current of 1 knot passes a float which is attached to a suspended sinker or current-anchor suspended halfway or at any depth, the sinker thus held suspended by the surface-float is evidently as stationary as the one at the bottom, and therefore it must be in perfectly still water, whatever the depth may be; consequently the superficial current does not descend to that depth.

Also, if another suspended sinker or current-anchor is lowered down a few fathoms (say 10 fathoms) from the surface, and the float attached to it has *no current* passing it, and consequently drifts away from the stationary float attached to the bottom and near which it was lowered, it is quite clear that the suspended sinker and its float are within the same influence, in fact in the same amount of current.

Again, if the suspended sinker be lowered to 20 fathoms by the side of the stationary float, and a current of about half a knot be then observed passing its float, although still drifting away from the stationary float, then, as this latter float showed a current of 1 knot passing it, and the float of the suspended sinker in 20 fathoms only showed a current of half a knot, it is also clear that the current-anchor or suspended sinker was in a current of only half the speed of the superficial current, viz. of half a knot only.

Also, if the suspended sinker be lowered to 50 fathoms, and the superficial current passing its float be three-fourths of that passing the float attached to the bottom, or running three-quarters of a knot, it is evident that the current at the depth of 50 fathoms was three-fourths less than the surface-current, or only running at the rate of one-fourth of a knot.

In this manner, then, my experiments were carried out at different depths, and at different times, in the Archipelago, Sea of Marmora, and Dardanelles, as being favourable positions for testing the superficial currents, and also of the existence of undercurrents, if any existed in these straits and seas, as some have supposed.

Thus, on the morning of December 19th, 1857, I hove to in H.M.S. 'Medina,' between Rodosto and Marmora Island, near the eastern entrance to the Dardanelles, and from a boat sounded with a shot and seine-twine in 350 fathoms; I then attached to the twine a piece of light wood as a stationary float. The superficial current was then tested by the common log-reel, run out from a boat kept stationary abreast of the stationary float, when a current of 0·9 of a knot was observed to be running towards the Dardanelles.

Experiments for trying the rate of the current at different depths were then made in the following manner:—A flat piece of wood like a log-ship

* Travels and Researches in Crete, vol. ii. p. 333.

on a large scale was weighted with a piece of lead of about 4 pounds, and slung by its corners like a kite, so as to act as a suspended anchor or sinker, and was lowered to a depth of 5 fathoms; and as no current was observed passing the float when the sinker was at this depth, it follows that both float and sinker were in the same amount of current, or in the upper stratum of the current; that is, both were drifting along in a current of 0·9 of a mile per hour.

It was then lowered to 10 fathoms, when a sensible current was observable passing the buoy or float, which measured about 0·3 of a knot per hour, or just one-third of the rate of current running past the float attached to the shot at the bottom in 350 fathoms; therefore the rate of current at 10 fathoms was ascertained to be only 0·6 of a knot per hour.

The suspended sinker was then lowered to 20 fathoms, when there appeared a much greater amount of current passing the float, and the rate was found by the log-line to be about 0·5 of a mile per hour, thus showing that the float of the suspended sinker was held in check by the sinker being in a current about half that of the surface-current, or running at only about 0·4 of a mile per hour at 20 fathoms' depth.

Again, on lowering the sinker to 30 fathoms there was immediately observed an increase of the superficial current passing its float, showing, therefore, a still diminishing current as the suspended sinker descended, since it was thus kept more stationary.

Then at 40, 50, 100, 200, and 300 fathoms the rate of the current passing the float of the suspended sinker was about 0·8 of a mile,—that is, nearly that of the surface-current when in all depths below 40 fathoms, so that an outward current of 0·1 of a knot per hour would appear to exist there; but in reality this was the result of using in this instance a too bulky float, by which the suspended sinker was dragged along at that rate; still water, therefore, undoubtedly existed below 40 fathoms, as confirmed by the density experiments and others in those depths.

This result was given to show the confusion and error almost sure to arise from using bulky apparatus as a float, that offered too much resistance to the surface-current; and a double source of confusion and error is the sure result if the observation is also carried out with any wind and sea.

No undercurrent could therefore have existed here on the eastern approach to the Dardanelles as many have imagined; for an undercurrent being an opposite current to the current observed running past the fixed float, the current then observed running past the float attached to the suspended sinker or current-anchor would have measured a greater rate than that passing the fixed float. Moreover, also, as the suspended sinker would have been dragged along by the undercurrent in an *opposite* direction to the surface-current, its float would have presented the singular phenomenon of going to windward of the fixed one; or, in other words, *would have run up against the stream instead of down with the surface-stream.* This, on a slight consideration of the phenomena, will be evident, and the delicacy

of the operation too, especially when testing any very slow ocean-current, of only 0·1 of a mile per hour or less, which is quite practicable, as I have frequently done. And it is rendered sure and easy by always having a fixed float with sinker at the bottom as a point of reference, even where objects are near and charts are correct, and also by using very fine twine as a log-line to each float, and allowing it to run out from five to ten times the usual interval in measuring a ship's rate. The diagram on p. 537 will illustrate the matter*.

The diagram referred to will illustrate the plan, and the result will be more comprehensible and satisfactory, because sooner completed, if we suppose the trials to have been made from two or three boats (instead of from only one boat), each being provided with one or two buoys and suspended sinkers to suit, and with lines to each arranged for different depths; and if also *a fine log-line is attached to each float* for measuring the distance it drifts in a given time, and from it the rate of each float, the boats being always kept abreast of each other. For although each float, from the different times each suspended sinker will take to reach its intended depth, will have drifted away to some small varying distance from the boats and stationary float, these varying intervals will correspond to the stray line paid out in measuring a ship's rate through the water; and being noted in the usual way (as the rate-lines will be duly marked at intervals of 10 feet), or by a piece of cork or rag attached to each, when at the given signal the interval, by watch or glass, is simultaneously commenced to be noted, the deduction of this stray portion from the whole length run out in the arranged interval of five or ten minutes or more, according to the speed of the current, will give the surface-rate of each, and the consequent rates of the currents in which the suspended sinkers are lowered are easily deduced from them.

Now all these observations showed no undercurrent into the Black Sea, such as Dr. Carpenter maintains must necessarily exist to restore the saline density of that sea; if, therefore, I can show how that density is otherwise maintained, and by a more natural and more universal process and influence in connexion with ocean movements in all seas, the theory of universal undercurrents, as a great circulating medium for recovering the equilibrium, is deprived of its main support,—the main ground upon which it is advocated as a predicted necessity.

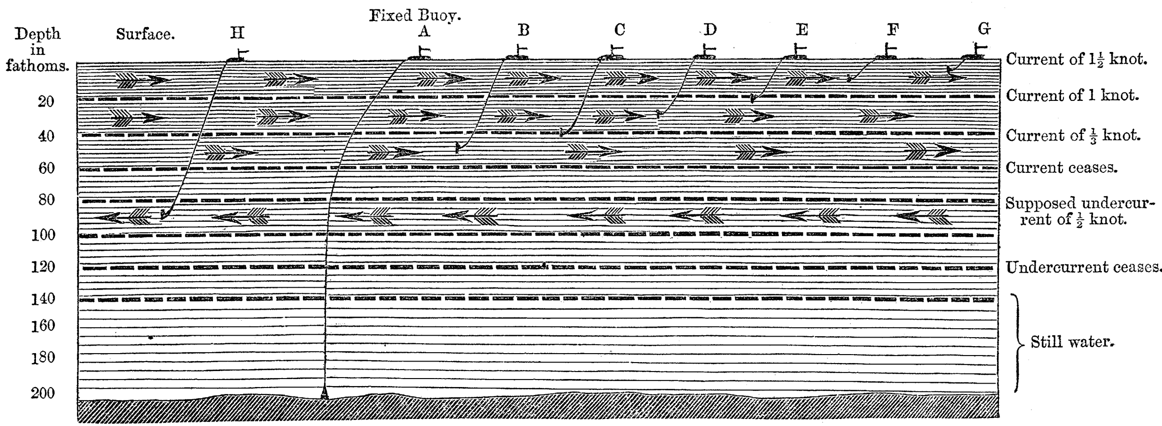
To better understand the remarks and facts that will follow upon the densities and currents of the Black Sea Straits, I must briefly notice the physical features influencing them.

First, the Black Sea attains a depth of about 1000 fathoms and more over a large portion of its area. The Sea of Marmora attains between 400 and 500 fathoms, and the Ægean about the same; whilst the Dardanelles and Bosphorus do not exceed 20 and 40 fathoms.

The facts, then, are simply that, although a diluted current of Black Sea

* Crete, vol. ii. Appendix, p. 338.

Diagram showing the mode of testing the existence, direction, and rate of surface- and undercurrents in the deeps of the sea.



B, C, D, E, F, G, and H, relative positions of the floats to each suspended sinker, after being simultaneously dropped near the fixed float, A, and allowed to drift in their respective currents for five minutes.

Thus, if a surface-current of $1\frac{1}{2}$ knot was found to be passing the fixed float A, and the floats B, C, D, E, F, G had reached those positions at the end of five minutes' interval, their suspended sinkers or drags were in diminishing rates of the surface-current; but the float H, from being dragged in the opposite direction, had evidently reached an undercurrent with its suspended sinker or current-drag.

water flows for a great part of the year as a skimming surface movement around and across the Sea of Marmora and through the Dardanelles, and at no greater depth than from 20 to 40 fathoms, viz. that of the barrier ridges between the Black Sea and Ægean, there occurs for several days in the year a strong *reverse current* into the Black Sea from the Ægean. This reverse current is frequent during the autumn and winter months, when the Black Sea rivers are at their lowest, so that the Black Sea level is then frequently overbalanced by the pressure of westerly gales in the Mediterranean or Ægean.

Therefore it is this recurring *return current* into the Black Sea that maintains, as I found to be the curious or interesting fact, the Ægean or Mediterranean density in the deeps of the Sea of Marmora in all depths below about 40 fathoms, and therefore that restores also the lost salinity of the Black Sea through the flow of diluted water so prevalent as a surface-current from it.

This return-current occurs sometimes for two or three days at a time, and occasionally at a rate even greater than the general outflowing current of 2 and 3 knots. The same occurs at the Kertch Straits.

Therefore, instead of the Black Sea being washed fresh, unless there was an undercurrent to restore it, as Dr. Carpenter argues, its normal density is restored by the surface return-current, as also that of the Sea of Azof.

But I am induced to believe, as I have elsewhere stated*, that the Black Sea has not become a diluted or brackish sea from a previously salt sea, but, on the contrary, from a freshwater lake has become a brackish one.

This I infer to be the fact from the latest deposits existing around the shores of the Black Sea, Sea of Marmora, and Dardanelles being of freshwater origin, a large *Dreissena* and a freshwater Cockle in them being mistaken previously for a Mussel and *Cardium* until I discovered the error.

Having thus shown these physical conditions *as facts* in connexion with the Black Sea, and that the undercurrent theory is a fallacy where it was expected and insisted upon as being a predicted necessity, as a constant counterbalance to the surface-outflow, from the great difference in the densities between the Black Sea and Ægean, viz. 13° and 29°, as shown by a common hydrometer, I shall now refer to the Baltic Sea and German Ocean, where even a greater difference in the saline density exists; and I shall be able to show also that precisely the same conditions of outflow and inflow exist there, and that it is surface-currents only which restore the lost salinity, and that no undercurrent is necessary there to prevent the saltness of the Baltic from being washed out; that, in fact, no undercurrent system does exist as a means of restoring the equilibrium and maintaining the normal conditions of that sea. This I shall show from Dr. Forchhammer's observations; but he has evidently mistaken the right

* Crete, vol. ii. p. 349.

conclusions to be drawn from the facts he gives regarding the densities and currents, and thus led Dr. Carpenter to adopt them as follows:—

"Now if it can be shown that a similar *vertical circulation* is maintained in the opposite direction, when the conditions of the case are altogether reversed, the explanation above given may, it is submitted, be regarded as having a valid title to acceptance. Such a converse case is presented by the Baltic, an inland basin which communicates with the German Ocean by three channels—the Sound, the Great Belt, and the Little Belt, of which the Sound is the principal. The amount of fresh water discharged into the Baltic is largely in excess of the quantity lost from its surface by evaporation; and thus its *level* would be continually *raised* if it were not kept down by a constant surface-current, which passes outwards through the channels just mentioned. But the influx of fresh water *reduces* the *density* of the Baltic water; and as the water which the outward current is continually carrying off contains a large quantity of salt, there would be a progressive reduction of that density, so that the basin would at last come to be filled with *fresh* water if it were not for a deeper inflow. Such an inflow of denser water might be predicted on Principle VI. as a physical necessity, arising from the constant want of equilibrium between the *lighter* column at the Baltic end of the Sound and the *heavier* column at its outlet in the German Ocean; and that such an undercurrent *into* the Baltic has an actual existence, was proved two hundred years ago by an experiment of the same kind as that by which we have recently proved the existence of an undercurrent *out of* the Mediterranean. This experiment is cited by Dr. Smith (*loc. cit*) in his discussion of the Gibraltar current, as supplying an analogical argument for his hypothesis of the existence of an undercurrent in the Strait of Gibraltar; but he does not make any attempt to assign a physical cause for the movement in either case"*.—*Proceedings of Royal Society*, vol. xix. p. 213.

I need only add, as a remark to this latter part, that a greater density below was no proof of an undercurrent, as shown by the contrary fact in the Dardanelles and Sea of Marmora.

In his paper in the Philosophical Transactions for 1865, "On the Composition of Sea-water in the different parts of the Ocean," in page 230 Dr. Forchhammer says, "In the Baltic likewise the water from the deeps contains more salt than that from the surface. The upper-current goes generally (not always) out of the Baltic, the reverse of the Mediterranean. The cause is evident, the excess of atmospheric water in the Baltic from the rivers surrounding it..... With the assistance of Captain Prosilius, in 1846, who commanded the vessel stationed at Elsinore, the surface-current was observed on 134 days, from 27th April to 11th September; of which on 24 days it ran from the north, on 86 from the south, and on 24 days there was no surface-current at all."

* "Prof. Forchhammer fully confirms Dr. Smith's statement, and further shows that the water which thus returns to the Baltic has the density of the Sound water, the surface-current being formed of the much lighter Baltic water."

"The mean quantity of salt for the current from the north was 15·994 per 1000; that for the current from the south 11·801; that for the period when there was no current at all 13·342. Once a week a sample was taken from the bottom. .... The mean of nineteen observations was 19·002, which is rather under than above the real mean, and proves that it is water from the Kattegat which runs at the bottom of the Sound."

"Experiments once a week, at Copenhagen, from 3rd March to 25th April, 1852, gave as follows:—for the surface 15·845 per 1000 salt, for the bottom of the harbour 17·546 ditto, which seemed to prove that the undercurrent at that season reached Copenhagen."

In page 222, Dr. Forchhammer also shows the salinity of the German Ocean, the Kattegat, and Sound, and in the eastern and western parts of the Baltic as follows:—

"German Ocean, mean of six analyses, 32·823. In the Kattegat and Sound the quantity of salt is very variable; a northerly wind makes it richer in salt than with a southerly wind. The mean of six analyses and 141 observations, in which only chlorine was determined, gives 16·230 per 1000, the maximum 23·243, and the minimum 10·869. In the Baltic the salinity varies very much, and is of course less in the eastern than the western portions. I found the mean 4·931 per 1000 salt, the maximum 7·481 between Bornholm and Sweden, the minimum at Kronstadt 0·610 per 1000 salt."

The relative saline densities are therefore as follows:—

German Ocean	32·823	mean.
Kattegat and Sound	16·230	,,
Baltic	4·931	,,
Baltic, between Bornholm and Sweden	7·481	maximum.

Dr. Forchhammer's observations are all upon the densities at different depths in the Sound, and some few temperatures at the surface and bottom. There were, unfortunately, none upon the rate of the surface-current in descending depths, as carried out by me in the Mediterranean and Black Sea Straits, or without doubt he would have found the same results, that is, by the fact recorded by him of the density being greater at the bottom of the Sound than at the surface, he would have ascertained a corresponding diminution in the outward rate of current in descent from the surface. But he has inferred, from the fact of there being a slight increase of density at the bottom of Copenhagen harbour, viz. of 17·546 at the bottom and of only 15·845 at the surface, that there was necessarily an undercurrent there from the denser water of the Kattegat and German Ocean; for he says that at Copenhagen the density was "For the surface 15·845 per 1000 salt, for bottom of harbour 17·546 per 1000, which seemed to prove that the undercurrent at that season reached Copenhagen." Thus quite overlooking the remarks he has made upon fluctuations in the saline densities produced at times, both in the Baltic and Kattegat, from the influence of winds, and forgetting, too, "that for twenty-four days out of the

134 the surface-current was running from the north at Elsinore," that is, into the Baltic from the Kattegat and German Ocean, and of course then doing what I have shown to occur in the Sea of Marmora and Black Sea, with every recurring and return-current into them, viz. restoring the lost saline density of the surface and deeps of those seas. Now the proportion of the inward return surface-current from the denser seas is very much greater in the Baltic Straits than in the Black Sea Straits, and, moreover, the depth on the dividing ridges of the Sound and Great Belt leading into the Baltic does not exceed 9 fathoms, or about 50 feet only; and thus, even here (notwithstanding the great supply of fresh water from the Baltic rivers into the Baltic between April and September, when the observations were made, and, therefore, the time of greatest supply in the whole year), the outside influences of wind &c. so outbalanced the surface-level between the Baltic and German Ocean, as to produce twenty-four days inward current in that period, and twenty-four days without any current, then stopping the Baltic outflow in fact. So that Dr. Forchhammer himself shows that the phenomena of the currents to and from the Baltic are, at one time, a mass of diluted water outwards over the very shallow barrier forming the straits, and then a run of the Kattegat denser water inwards for the restoration of the normal saltness. What need, then, was there for looking to an undercurrent as the great source of such a resupply, when it was evidently produced by a greater agency during the twenty-four days return surface-current inwards, when the German Ocean must have stood at a higher level than the Baltic? One more remark is necessary to show another fallacious conclusion of Dr. Forchhammer in favour of the undercurrent theory. In p. 233 he says, "I observed on the 2nd of March, 1850, the temperature of the undercurrent at a depth of 108 feet to be 36°·8 Fahr., while the temperature of the surface was 34°·9 Fahr.,"—thus intimating that because he found a higher temperature and density in 108 feet at Elsinore it positively indicated an undercurrent from the German Ocean or Kattegat, the conditions of density and temperature being those of the outside sea and not of the Baltic. But these were the natural conditions of things there, because in the depth of 108 feet at Elsinore he was in a depth more than twice that of the submarine shallow barrier that separates the Baltic from the Kattegat, and on the Kattegat side of the ridge. The barrier or ridge is at the southern end of both the Great Belt and Sound, and is thirty miles to the south of Elsinore, which is therefore on the German Ocean side of the ridge; and, moreover, the depth of 108 feet at Elsinore was in the greatest depth of the Kattegat anywhere within fifty or sixty miles northward, and the barrier ridge is only 30 feet deep here. The undercurrent view, then, has no ground of support from this fact; but the contrary, for the increase of temperature and density was found exactly where it was natural to expect it, that is, in the region of depths below the comparatively shallow barrier of about 50 feet in depth which separates the Baltic from the Kattegat; and especially so as

it was in the depth of 108 feet, so considerably below the skimming surface-current of lighter water flowing from the Baltic. The denser and warmer water thus found in 108 feet was therefore a continuity of the warmer and denser waters of the deeps of the Kattegat and German Ocean. But the Kattegat being shallow, with a wide entrance between it and the German Ocean, it has no deep trough so much below the separating barrier for retaining still water as in the Sea of Marmora; its waters would, therefore, fluctuate in density throughout, between the densities of the Baltic and German Ocean.

As Dr. Carpenter has made the fallacy regarding the Baltic conditions on the authority of Prof. Forchhammer's statements and opinions the strongest reason and basis of his enlarged theory of universal undercurrents, I must refer to another circumstance, another error of the late Professor in assuming the existence of an undercurrent at the Sound flowing into the Baltic when the surface-current was running out, viz. the fact often observed in the Sound, that ships of deep draught are frequently carried past the lighter draught ship when sailing together into the Baltic through the Sound*.

There is, however, another and more probable explanation of the phenomenon, namely, that the deep-draught ships are, by the lowness of their keels in comparison with that of the light-draught craft, under a lesser influence of the outward surface-current, through the whole strength of the current not descending to the depth of their greater draught, as surface-currents always diminish in descent. The mean strength of a current felt by a vessel drawing twenty feet of water would consequently be less than the mean strength felt by one drawing only eight or ten feet; this will be evident, especially as the surface-current from the Baltic cannot descend much below the depth of sixty feet, viz. that of the Barrier ridge across the Sound and Great and Little Belts, moreover there is no tidal influence there to force or confuse the outflowing surface-current from the Baltic.

Judging from these explanations and facts there appears to be really no evidence, from the observations of Prof. Forchhammer, that an undercurrent is a real necessity for the restoration of the lost salinity of the Baltic any more than in the Black Sea; but, on the contrary, that the evidences and facts are confirmatory of there being no such undercurrent, and no such necessity for one in either case.

Therefore, finding the undercurrent theory fallacious in both instances, I have no faith in its application to the Ocean as a grand law of interchange between surface and deeps, pole and equator, as the great universal movement advocated by Dr. Carpenter. I am therefore induced here, from the apparent importance of some views and facts bearing on the question, to reiterate the following arguments in support of this opinion, which were given elsewhere †, commencing the discussion with the interesting facts regarding the high normal temperature of the deeps of the

* Phil. Trans. 1865, p. 231.

† Crete, vol. ii. Appendix.

Mediterranean as compared with the deeps of the Atlantic Ocean on the west side of the 150- or 160-fathom barrier that separates the one from the other at the western embouchure of the Straits of Gibraltar.

The very high temperature of the depths of the Mediterranean below about 200 fathoms, in all seasons, as compared with that of the Atlantic and Pacific (where, according to Ross, Belcher, Denham, Pullen, and others, it seems to remain at about $39\frac{1}{2}°$ Fahr. * in all latitudes between the Arctic and Antarctic zones) results apparently from its insulation from the Atlantic deeps by the 150-fathom bank or submarine ridge across the entrance of the Gibraltar Straits, and thus appears to have settled into a mean resulting from a small terrestrial influence from below and the large solar influence above, since the normal temperature is constantly at 59° † at all depths below 100 to 200 fathoms.

The fluctuations of temperature in the Mediterranean Sea are consequently confined to this upper zone of about 100 fathoms, in which the temperature varies with the seasons, being in the summer and autumn from 10° to 20° higher than the normal temperature, whilst in winter it rises up at the surface to the normal temperature of 59°—4°, viz. 55°; and is then even sometimes 10° lower at the surface and a few fathoms below it, viz. in January and February, the coldest months.

In the same parallel in the Atlantic the normal temperature of $39\frac{1}{2}°$ —4° is not reached in summer in less than 1000, or in 1200 fathoms in the tropics. This is a peculiar condition of the two seas deserving notice. Had the normal temperature of the Mediterranean been as low as that of the Atlantic, the superficial influence would no doubt have extended downwards to the same depths as in the Atlantic. Upon the first consideration of these facts, however, the inference seems to be, that the Atlantic deeps are under the influence of cooling-down undercurrents from the poles. But appreciable undercurrent movements as a universal movement (such as the theory advocates) I have no belief in, except, probably, where two great streams meet, such as the Arctic current and the Gulf-stream.

It has been well shown, too, in support of this opinion, during the soundings taken across the Atlantic, that perfectly still water reigns in a large area of its deeps, by the fact of the sounding-line, on several occasions, having coiled itself upon the sinker when some 200 or 300 fathoms more than the actual depth had been accidentally or intentionally paid out from the ship, and thus the coils came up in a bunch together *round the deep-sea lead*, around which the line had become coiled as it stood upright in the soft ooze or clay usual in great depths. This result was, therefore, a most excellent test for showing that no appreciable movement or current existed in a very considerable portion of those depths; for it proved that the line must have descended in the lower depths quite vertically when slack, with

* —3° or 4° as the constant error now to be applied to all the earlier deep-sea temperatures.

† —the 3° or 4° as the constant error.

the lead at the bottom, as well as before it reached the bottom, so that no incline of the line from a perfectly vertical course of descent could have occurred for several hundred fathoms above the bottom; all must have been perfectly still water there, for the deviation of the line for a few inches only out of the perpendicular in the lower depths would have prevented the line from coiling itself around the upright lead, and so from this perfect stillness the lead returned to the surface with the excess of 200 fathoms coiled round it*. Now it is perfectly impossible that the ship could have been kept stationary over the same spot in the Atlantic, under the most favourable circumstances, even for a few minutes, much less so during the time a sounding-line takes to descend in about 2000 fathoms; for the combined influence of swell and of the smallest superficial current during this time would drift her from it considerably, as must be evident to every one. Hence it will be perceived that, unless perfectly still water existed in the lower depths, no coiling together of the excess of line paid out around the lead could occur; and as it occurred on several occasions, I have been led to interpret the fact, as did Capt. Dayman, as being a most interesting and satisfactory test, where it occurred, of the perfect stillness of the ocean deeps there.

It is shown by the few soundings that have been taken in the Atlantic, that probably a continuous depth of at least 2000 fathoms extends along it between the Arctic and Antarctic circles.

The consideration of the above points, then, opens up the question of how this low normal temperature of the Atlantic and Pacific deeps is retained in continuity, with a higher terrestrial temperature below, as generally supposed, and a higher atmospheric temperature above—whether it is chiefly, if not entirely, due to the *horizontal conduction of this low temperature* from the Arctic and Antarctic zones and seas during the long ages the present poles have been the sources of cold, combined also with the great density resulting from this low normal state, and consequent tendency of such cold and dense water to remain in the deeps (a view I am more inclined to accept), or whether entirely due to a continuing undercurrent movement between the poles and the equator, as others suppose.

I only touch upon the question here, and thus merely state, in reference to the undercurrent theory, that there seems to be an opposing difficulty in the first thought upon it,—first, because I conceive that the horizontal conduction of extreme cold can evidently occur in a continuing column of equable depth, such as exists in the ocean deeps, and when completely effected remain so, without requiring an appreciable undercurrent to maintain it; secondly, because the existence of such a current seems to require one of two conditions—either a much less density of the substratum of fluid in continuity before it, so as to cause a horizontal flow, or a pres-

* See Capt. Dayman's Report of deep-sea soundings across the Atlantic, pp. 7 & 8.

sure in that direction, from the greater density of the substratum at the source of its origin; but the temperature of the greater depths that are in continuity seems to be of the same low normal condition below about 1000 or 1200 fathoms, so that there is no such difference to set up an appreciable horizontal movement in those deeps.

Although undercurrents undoubtedly exist in the atmosphere, and thus may lead to the possibility or belief in such general movements as a law of the deeps of the sea also, yet the modes in which the solar influence operates upon the two media are diametrically opposite. In the sea the rarefying influence of the sun commences from, and therefore remains at or near the surface, whilst in the atmosphere it commences from below, and therefore disturbs and causes the lower strata to ascend.

The sea is also a comparatively non-elastic fluid, whilst the air is the most elastic, and thus yields to every local influence, whether of heat or cold.

The isothermal temperature of the ocean deeps (viz. about 39½° Fahr.) has been supposed to be that at which the water attains its greatest density, probably because it is found at the lowest tried depths of the Atlantic and Antarctic seas, and because of its being the temperature of greatest density of fresh water; and therefore it has been said that a lower temperature made sea-water lighter, causing it to float upon that at the above-mentioned temperature.

But this is contradicted by the temperatures found by Sir E. Parry, and by the recent experiments of M. Edland, M. Despretz, and others, which seem to show that the greatest density of sea-water is attained between 22° and 25° Fahr.

It seems to me therefore (and I was impressed with the opinion before knowing of this fact and the statements that confirm it) that this isothermal temperature of 39½°—4°, found throughout the Antarctic deeps, is the settled mean temperature produced by the atmospheric influence upon these areas, as about 59° Fahr. is of the eastern basin of the Mediterranean, and about 55½° Fahr. is of the deeps of the Greek Archipelago, and 54° for the Sea of Marmora*—this difference in similar depths occurring in consequence of the separation of the deeps of the two basins by a submerged but comparatively shallow ridge between them, as the Mediterranean deeps are separated from the Atlantic by the shallowest part of the Straits of Gibraltar, and with an isothermal temperature of 59° for the deeps on one side, and of 39½° on the other, subject to the deduction of 4° or 5° from each.

These facts suggest the view that there really may not be an exact correspondence between the lowest temperatures of the Atlantic, Pacific, and Indian oceans, although, when a temperature in excess of or under 39½° has been found, there has generally been supposed, since Sir James Ross's establishment of this as the normal temperature of the ocean deeps to be an error of observation, or a defect in the instrument used.

* —4° for each.

The foregoing quotations, and recapitulations of arguments and reasons from 'Researches in Crete,' which, in my humble judgment and experience, seemed to be sound, in opposition to the undercurrent theory as a grand circulation, and general and appreciable as a fact, I again offer in concluding these remarks, but with all due deference and diffidence, although I am strongly of opinion still, from my practical experience and investigations regarding surface- and deep-water currents, that differences of density and of level are more generally rectified by superficial and littoral movements, than by undercurrents running up hill or burrowing in mid-deeps. But if recognizable or measurable as a physical fact anywhere, it is only local and not universal, and is merely an atomic interchange of insensible amount in general, in the greater depths of the ocean or of inland seas.

On the Gibraltar Undercurrent.

There are also strong reasons for inducing me to dissent from Dr. Carpenter's conclusions as proofs of the undercurrent he asserts to having found indisputable proofs of in the Gibraltar Straits; for to my mind, on carefully considering the observations, as well as the means employed, and circumstances at the one trial (Station 64) which was accepted as an undoubted evidence of such an undercurrent, against the four others that showed no such result, there does not appear to be just grounds for asserting that it really exists, as a positive result of the trials; for in such a question of science the fact should be free of any ground of doubt.

In such a Strait as that of Gibraltar, however, where there are tidal influences combined with the general inset from the Atlantic, an undercurrent at certain times is a possibility; but, with all due deference to Dr. Carpenter, I cannot agree with him in inferring from the single and, to my experience, unsatisfactory result obtained at Station 64, "that a strong presumption may be fairly raised for the *constant* existence of such a return-current, though its force and amount are liable to variation," when the results of his four other trials, viz. two at and near Station 39, and one at 65 & 66, showed no undercurrent, the former being in the narrowest part of the Strait, and the latter over the shallow ridge that unites Europe with Africa, the average depth of which does not exceed apparently more than about 130 or 140 fathoms, although there are depths of 160 near to the African side. It extends across, between Cape Trafalgar and Cape Spartel, the two western capes of the Strait.

The width of the Strait between these two capes is $22\frac{1}{2}$ miles, and the width in its narrowest part near Tarifa is only $7\frac{1}{2}$ miles.

There is therefore a great convergence of the confining coasts and descending slopes to this part, and a necessarily convergence of the Atlantic tidal wave, as well as general inset of the Atlantic current.

In this constricted part of the strait also the greater portion of the depth (fully 5 miles of the $7\frac{1}{2}$ across) is more than twice as great as on

the barrier-ridge to the westward that separates the deeps of the Atlantic from the proper Mediterranean deeps.

Therefore, as there is here a great convergence or concentration of the Atlantic inset, here there would naturally be a deeper tendency of the inflowing current, as well as of an uprising of the lower part of it, where this concentration produced a more rapid commingling of converging waters, and a sort of boiling-up of parts of the deeper waters would be the natural result of this convergence and constriction. Colder waters would therefore come towards the surface, and *vice versâ*.

Now Dr. Carpenter shows this to be the result here, although he does not recognize what appears to me to be the natural and simple explanation of the phenomenon as above given. He says in regard to this :—"It was not a little perplexing to find, when we had fairly entered the Strait and were proceeding along the mid-channel towards Gibraltar, that the surface-temperature of the sea fell still further to 66°·4, whilst the temperature of the air rose to 76°·6, thus showing the then unprecedented difference of 10°·2 between the two ;" and on his return to the same part about two months afterwards, viz. at Station 64, he found the surface-temperature there 66°.

Now, if this be the true and simple explanation of the low surface-temperature over the position of greatest intermixture or boiling-up of the currents there, as I suggest and believe, we should expect the same thing to occur in some parts over the ridge which extends across the western entrance to the Straits, where the Atlantic current or inflow is also somewhat concentrated, *or first meets it as an obstruction*, and thus causes an uprise of the cold water from below to the surface. And, curious enough, the two surface-temperatures taken by Dr. Carpenter at this part, viz. at Stations 65 and 66, show in proof a much lower temperature, only 63° at the first of these, and 69° at the other. The temperature of 66° at Station 64 is therefore clearly a commingling of the uprisen cold waters over the barrier-ridge ; for the mean surface-temperature at 50 miles' distance, on the west side of the ridge, was 72½ degrees, and from about 50 miles' distance from Gibraltar on the east of the Strait it was 73½ degrees, that is, the mean of seven observations taken about the former distance by Mr. Gwyn Jeffreys, and of seven about the latter by Dr. Carpenter. Deductions from temperature and density in such positions as the narrows and over the barrier-ridges are therefore, to my mind, not reliable; I experienced the same at the narrows of the Dardanelles, near the two Castles, and so carried out my observations in more normal conditions or tranquil areas, and in parts free of local disturbing influences that might tend also to divert the direction of the lower currents, as well as of those near the surface, and lead to erroneous conclusions favouring a bias for any theory or prediction.

As Station 65 was one of Dr. Carpenter's positions for trying for the under-current he asserts to exist, and as he has drawn some inferences in favour of

the positiveness of such an undercurrent there from the temperature and small difference in the densities, although the results did not show it by the current-drag operation, I am under the necessity of referring to it. He says, paragraph 67, page 182, "We commenced our observations on the morning of October 1st at the point of greatest depth (Station 65). The temperature of the surface at 6 A.M. was only 63°, which was at least 8° lower than the average temperature at that hour within the Mediterranean. The bottom-temperature at 198 fathoms was 54°·5, and the specific gravity of the bottom-water was 1028·2. The coincidence both in temperature and specific gravity with the bottom-water at Station 64 was thus very close. The place of the ship having been determined by angles taken with the shore, the rate of the surface-movement was tested as on former occasions, and was found to be 1·277 mile per hour, its direction being E. ½ S. The 'current-drag' was then sunk to 150 fathoms, the greatest depth at which it was thought safe to use it; and the boat from which it was suspended moved E. ¾ N. at the rate of 0·840 mile per hour. This observation indicated a very considerable retardation of the rate of *in*flow, but gave no evidence of an *out*flow. It did not, however, negative the inference deducible from the temperature, and still more from the specific gravity, of the water beneath, that an outflow takes place in that lowest stratum which we could not test by the 'current-drag.'"

The remark I feel it necessary to make is, that although the "current-drag" showed no undercurrent here in 150 fathoms in a depth of 198 fathoms, but, on the contrary, there appeared to be an E. ¾ N. current at that depth of 0·84, or about ¾ mile per hour, yet against this result Dr. Carpenter insists that "it did not, however, negative the inference deducible from the temperature, and still more from the specific gravity, of the water beneath, that an outflow takes place in the lowest stratum." Now, according to the depth, the sounding-drag when down in 150 fathoms was nearly down to the level of the barrier-ridge extending across the Straits there; and moreover, from the Station being where the depth was so great as 198 fathoms, it was on the *west side of the barrier* and considerably below it. The temperature in that depth also, being 54°·5, corresponded closely with a temperature obtained by Mr. Jeffreys at Station 37, a little more to the westward, in 190 fathoms, which was there 53°·7, whilst on the Mediterranean side, in 181 fathoms at Station 63, the temperature was 54°·7; so that there was nothing abnormal in these temperatures at about the same depths, on different sides of the barrier-ridge, viz. that of a degree only in one, and in the other on the Atlantic side of about ¾ of a degree lower temperature than that of the Mediterranean side, where it was at its normal temperature of the deeps on that side; for on the Atlantic side of the barrier the temperature lowers gradually down to its normal depth of about 39½° in the deeper regions, being at Station 35 in 335 fathoms 51°·5 at about 30 or 40 miles to the westward of the one at 65, where the "current-drag" operation for testing the current was taken,

but its result ignored by Dr. Carpenter in favour of the supposed abnormal conditions of temperature and density there. Therefore I fail in being able to agree with Dr. Carpenter's predilection for the density and temperature there, against the "current-drag" indications. If therefore he ignores the "current-drag" test here, he must still more ignore the result at Station 64 by the same means, by boat and basket, and under still more unfavourable circumstances for doing it, when he so sanguinely relies upon it as "a *conclusive proof* that there was at this time a return-current in the mid-channel of this narrowest part of the Strait, from the Mediterranean towards the Atlantic, flowing beneath the constant surface-stream from the Atlantic into the Mediterranean."

For, with the boat and basket as a means for testing the surface and undercurrent, and without a fixed float attached to the bottom as a stationary point of reference for measuring the rates from, instead of by angles upon a chart of small scale, I cannot, from my experience of such operations (and I do not know any one who has had more experience or given more consideration to the subject for ascertaining the proper or best mode of doing it), agree that the result at Station 64 was a satisfactory or sufficient proof of such an undercurrent outflow as Dr. Carpenter contends for. For with a ship's cutter as a float, and with a force of wind of 4 against the surface-current, and producing so much sea (for Dr. Carpenter states it was necessary in consequence to use a larger boat than before, and not leave it to drift without a crew as on former trials), with these two forces, of wind and short sea together, acting in the presumed undercurrent direction, the result certainly cannot be accepted as a "conclusive proof" of such an undercurrent in 250 fathoms of 0·400, or nearly half a mile per hour. The conclusion to my mind was that the bulky boat, from being exposed most probably nearly broadside on to an easterly wind, and therefore following swell, was drifting faster than the inflowing surface-current from the westward, and thus drew the "current-drag" some little distance to the westward, against the $1\frac{3}{4}$-knot surface-current, the "current-drag" being probably in still water in 250 fathoms. I am sure that in this view I shall have many scientific men, landsmen as well as nautical, in full agreement with me, and that for the solution of a question of physical science, and in support of such large views as advocated by Dr. Carpenter, the result was not conclusive.

One more remark touching the "undercurrent flow up-hill" theory of Dr. Carpenter as the result of the 1028·1 specific gravity and of 55°·3 at the bottom at Station 67, in 188 fathoms. Now both these results are apparently to me not abnormal conditions, as compared with the other results in about the same depths, as I have shown in commenting upon the opinions following the density and temperature at Station 65; for I can only conclude from Dr. Carpenter's remarks that the position given of Station 67 in 188 fathoms was evidently on the *west side* of the barrier-ridge, the down-hill or Atlantic side of it, as the line of his section on the Chart of the

Strait shows, and not on the east or *up-hill* side, for an undercurrent coming from the Mediterranean, even if such existed there as an undercurrent. But I must give my reason for not considering this temperature and density at Station 67 as abnormal; for the position being clearly several miles on the Atlantic side of the ridge*, we should expect to find proximate Atlantic conditions on that side. Now, as Dr. Carpenter has no densities between Lisbon and the Straits, except at Station 67, he has no true comparison with the Atlantic conditions of either the surface or the deep water at that part; for the lighter density of the surface-water of the Straits is apparently due to its being a diluted or lowered condition of that of the Atlantic in the same parallel from the influence of the two large Spanish rivers, the Guadiana and Guadalquiver, which fall into the sea so near the entrance to the Straits: not so, however, the density in the depths of 188 fathoms; for there we should expect to find the normal density nearly of the proximate part of the Atlantic, which, if denser than the Atlantic in general, the same would be found in the deeper waters drawn from it by the indraft current into the Mediterranean, the river influence being confined to the surface and being also drawn into the Straits.

Then in regard to the specific gravity of 1028·1 at the bottom, which induced Dr. Carpenter to consider it to be Mediterranean water and not Atlantic, because some slight degree heavier than the mean of the Atlantic found by him between Lisbon and England, I am induced to believe, from Dr. Forchhammer's researches, that such a density is about the normal condition of the Atlantic deeps near the African coast in this parallel; for he shows that the maximum salinity of the Atlantic lies to the south-west of the Straits, about the parallel of 24° and up to about 36° north latitude, and some 300 miles only distant from the African coast, where he says it is 37·908 per 1000, and that this salinity is nowhere exceeded in the Mediterranean, but where its abnormal maximum density between Crete and the Libyan coast is found, which he shows is only exceeded in the whole ocean by the density found in the Red Sea†. This great salinity off the Morocco coast he attributes to the absence of rivers upon it; therefore it seems to me that as we have a source for a salinity as great as that of the mean salinity of the Mediterranean so near, on the outside of the Straits, we have no proof that the water of 1028·1 specific gravity, found by Dr. Carpenter at the depth of 188 fathoms at Station 67, and clearly on the outside of the barrier-ridge, is not Atlantic water, instead of being Mediterranean water, as he concluded, and concluded from it also that there was an "uphill outflow" as a necessary result.

This, however, is an excusable oversight or misunderstanding of the conditions, as, with all due deference, it seems to me to be, in one not familiar with the indications of a few scattered soundings on a chart of the probable line of direction of the crest of a submerged ridge.

* See Chart of the Cruise of H.M.S. 'Porcupine.'

† See Phil. Trans. 1865, p. 220.

Postscript, June 23rd, 1871.

As the undercurrent theory, in its larger view, as first put forth by Capt. Maury, will remain a source of error still for the misguidance of the physical geographer and philosopher, whilst the fallacy or mistaken facts also remain uncontradicted, upon which it was mainly and originally founded by the eminent author of the 'Physical Geography of the Sea,' it is therefore now necessary for me to show, after what I have previously written on the question, that the assertion of an undercurrent of from 1 to 1¾ knot per hour in the Atlantic as counter to a surface-current of much smaller amount on the outside of the Gulf-stream, is based upon a mistaken estimate of the results of the experiments that were supposed to indicate such an undercurrent.

Capt. Maury says, in p. 141, 'Physical Geography of the Sea,' when discussing his undercurrent views in the chapter headed "Undercurrents:"—

"Lieut. J. C. Walsh, of the United States schooner 'Taney,' and Lieut. S. P. Lee, in the United States brig 'Dolphin,' both, while they were carrying on a system of observations in connexion with the wind and current charts, had their attention directed to the subject of submarine currents. They made some interesting experiments on the subject. A block of wood was loaded to sinking, and by means of a fishing-line or a bit of twine let down to the depth of 100 or 500 fathoms; a small float, just sufficient to keep the block from sinking further, was then tied to the line, and the whole let go from the boat.

"To use their own expression, it was wonderful, indeed, to see this *barrega* move off against wind and sea and surface-current at the rate of over one knot an hour as was generally the case, and on one occasion as much as 1¾ knot. The men in the boat could not repress exclamations of surprise; for it really appeared as if some monster of the deep had hold of the weight below, and was walking off with the line. Both officers and men were amazed at the sight."

In paragraph 273 he says, "It may, therefore, without doing violence to the rules of philosophical investigation, be conjectured that the equilibrium of all the seas is preserved, to a greater or less extent, by this system of currents and counter currents at and below the surface. If we except the tides and the partial currents of the sea, such as those that may be created by the wind, we may lay it down as a rule that all the currents of the ocean owe their origin to difference of specific gravity between sea-water at one place and sea-water at another; for whenever there is such a difference, whether it be owing to difference of temperature alone or difference of saltness, &c., it is a difference that disturbs equilibrium, and currents are the consequence. The heavier water gives towards the lighter, and the lighter whence the heavier comes; for two fluids differing in specific gravity, and standing at the same level, cannot balance each other."

From the above reasonings, it is clear that the eminent author, from the supposition that a great undercurrent movement in the Atlantic had been discovered as the result of the observations and experiments of Lieutenants Walsh and Lee, was induced to propound his fascinating but fallacious theory regarding the origin of "all the currents of the ocean" being more due to temperature and density than to tides and winds.

Now it is true in regard to tides, that is, the currents resulting from tide-waves are mainly littoral and local. It is not so, however, as the result of winds, which from my experience are the main sources of ocean-currents, without ignoring that from the rotation of the earth, which are therefore chiefly superficial, but capable of reaching a considerable depth where the water is deep enough, even to 50 and more fathoms, with no greater surface-movement than from three-tenths to five-tenths, or half a knot per hour, as I have on several occasions experienced from a perfect stillness of the sea from the surface down to the greatest depths in perfect calm weather, but which was set in motion in the same direction as the wind to that depth a few hours only after a 4- or 5-knot breeze had set in.

To show that Capt. Maury had mainly founded his theory upon the observations of Lieutenants Walsh and Lee, I must quote from the Report of the former as being the one most important and complete, as was supposed, in proof of the rapid undercurrent believed in by Capt. Maury, and supposed to have been confirmed by other phenomena connected with the fallacious idea of the ploughing of icebergs through fields of ice in Baffin's Bay, by the force of a mighty undercurrent*, instead of the fact of the field of ice flowing past them, by reason of the greater strength of the surface-current over the current in the depths to which the base of the bergs reached, as no doubt must be the fact in that bay or strait from the southerly drift of both into the Atlantic.

"Report of Lieut. Walsh, U.S.N., to Lieut. M. F. Maury, of the Observatory at Washington.

"The next subject to which I would refer is our investigation of the undercurrents of the ocean. I regret we had so few opportunities for the interesting experiments, but enough has been done to seem to warrant the conclusion that these undercurrents are *generally* stronger setting in various different directions than those of the surface. I am well aware there is no mode of testing their exact velocity, but that practised by myself, which I will describe, was certainly all-sufficient to show their real velocity. There may be none so rapid as that mighty ocean-river the Gulf-stream. Unfortunately the weather prevented our making these investigations in that interesting region; but in the various parts of the Atlantic in which we succeeded in these experiments, on only two occasions did we find the undercurrent of less velocity than that running in a different direction above it.

* See Physical Geography of the Sea, pp. 162 & 163.

"The following is the mode practised: the surface-current was first tried by the usual mode (a heavy iron kettle being lowered from a boat to the depth of 80 fathoms), then for the trial of the undercurrent a large chip-log, of the usual quadrantal form, the arc of it measuring full 4 feet, and heavily loaded with lead to make it sink and keep upright, was lowered by a light but strong cod-line to the depth of 126 fathoms (the length of the line); a barrega was attached as a float, a log-line fastened to this barrega, and the rate of motion to this float, as measured by this log-line and the glass, and the direction as shown by a compass, were assumed as the velocity and set of the undercurrent. No allowance was made for the drag of the barrega, *which was always in a different direction from the surface-current.* It was wonderful, indeed, to see this barrega move off against wind and sea and surface-current at the rate of over one knot an hour, as was generally the case, and on one occasion as much as $1\frac{3}{4}$ knot. The men in the boat could not repress exclamations of surprise, for it really appeared as if some monster of the deep had hold of the weight below, and was walking off with it."

It is therefore quite evident that Capt. Maury adopted Lieut. Walsh's identical words and views as the sound solution of the experiments, viz. that a great oceanic undercurrent circulation existed as a counterpoise to the disturbed densities arising from temperature and salinity.

Lieut. Walsh next cites from the log several instances of the experiments, viz. at six positions in the Atlantic, between 24° 43′ North and 65° 2′ West, and 33° 58′ North and 72° West, in which the weighted chip-log was lowered to 126 fathoms in each position, to test the undercurrent at that depth, as erroneously supposed. But in fact it was merely giving a more correct indication of the surface-current than that resulting from the iron kettle in 80 fathoms, with a large boat as its float, under the erroneous impression that the iron kettle would be in still water at that depth, and that it would retain the boat stationary as if anchored to the bottom; this, too, against wind and sea. It is, however, evident that the kettle-and-boat experiment could only show a vitiated result, a diminished surface-current to that actually existing.

The kettle-and-boat experiment were only used once, however, at the last position, in connexion with the large chip-log lowered down to 126 fathoms, which Lieut. Walsh regrets, by saying, "which it would have been better to have always done."

Now it must be quite evident from what I have before shown, from my experiments for testing surface and undercurrents, or from the diagram referred to, p. 537, how the rate of descending surface-currents, and of any undercurrent, can be correctly ascertained, if existing as an appreciable fact, although Lieut. Walsh did not then know "of any means of doing so correctly;" that, therefore, the float to the large 4-feet diameter chip-log, lowered down to 126 fathoms, would naturally appear to run to windward of the heavy boat attached to the iron kettle of less dimensions

than the former, and therefore of less resistance to the drag of the boat by the wind, sea, and surface-current, even if both kettle and chip-log had reached the region of perfectly still water.

But if, as may have been probable, the kettle was still in a portion of the surface-current, and the chip-log in about 50 fathoms lower down was in the still regions, or even a more diminished rate of the surface-current, the float of the latter would more rapidly separate from the boat in the opposite direction to the surface-current, and thus appear to be marvellously dragged by an undercurrent against wind and sea and surface-current—that is, against or opposite to the boat's natural drift. Now, as Lieut. Walsh notices that the weather was too rough for attempting deep soundings, except on the 14th of May, we must infer that there was sufficient wind and sea to cause considerable drift to the boat; but he does not notice the direction of the wind.

Therefore, as there was no fixed object as a point of comparison sufficiently exact in the last experiment, when the kettle was used, much less in the others, when only the chip-log was used, and with a compass bearing from a drifting boat for ascertaining the presumed direction of the undercurrent, even the true direction of the surface-current cannot be depended upon by reversing the direction he has given for the undercurrent (this is, by assuming that the surface-current ran E.N.E. $1\frac{1}{2}$ knot when he gives the undercurrent as setting W.S.W. $1\frac{1}{2}$ knot), since there were so many sources of error vitiating the results. A fixed object for reference can always be obtained in any depth by a 20-lb. lead and sufficient twine, and a light float attached when it has reached the bottom, as I have long since shown and recommended *as a necessity* in all such delicate experiments in mid-ocean or elsewhere. The following are the results at the six positions given by Lieut. Walsh:—

Date.	Lat.	Long.	Depth.	Rate.	Direction.	Temperature, surface.	Fathoms, 100.
	° ′	° ′	fms.	knot.		°	
May 11th ...	24 43	65 2	126	1	W.S.W.	77·3	73·5
May 12th ...	24 55	64 43	126	$1\frac{3}{4}$	S.E.	75	69
May 13th ...	26 42	64 4	126	$1\frac{1}{2}$	W.S.W.	77·5	74·5
May 14th ...	26 46	63 53	126	$1\frac{1}{4}$	N. by E.	77	
May 18th ...	30 06	67 56	126	$\frac{1}{6}$	N.E.	70	65
May 29th ...	33 58	72 00	126	1	W.N.W.	71	67

It is therefore only the rate as given above for the undercurrents that can be relied upon as the rates of a surface-current that really existed in those positions, and which from the high mean temperature for the whole six, of 74°·6, and of 77°·3 at three of the positions, as the surface-temperature for the month of May, and of 73·5 and 74·5 in a depth of 100 fathoms at two of them, would seem to show it to have been a continuation of a portion of the trade-wind or equatorial current, its easterly portion running outside of the West-India Islands, but somewhat checked, and even perhaps at times

diverted by the local winds; for the power of winds to divert even the "mighty Gulf-stream" of $3\frac{1}{2}$ knots is shown in the 'Notes on the Gulf-stream,' by A. D. Bache, Superintendent of the United States Coast Survey, who shows it to be driven sometimes out of its usual course fully thirty miles by N.W. and westerly gales.

In concluding these remarks upon the errors regarding the under-current theory, I feel that it is due to our distinguished transatlantic hydrographers and geographers, that as theirs were the pioneer efforts in such investigations on a large scale, it was natural that they should have been defective, from the little attention given to such researches previously. But it is necessary that these errors should now be well understood, and shown to have arisen from a fallacious estimate of the experiments, that the philosophical naturalist and physicist be no longer misguided by them, and thus attribute so grand and large an influence to undercurrent, as erroneously shown by the experiments of Lieut. Walsh and Lee, and as the assumed necessary result of the small difference of density between one part of the ocean and another; for surface-current circulation and return can and, indeed, must tend largely to restore it, aided by the rain and river supply of fresh water met with in its circulation and return. This is even shown to be so under the equator, from the large African rivers and also from the Amazon and others joining the equatorial current, from the elaborate investigations (the twenty years' researches) of the late Dr. Forchhammer, as summarized in his most interesting and valuable discussion of these analyses in his paper in the Philosophical Transactions for 1865, wherein the analyses and temperatures of the sea-water are given from all parts of the globe, and a most remarkable and able deduction of the surface-currents from them. But the learned Doctor, misguided, no doubt, by the supposed existence of the great undercurrent movement in the Atlantic as propounded by Capt. Maury, and also by the misunderstanding of the facts and the incompleteness of the observations for correctly ascertaining the conditions existing between the Baltic and German Ocean, and so, as a philosophical physicist, thus misled, was induced to ascribe a greater influence to undercurrent circulation than to superficial, as a means of restoring the equilibrium from reduced or increased densities. I have before admitted that where two currents meet, such as the Polar and Gulf-stream, both strong in force and of great difference in density or temperature, and in directions nearly at right angles such as these two, an undercurrent or intermediate current of appreciable amount may exist.

That denser water will intermix with lighter water in its deeper portion, when they meet and when depths are equal and difference of the densities great, as between pure fresh and sea-water, I am aware from my experience at the mouths of large rivers.

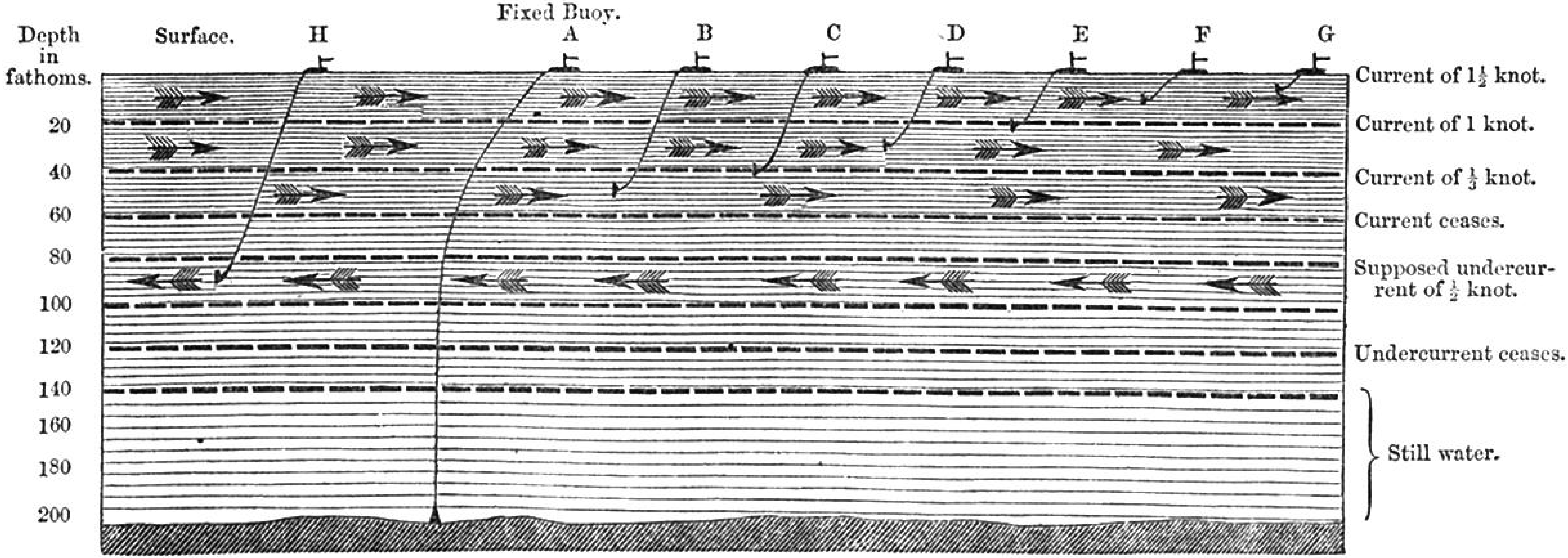
This is a fact experienced every year at the Damietta and Rosetta mouths of the Nile for five or six weeks, during the lowest condition of the Nile, when there is only a surface-outflow there; and it extends five or six miles

within their embouchures, as I first ascertained when carrying out my current tests in that river; brackish water was consequently found in the lower depths, percolation of sea-water through the sandy substrata no doubt then partly accounting for its brackish condition. But eddy surface-currents along the sides of the river, under the influence of the prevailing N.W. winds blowing directly into the mouths, no doubt also assist the intermixture as far as it goes—just as the return-current down the European coast is diluted and intermixed in its general and superficial density from the rains and rivers of the north, and thus tends to restore the lost freshness of the equatorial or trade-wind and Gulf-stream currents, as the tropical rivers and rains tend to restore the loss in the low latitudes: thus condensation from evaporation and redilution by surface-currents are throughout mainly maintaining the equilibrium. Dr. Forchhammer shows that this lighter density or dilution of the encircling superficial waters from the equator commences from the American rivers from the parallel a little north of the Bermudas, and that it exists all along the European coast and again on the African coast from the African rivers; and he has shown that the effect of the La Plata is found 900 miles from its mouth. The fact I have given of the condition at the Nile's mouths at certain seasons is an extreme case, quite in accordance with the great undercurrent theorists' views, and I mention it as a fact of interest to them. But nevertheless I believe, from my own experience, and from the facts to be gathered from Dr. Forchhammer's elaborate researches into the temperatures and saline densities, that as it is not an appreciable and measurable movement as an undercurrent at the Nile or Dardanelles, and only chemically testable by the tongue or hydrometer, so are there no great mechanical and appreciable movements as undercurrents in the ocean as a necessary result of the very slight difference in the densities of one part of the ocean and another. Nevertheless a complete investigation into the phenomena of ocean-currents is a most desirable operation, and can be so easily accomplished on the plan I have found so practicable and easy, and recommended several years ago for adoption by all scientific captains when crossing the great oceans, especially when calms detain them and favour the experiment, without fear of the results being confused or mistaken; for then only should it be carried out where there are great depths and where strong surface-currents exist.

XII. "On the Physical Principles concerned in the passage of Blood-corpuscles through the Walls of the Vessels." By Richard Norris, M.D., Professor of Physiology, Queen's College, Birmingham. Communicated by Dr. Sharpey, Sec. R.S. Received June 12, 1871.

In the year 1846 my much-lamented teacher, Dr. Augustus Waller, published in the Philosophical Magazine two able papers relating to the

Diagram showing the mode of testing the existence, direction, and rate of surface- and undercurrents in the deeps of the sea.



B, C, D, E, F, G, and H, relative positions of the floats to each suspended sinker, after being simultaneously dropped near the fixed float, A, and allowed to drift in their respective currents for five minutes.

Thus, if a surface-current of $1\frac{1}{2}$ knot was found to be passing the fixed float A, and the floats B, C, D, E, F, G had reached those positions at the end of five minutes' interval, their suspended sinkers or drags were in diminishing rates of the surface-current; but the float H, from being dragged in the opposite direction, had evidently reached an undercurrent with its suspended sinker or current-drag.