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XXIX. *On Ocean-currents.*—PART III. *On the Physical Cause of Ocean-currents. By* JAMES CROLL, *of the Geological Survey of Scotland.*

[Continued from vol. xxxix. p. 194.]

THERE is no point connected with ocean-currents on which more diversity of opinion has existed than in regard to their origin. At present, however, there may be said to be only two theories held on the subject, viz. that which attributes the currents to the influence of the trade and other winds, and that which attributes them to differences in specific gravity between the waters of intertropical and polar regions. The latter theory appears at present to be the more prevalent of the two, although, perhaps, not so among scientific men. It is difficult to conceive how a theory so manifestly erroneous should have gained such general acceptance. Its popularity is no doubt chiefly owing to the very great prominence given to it by Lieut. Maury in his interesting and popular work 'The Physical Geography of the Sea.' Another cause which must have favoured the reception of this theory is the ease with which it is perceived how, according to it, circulation of the waters of the ocean is supposed to follow. One has no difficulty, for example, in perceiving that if the intertropical waters of the ocean are expanded by heat, and the waters around the poles contracted by cold, the surface of the ocean will stand at a higher level at the equator than at the poles. Equilibrium being thus disturbed, the water at the equator will tend to flow towards the poles as a surface-current, and the water at the poles towards the equator as an undercurrent. This, at first sight, looks well, especially to those who take but a superficial view of the matter.

We shall examine this theory at some length, for two reasons: 1, because it lies at the root of a great deal of the confusion and misconception which have prevailed in regard to the whole subject of ocean-currents; 2, because, if the theory is correct, it militates strongly against the physical theory of secular changes of climate advanced in the preceding part of this paper. We have already seen that when the excentricity of the earth's orbit reaches a high value, a combination of physical circumstances tends to lower the temperature of the hemisphere which has its winter solstice in aphelion, and to raise the temperature of the opposite hemisphere, whose winter solstice will, of course, be in perihelion. The direct result of this state of things, as was shown, is to strengthen the force of the trade-winds on the cold hemisphere, and to weaken their strength on the warm hemisphere; and this, in turn, we also saw tends to impel the warm water of the intertropical region over on the warm hemisphere, and to prevent it, in a very large degree, from passing into the cold hemisphere. This deflection of the ocean-currents tends to an enormous extent to increase the difference of temperature previously existing between the two hemispheres. In other words, the warm and equable condition of the one hemisphere, and the cold and glacial condition of the other, are, to a great extent, due to this deflection of ocean-currents. But if the theory be correct which attributes the motion of ocean-currents to a difference in density between the sea in intertropical and polar regions, then it follows that these currents (other things being equal) ought to be stronger on the cold hemisphere than on the warm, because there is a greater difference of temperature and, consequently, a greater difference of density between the polar seas of the cold hemisphere and the equatorial seas, than between the polar seas of the warm hemisphere and the equatorial seas. And this being the case, notwithstanding the influence of the trade-winds of the cold hemisphere blowing over upon the warm, the currents will, in all probability, be stronger on the cold hemisphere than on the warm. In other words, the influence of the powerful trade-winds of the cold hemisphere to impel the warm water of the equator over upon the warm hemisphere will probably be more than counterbalanced by the tendency of the warm and buoyant waters of the equator to flow towards the dense and cold waters around the pole of the cold hemisphere. But if ocean-currents are due not to difference in specific gravity, but to the influence of the trade-winds, then it is evident that the waters at the equator will be impelled, not into the cold hemisphere, but into the warm.

As Lieut. Maury appears to be the acknowledged exponent of the theory which attributes ocean-currents to the difference of specific gravity between the waters at the equator and the poles, I shall now proceed to consider at some length his views on the subject, the more especially as we find in his work on the physical geography of the sea almost every argument that can be advanced in favour of the theory which he advocates.

Although considerable diversity of opinion has prevailed in regard to the cause of ocean-currents, yet it is remarkable how little is to be found of a purely scientific character bearing directly on the dynamics of the subject.

Another reason which has induced me to select Maury's work is, that it not only contains a much fuller discussion on the cause of the motion of ocean-currents than is to be found anywhere else, but also that it has probably passed through a greater number of editions than any other book of a scientific character in the English language in the same length of time.

Lieut. Maury *on the Cause of the Motion of Ocean-currents.*

Although Lieut. Maury has expounded his views on the cause of ocean-currents at great length in the various editions of his work, yet it is somewhat difficult to discover what they really are. This arises chiefly from the generally confused and sometimes contradictory nature of his hydrodynamical conceptions. After a repeated perusal of several editions of his book, the following, I trust, will be found to be a pretty accurate representation of his theory :—

Ocean-currents, according to Maury, due to difference of specific gravity.—Although Maury alludes to a number of causes which, he thinks, tend to produce currents, yet he deems their influence so small that, practically, all currents may be referred to difference of specific gravity.

"If we except," he says, "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 the differences of specific gravity between sea-water at one place and sea-water at another; for wherever there is such a difference, whether it be owing to difference of temperature or to difference of saltness, &c., it is a difference that disturbs equilibrium, and currents are the consequence" (§ 467)*. To the same effect see §§ 896, 37, 512, 520, and 537.

Notwithstanding the fact that Maury is continually referring to difference of specific gravity as the great cause of currents, it is difficult to understand in what way he conceives this difference to act as a cause.

Difference of specific gravity between the waters of the ocean

* The edition from which I quote, unless stated to the contrary, is the one published by Messrs. T. Nelson and Sons, 1870.

at one place and another can give rise to currents only through the influence of the earth's gravity. All currents resulting from difference of specific gravity can be ultimately resolved into the general principle that the molecules that are specifically heavier *descend* and displace those that are specifically lighter. If, for example, the ocean at the equator be expanded by heat or by any other cause, it will be forced by the denser waters in temperate and polar regions to rise so that its surface shall stand at a higher level than the surface of the ocean in these regions. The surface of the ocean will become an inclined plane, sloping from the equator to the poles. Hydrostatically, the ocean, considered as a mass, will then be in a state of equilibrium; but the individual molecules will not be in equilibrium. The molecules at the surface in this case may be regarded as lying on an inclined plane sloping from the equator down to the poles, and as these molecules are at liberty to move they will not remain at rest, but will descend the incline towards the poles. When the waters at the equator are expanded, or the waters at the poles contracted, gravitation makes, as it were, a twofold effort to restore equilibrium. It in the first place sinks the waters at the poles, and raises the waters at the equator, in order that the two masses may balance each other; but this very effort of gravitation to restore equilibrium to the mass destroys the equilibrium of the molecules by disturbing the level of the ocean. It then, in the second place, endeavours to restore equilibrium to the molecules by pulling the lighter surface-water at the equator down the incline towards the poles. This tends not only to restore the level of the ocean, but to bring the lighter water to occupy the surface and the denser water the bottom of the ocean; and when this is done, complete equilibrium is restored, both to the mass of the ocean and to its individual molecules, and all further motion ceases. But if heat be constantly applied to the waters of the equatorial regions, and cold to those of the polar regions, and a permanent disturbance of equilibrium maintained, then the continual effort of gravitation to restore equilibrium will give rise to a constant current. In this case, the heat and the cold (the agents which disturb the equilibrium of the ocean) may be regarded as causes of the current, inasmuch as without them the current would not exist; but the real efficient cause, that which impels the water forward, is the force of gravity. But the force of gravity, as has already been noticed, cannot produce motion (perform work) unless the thing acted upon *descend.* Descent is implied in the very conception of a current produced by difference of specific gravity.

But Maury speaks as if difference of specific gravity could give rise to a current without any descent.

"It is not necessary," he says, "to associate with oceanic currents the idea that they must of necessity, as on land, run from a higher to a lower level. So far from this being the case, some currents of the sea actually run up hill, while others run on a level. The Gulf-stream is of the first class" (§ 403). "The top of the Gulf-stream runs on a level with the ocean; therefore we know it is not a descending current" (§ 18). And in § 9 he says that between the Straits of Florida and Cape Hatteras the waters of the Gulf-stream "are actually forced up an inclined plane, whose submarine ascent is not less than 10 inches to the mile." To the same effect see §§ 25, 59.

It is perfectly true that "it is not necessary to associate with ocean-currents the idea that they must of necessity, as on land, run from a higher to a lower level." But the reason of this is that ocean-currents do not, like the currents on land, owe their motion to the force of gravitation. If ocean-currents result from difference of specific gravity between the waters in tropical and polar regions, as Maury maintains, then it is necessary to assume that they are descending currents. Whatever be the cause which may give rise to a difference of specific gravity, the motion which results from this difference is due wholly to the force of gravity; but gravity can produce no motion unless the water *descend.*

This fact must be particularly borne in mind while we are considering Maury's theory that currents are the result of difference of specific gravity.

Ocean-currents, then, according to Maury, owe their existence to the difference of specific gravity between the waters of intertropical and polar regions. This difference of specific gravity he attributes to two causes—(1) to difference as to *temperature,* (2) to difference as to saltness. There are one or two causes of a minor nature affecting the specific gravity of the sea, to which Maury alludes; but these two determine the general result. Let us begin with the consideration of the first of these two causes, viz.:—

Difference of specific gravity resulting from difference of temperature.—Maury explains his views on this point by means of an illustration. "Let us now suppose," he says, "that all the water within the tropics, to the depth of one hundred fathoms, suddenly becomes oil. The aqueous equilibrium of the planet would thereby be disturbed, and a general system of currents and counter currents would be immediately commenced—the oil, in an unbroken sheet on the surface, running toward the poles, and the water, in an undercurrent, toward the equator. The oil is supposed, as it reaches the polar basin, to be reconverted into water, and the water to become oil as it crosses Cancer and Capricorn, rising to the surface in intertropical regions, and returning as before" (§20). "Now," he says (§22), "do not the cold waters of the north, and the warm waters of the Gulf, made specifically lighter by tropical heat, and which we see actually preserving such a system of countercurrents, hold, at least in some degree, the relation of the supposed water and oil?"

In § 24 he calculates that at the Narrows of Bemini the difference in weight between the volume of the Gulf-water that crosses a section of the stream in one second, and an equal volume of water at the ocean temperature of the latitude, supposing the two volumes to be equally salt, is fifteen millions of pounds. Consequently the force per second operating to propel the waters of the Gulf towards the pole would in this case, he concludes, be the "equilibrating tendency due to fifteen millions of pounds of water in the latitude of Bemini." In §§ 511 and 512 he states that the effect of expanding the waters at the torrid zone by heat, and of contracting the waters at the frigid zone by cold, is to produce a set of surface-currents of warm and light water from the equator towards the poles, and another set of undercurrents of cooler and heavy water from the poles towards the equator. See also to the same effect §§ 513, 514, 896.

There can be no doubt that Maury concludes that the waters in intertropical regions are expanded by heat, and those in polar regions are contracted by cold, and that this tends to produce a surface-current from the equator to the poles, and an undercurrent from the poles to the equator.

We shall now consider his second great cause of ocean-currents, viz.:—

Difference of specific gravity resulting from difference in degree of saltness.—Maury maintains, and that correctly, that saltness increases the density of water—that, other things being equal, the saltest water is the densest. He suggests "that one of the purposes which, in the grand design, it was probably intended to accomplish by having the sea salt and not fresh, was to impart to its waters the forces and powers necessary to make their circulation complete" (§ 495).

Now it is perfectly obvious that if difference in saltness is to cooperate with difference in temperature in the production of ocean-currents, the saltest waters, and consequently the densest, must be in the polar regions, and the waters least salt, and consequently lightest, must be in equatorial and intertropical regions. Were the saltest waters at the equator, and the freshest at the poles, it would tend to neutralize the effect due to heat, and, instead of producing a current, would simply tend to prevent the existence of the currents which otherwise would result from difference of temperature.

A very considerable portion of Maury's book, however, is devoted to proving that the waters of equatorial and intertropical regions are salter and heavier than those of the polar regions; and yet, notwithstanding this, he endeavours to show that this difference in respect to saltness between the waters of the equatorial and the polar regions is one of the chief causes, if not the chief cause, of ocean-currents. In fact, it is for this special end that so much labour is bestowed in proving that the saltest water is in the equatorial and intertropical regions, and the freshest in the polar.

"In the present state of our knowledge," he says, "concerning this wonderful phenomenon (for the Gulf-stream is one of the most marvellous things in the ocean) we can do little more than conjecture. But we have two causes in operation which we may safely assume are among those concerned in producing the Gulf-stream. One of these is the increased saltness of its water after the trade-winds have been supplied with vapour from it, be it much or little; and the other is the diminished quantum of salt which the Baltic and the Northern Seas contain" (§ 37). "Now here we have, on one side, the Caribbean Sea and Gulf of Mexico, with their waters of brine; on the other, the great Polar basin, the Baltic, and the North Sea, the two latter with waters that are but little more than brackish. In one set of these sea-basins the water is heavy, in the other it is light. Between them the ocean intervenes; but water is bound to seek and to maintain its level; and here, therefore, we unmask one of the agents concerned in causing the Gulf-stream" (§ 38). To the same effect see §§ 52, 522, 523, 524, 525, 526, 528, 530, 554, 556.

Lieut. Maury's *two causes neutralize each other.* Here we have two theories put forth regarding the cause of ocean-currents, the one in direct opposition to the other. According to the one theory, ocean-currents exist because the waters of equatorial regions, in consequence of their higher temperature, are *less dense* than the waters of the polar regions; but according to the other theory, ocean-currents exist because the waters of equatorial regions, in consequence of their greater saltness, are *more dense* than the waters of the polar regions. If the one cause be assigned as a reason why ocean-currents exist, then the other can be equally assigned as a reason why they do not exist. According to both theories it is the difference of density between the equatorial and polar waters that gives rise to currents; but according to the one theory the equatorial waters are *lighter* than the polar, whilst according to the other theory they are *heavier* than the polar. Either the one theory or the other may be true, or neither; but it is logically impossible that both of them can, for the simple reason that the waters of the equator cannot at the same time be both lighter and heavier than the water at the poles. They may be either the one or the other, but they cannot be both. Let it be observed that it is not two currents, the one contrary to the other, with which we have at present to do; it is not temperature producing currents in one direction, and saltness producing currents in the contrary direction. We have two theories regarding the origin of currents, the one diametrically opposed to the other. The tendency of the one cause assigned is to prevent the action of the other cause. If temperature is allowed to act, it will make the intertropical waters lighter than the polar, and then, according to theory, a current will result. But if we bring saltness into play (the other cause) it will do the reverse: it will increase the density of the intertropical waters and diminish the density of the polar; and so far as it acts it will diminish the currents produced by temperature, because it will diminish the difference of specific gravity between the intertropical and polar regions which had been previously caused by temperature. And when the effects of saltness are as powerful as those of temperature, the difference of specific gravity produced by temperature will be completely effaced, or, in other words, the waters of the equatorial and polar seas will be of the same density, and consequently no current will exist. And so long as the two causes continue in action, no current can arise, unless the energy of the one cause should happen to exceed that of the other; and even then a current will only exist to the extent by which the strength of the one exceeds that of the other.

The contrary nature of the two theories will be better seen by considering the way in which he supposes difference in saltness is produced and acts as a cause.

If there is a constant current resulting from the difference in saltness between the equatorial and polar waters, then there must be a cause which maintains this difference in saltness. The current is simply the effort to restore the equilibrium lost by this difference; and the current would very soon do this, and then all motion would cease, were there not a constantly operating cause maintaining this disturbance. What, then, according to Maury, is the cause of this disturbance, or, in other words, what is it that keeps the equatorial waters salter than the polar?

The agencies in operation which keep the waters in equatorial regions salter than the polar are stated by him to be heat, radiation, evaporation, precipitation, and secretion of solid matter in the form of shells, &c. The two most important, however, are evaporation and precipitation.

The trade-winds enter the equatorial regions as relatively dry winds thirsting for vapour; consequently they absorb far more moisture than they give out; and the result is that, in intertropical regions, evaporation is much in excess of precipitation; and as fresh water only is taken up, the salt being left behind, the process, of course, tends to increase the saltness of the intertropical seas. Again, in polar and extratropical regions the reverse is the case; precipitation is in excess of evaporation. This tends in turn to diminish the saltness of the waters of those regions. See on these points §§ 31, 33, 34, 37, 179, 517, 526, and 552.

In the system of circulation produced by difference of temperature, as we have already seen, the surface-currents flow from the equator to the poles, and the under or return currents from the poles to the equator; but in the system produced by difference of saltness, the surface-currents flow from the poles to the equator, and the return undercurrents from the equator to the poles. That the surface-currents produced by difference of saltness flow from the poles to the equator, Maury thinks is evident for the two following reasons:—

(1) As evaporation is in excess of precipitation in intertropical regions, more water is taken off the surface of the ocean in those regions, than falls upon it in the form of rain. This excess of water falls in the form of rain on temperate and polar regions, where, consequently, precipitation is in excess of evaporation. The lifting of the water off the equatorial regions and its deposit on the polar tend to lower the level of the ocean in equatorial regions and to raise the level in polar; consequently, in order to restore the level of the ocean, the surface-water at the polar regions flows towards the equatorial regions.

(2) As the water taken up at the equator is fresh, and the salt is left behind, the ocean, in intertropical regions, is thus made salter and consequently denser. This dense water, therefore, sinks and passes away as an undercurrent. This water, evaporated from intertropical regions, falls as fresh and lighter water in temperate and polar regions; and therefore not only is the level of the ocean raised, but the waters are made lighter. Hence, in order to restore equilibrium, the waters in temperate and polar regions will flow as a surface-current towards the equator. Undercurrents will flow from the equator to the poles, and surface or upper currents from the poles to the equator. Difference in temperature and difference in saltness, therefore, in every respect tend to produce opposite effects.

That the above is a fair representation of the way in which Maury supposes difference in saltness to act as a cause in the production of ocean-currents will appear from the following quotations:—

"In those regions, as in the trade-wind region, where evaporation is in excess of precipitation, the general level of this supposed sea would be altered, and immediately as much water as is carried off by evaporation would commence to flow in from north and south toward the trade-wind or evaporation region, to restore the level" (§ 509). "On the other hand, the winds have taken this vapour, borne it off to the extratropical regions, and precipitated it, we will suppose, where precipitation is in excess of evaporation. Here is another alteration of sea-level, by elevation instead of by depression; and hence we have the motive power for a *surface-current from each pole towards the equator*, the object of which is only to supply the demand for evaporation in the trade-wind regions" (§ 510).

The above result would follow, supposing the ocean to be fresh. He then proceeds to consider an additional result that follows in consequence of the saltness of the ocean.

"Let evaporation now commence in the trade-wind region, as it was supposed to do in the case of the fresh-water seas, and as it actually goes on in nature—and what takes place? Why, a lowering of the sea-level as before. But as the vapour of salt water is fresh, or nearly so, fresh water only is taken up from the ocean; that which remains behind is therefore more salt. Thus, while the level is lowered in the salt sea, the equilibrium is destroyed because of the saltness of the water; for the water that remains after evaporation takes place is, on account of the solid matter held in solution, specifically heavier than it was before any portion of it was converted into vapour" (§ 517).

"The vapour is taken from the surface-water; the surface-water thereby becomes more salt, and, under certain conditions, heavier. When it becomes heavier, it sinks; and hence we have, due to the salts of the sea, a vertical circulation, namely, a descent of heavier—because salter and cooler—water from the surface, and an ascent of water that is lighter—because it is not so salt—from the depths below" (§ 518).

In section 519 he goes on to show that this vapour removed from the intertropical region is precipitated in the polar regions, where precipitation is in excess of evaporation. "In the precipitating regions, therefore, the level is destroyed, as before explained, by elevation, and in the evaporating regions by depression; which, as already stated, gives rise to a system of *surface-currents*, moved by gravity alone, from the *poles towards the equator*" (§ 520).

"This fresh water being emptied into the Polar Sea and agitated by the winds, becomes mixed with the salt; but as the agitation of the sea by the winds is supposed to extend to no great depth, it is only the upper layer of salt water, and that to a moderate depth, which becomes mixed with the fresh. The specific gravity of this upper layer, therefore, is diminished just as much as the specific gravity of the sea-water in the evaporating regions was increased. *And thus we have a surface-current of saltish water from the poles towards the equator, and an undercurrent of water salter and heavier from the equator to the poles*" (§ 522).

"This property of saltness imparts to the waters of the ocean another peculiarity, by which the sea is still better adapted for the regulation of climates, and it is this: by evaporating fresh water from the salt in the tropics, the surface-water becomes heavier than the average of sea-water. This heavy water is also warm water; it sinks, and being a good retainer, but a bad conductor of heat, this water is employed in transporting through *undercurrents* heat for the mitigation of climates in far distant regions" (§ 526).

"For instance, let us suppose the waters in a certain part of the torrid zone to be 90°, but, by reason of the fresh water which has been taken from them in a state of vapour, and consequently, by reason of the proportionate increase of salts, these waters are heavier than waters that may be cooler, but not so salt. This being the case, the tendency would be for this warm but salt and heavy water to flow off as an *undercurrent towards the polar or some other regions of lighter water*" (§ 554).

That Maury supposes the warm water at the equator to flow to the polar regions as an undercurrent is further evident from the fact that he maintains that the climate of the arctic regions is mitigated by a warm undercurrent, which comes from the equatorial regions, and passes up through Davis Straits. See §§ 534–544.

The question now suggests itself: to which of these two antagonistic causes does Maury really suppose ocean-currents must be referred? Whether does he suppose, difference in temperature or difference in saltness, to be the real cause? I have been unable to find any thing from which we can reasonably conclude that he prefers the one cause to the other. It would seem that he regards both as real causes, and that he has failed to perceive that the one is destructive of the other. But it is difficult to conceive how he could believe that the sea in equatorial regions, by virtue of its higher temperature, *is* lighter than the sea in polar regions, while at the same time it *is not* lighter but heavier, in consequence of its greater saltness—how he could believe that the warm water at the equator flows to the poles as an upper current, and the cold water at the poles to the equator as an *under*current, while at the same time the warm water at the equator does not flow to the poles as a surface-current, nor the cold water at the poles to the equator as an undercurrent, but the reverse. But, unless these absolute impossibilities be possible, how then can an ocean-current be the result of both causes?

The only explanation of the matter appears to be that Maury has failed to perceive the contradictory nature of his two theories. This fact is particularly seen when he comes to apply his two theories to the case of the Gulf-stream. He maintains, as has already been stated, that the waters of the Gulf-stream are salter than the waters of the sea through which they flow (see §§ 3, 28, 29, 30, 34, and several other places). And he states that one of the chief causes of the Gulf-stream is this, that "we have on one side the Caribbean Sea and Gulf of Mexico, with their waters of brine; on the other the great Polar Basin, the Baltic, and the North Sea, the two latter with waters that are but little more than brackish. In one set of these sea-basins the water is heavy, in the other it is light. Between them the ocean intervenes; but water is bound to seek and to maintain its level; and here, therefore, we unmask one of the agents concerned in causing the Gulf-stream" (§ 38). There can be no doubt whatever that it is the *density* of the waters of the Gulf-stream at its fountain-head, the Gulf of Mexico, resulting from its superior saltness, and the deficiency of density of the waters in polar regions and the North Sea &c., that is here considered to be unmasked as one of the agents. If this be a cause of the motion of the Gulf-stream, how then can the difference of temperature between the waters of intertropical and polar regions assist as a cause? This difference of temperature will simply tend to undo all that has been done by difference of saltness; for it will tend to make the waters of the Gulf of Mexico lighter, and the waters of the polar regions heavier. But Maury maintains, as we have seen, that this difference of temperature is also a cause, which shows that he does not perceive the contradiction.

This is still further apparent. Maury maintains, as stated, that "the waters of the Gulf-stream are salter than the waters of the sea through which they flow," and that this excess in saltness, by making the water heavier, is a cause of the motion of the stream. But he maintains that, notwithstanding the effect which greater saltness has in increasing the density of the waters of the Gulf-stream, yet, owing to their higher temperature, they are actually lighter than the water through which they flow; and as a proof that this is the case, he adduces the fact that the surface of the Gulf-stream is roof-shaped (§§ 39–41), which it could not be were its waters not actually lighter than the waters through which the streams flow. So it turns out, in contradiction to what he had already stated, that it is the lesser density of the waters of the Gulf-stream that is the real cause of their motion. The greater saltness of the waters, to which he attributes so much, can in no way be regarded as a cause of motion. Its effect, so far as it goes, is to stop the motion of the stream rather than to assist it.

But, again, although Maury maintains that difference of saltness and difference of temperature are both causes of ocean-currents, yet he appears actually to admit that temperature and saltness neutralize each other so as to prevent change in the specific gravity of the ocean, as will be seen from the following quotation :—

"It is the trade-winds, then, which prevent the thermal and specific-gravity curves from conforming with each other in intertropical seas. The water they suck up is fresh water; and the salt it contained, being left behind, is just sufficient to counterbalance, by its weight, the effect of thermal dilatation upon the specific gravity of sea-water between the parallels of 34° north and south. As we go from 34° to the equator, the water grows warmer and expands. It would become lighter; but the trade-winds, by taking up vapour without salt, make the water salter, and therefore heavier. The conclusion is, the proportion of salt in sea-water, its expansibility between 62° and 82°, and the thirst of the trade-winds for vapour are, where they blow, so balanced as to produce *perfect compensation*; and a more beautiful compensation cannot, it appears to me, be found in the mechanism of the universe than that which we have here stumbled upon. It is a triple adjustment: the power of the sun to expand, the power of the winds to evaporate, and the quantity of salts in the sea—these are so proportioned and adjusted that when both the wind and the sun have each played with its forces upon the intertropical waters of the ocean, *the residuum of heat and of salt should be just such as to balance each other in their effects*; *and so the aqueous equilibrium of the torrid zone is preserved*" (§ 436, eleventh edition).

"Between 35° or 40° and the equator evaporation is in excess of precipitation; and though, as we approach the equator on either side from these parallels, the solar ray warms and expands the surface-water of the sea, the winds, by the vapour they carry off, and the salt they leave behind, *prevent it from making that water lighter*" (§ 437, eleventh edition).

"Philosophers have admired the relations between the size of the earth, the force of gravity, and the strength of fibre in the flower-stalks of plants; but how much more exquisite is the system of counterpoises and adjustments here presented between the sea and its salts, the winds and the heat of the sun!" (§ 438, eleventh edition).

How can this be reconciled with all that precedes regarding ocean-currents being the result of difference of specific gravity caused by a difference of temperature and difference of saltness? Here is a distinct recognition of the fact that difference in saltness, instead of producing currents, tends rather to prevent the existence of currents, by counteracting the effects of difference in temperature. And so effectually does it do this, that for 40°, or nearly 3000 miles, on each side of the equator there is absolutely no difference in the specific gravity of the ocean, and consequently nothing, either as regards difference of temperature or difference of saltness, that can possibly give rise to a current.

But it is evident that, if between the equator and latitude 40° the two effects completely neutralize each other, it is not at all likely that between latitude 40° and the poles they will not to a very large extent do the same thing. And if so, how can ocean-currents be due either to difference in temperature or to difference in saltness, far less to both. If there be any difference of specific gravity of the ocean between latitude 40° and the poles, it must be only to the extent by which the one cause has failed to neutralize the other. If, for example, the waters in latitude 40°, by virtue of higher temperature, are less dense than the waters in the polar regions, they can be so only to the extent that difference in saltness has failed to neutralize the effect of difference in temperature. And if currents result, they can do so only to the extent that difference in saltness has thus fallen short of being able to produce complete compensation. Maury, after stating his views on compensation, seems to become aware of this; but, strangely, he does not appear to perceive, or, at least, he does not make any allusion to the fact, that all this is fatal to the theories he had been advancing about ocean-currents being the combined result of differences of temperature and difference of saltness. For, in opposition to all that he had previously advanced regarding the difficulty of finding a cause sufficiently powerful to account for such currents as the Gulf-stream, and the great importance that difference in saltness had in the production of currents, he now begins to maintain that so great is the influence of difference in temperature in causing currents that difference in saltness, and a number of other compensating causes are actually necessary to prevent the ocean-currents from becoming too powerful.

"If all the intertropical heat of the sun," he says, "were to pass into the seas upon which it falls, simply raising the temperature of their waters, it would create a thermo-dynamical force in the ocean capable of transporting water scalding hot from the torrid zone, and spreading it while still in the tepid state around the poles . . . . Now, suppose there were no trade-winds to evaporate and to counteract the dynamical force of the sun, this hot and light water, by becoming hotter and lighter, would flow off in currents with almost mill-tail velocity towards the poles, covering the intervening sea with a mantle of warmth as a garment. The cool and heavy water of the polar basin, coming out as undercurrents, would flow equatorially with equal velocity."

"Thus two antagonistic forces are unmasked, and, being unmasked, we discover in them a most exquisite adjustment—a compensation—by which the dynamical forces that reside in the sunbeam and the trade-wind are made to counterbalance each other, by which the climates of intertropical seas are regulated, and by which the set, force, and volume of oceanic currents are measured" (§§ 437 and 438, eleventh edition).

The force resulting from difference of specific gravity not sufficient to produce motion.—I shall now consider whether the forces to which Maury appeals have the potency that he attributes to them. Is the force derived from the difference of specific gravity between the waters of the ocean in intertropical and polar regions sufficient to account for the motion of ocean-currents?

The utter inadequacy of this cause has been so clearly shown by Sir John Herschel, that one might expect that little else would be required than simply to quote his words on the subject, which are as follows:—

"First, then, if there were no atmosphere, there would be no Gulf-stream, or any other considerable ocean-current (as distinguished from a mere surface-drift) whatever. By the action of the sun's rays, the *surface* of the ocean becomes *most* heated, and the heated water will, therefore, neither directly tend to *ascend* (which it could not do without leaving the sea) nor to *descend*, which it cannot do, being rendered buoyant, nor to move laterally, no lateral impulse being given, and which it could only do by reason of a general declivity of surface, the dilated portion occupying a higher level. Let us see what this declivity would amount to. The equatorial surface-water has a temperature of 84°. At 7200 feet deep the temperature is 39°, the level of which temperature rises to the surface in latitude 56°. Taking the dilatability of sea-water the same as that of fresh, a uniformly progressive increase of temperature, from 39° to 84° Fahr., would dilate a column of 7200 feet by 10 feet, to which height, therefore, above the spheroid of equilibrium (or above the sea-level in lat. 56°), the equatorial surface is actually raised by dilatation. An arc of 56° on the earth's surface measures 3360 geographical miles; so that we have a slope of 1-28th of an inch per geographical mile, or 1-32nd of an inch per statute mile for the water so raised to run down. As the accelerating force corresponding to such a slope (of 1-10th of a second, $0''\cdot 1$) is less than one two-millionth part of gravity, we may dismiss this as a cause capable of creating only a very trifling surface-drift, and not worth considering, even were it in the proper direction to form, by concentration, a current from east to west, *which it would not be, but the very reverse.*"—*Physical Geography*, article 57.)

It is singular how any one, even though he regarded this conclusion as but a rough approximation to the truth, could entertain the idea that ocean-currents can be the result of difference in specific gravity. There are, however, one or two reasons which may be assigned why the above has not been generally received as conclusive. These calculations refer to the difference of gravity resulting from difference of temperature; but this is only one of the causes to which Maury appeals, and even not the one to which he most frequently alludes. Maury insists so strongly on the effects of difference of saltness, that many would no doubt suppose that, although Herschel may have shown that difference in specific gravity arising from difference of temperature could not account for the motion of ocean-currents, yet nevertheless this, combined with the effects resulting from difference in saltness, might account for their motion. This, of course, would not be the case with those who perceived the contradictory nature of Maury's two causes; but most people probably read the 'Physical Geography of the Sea' without being aware that the one cause is destructive of the other. Another reason is, a few very plausible-looking objections have been strongly urged by Maury and others against the theory that ocean-currents can be caused by the impulses of the trade-winds, which have not been duly considered; and probably these objections appear to many as formidable against this theory as Herschel's arguments appear against Maury's theories.

There is one slight objection to Herschel's result: he takes 39° as the temperature of maximum density. This, however, as we shall see, does not materially affect his conclusions.

Observations on the temperature of the maximum density of sea-water have been made by Erman, Despretz, Rossetti, Neumann, Marcet, Hubbard, Horner, and others. No two of them have arrived at exactly the same conclusion. This probably results from the fact that the temperature of maximum density depends upon the amount of salt held in solution. No two seas, unless they are equal as to saltness, have the same temperature of maximum density. The following Table of Despretz will show how rapidly the temperature of both the freezing-point and of maximum density is lowered by additional amounts of salt.

Amount of salt.	Temperature of freezing-point.	Temperature of Maximum density.
0·000123	$-1^{\circ}{\cdot}21$ C.	$+1^{\circ}{\cdot}19$ C.
0·0246	$-2{\cdot}24$	$-1{\cdot}69$
0·0371	$-2{\cdot}77$	$-4{\cdot}75$
0·0741	$-5{\cdot}28$	$-16{\cdot}00$

He found the temperature of maximum density of sea-water, whose density at 20°C. was 1·0273, to be $-3^{\circ}{\cdot}67$ C. (25°·4 F.), and the temperature of freezing-point $-2^{\circ}{\cdot}55$ (27°·4F.)*. Somewhere between 25° and 26° F. may therefore be regarded as the temperature of maximum density of sea-water of average saltness. We have no reason to believe that the ocean, from the surface to the bottom, even at the poles, is at 27°·4 F., the freezing-point. An error to the extent of a degree or two, however, will not materially affect the conclusion at which we may arrive. Let us therefore assume the temperature of the ocean at the poles to be 32°, and the surface-temperature at the equator to be 80°. Maury states that at the depth of 7200 feet at the equator the temperature is about 36° (§ 440, eleventh edition). Although this agrees pretty nearly with the results arrived at by several observers who have attempted to determine the temperature of the ocean at great depths in equatorial regions, still 36°, the temperature assigned at 7200 feet below the surface, is probably too high; for these observations were made with thermometers unprotected from the pressure of the water on their bulbs, which at so great a depth would equal more than 200 atmospheres; 32°, at a depth of 7200 feet, may probably be nearer the truth than 36°. But we shall assume that we must descend to a depth of, say, 10,000 feet, before the temperature of 32°, that of the poles, is reached. Let us also assume that the temperature decreases at a uniform rate from the surface downwards to that depth. Calculating, then, from Muncke's Table of the Expansion of Sea-water, we have about 18 feet as the height at which the water at the equator stands above the level of the ocean at the poles. The distance from the equator to the poles is about 6200 miles. The force impelling the water down this slope of 18 feet in 6200 miles would therefore be equal to about $\frac{1}{1,820,000}$ that of gravity. For example, the force impelling a cubic foot (64 lbs.) of water at the surface of the ocean would scarcely be equal to the weight of one-fourth of a grain.

But in reality it would not nearly equal this; for we have been assuming in our calculations that the temperature of the

* Philosophical Magazine, vol. xii. p. 1 (1838).

ocean at the equator decreases at a uniform rate from the surface downwards, which is far from being the case. The rate of decrease is most rapid at the surface, and decreases as we descend. The principal part of the decrease of temperature takes place within no very great depth from the surface; consequently the greater part of the excess of temperature at the equator over that at the poles affects the sea to no great depth. But there is another reason why the expansion of the waters at the equator cannot amount to near 18 feet. It is this: the rate at which water expands as its temperature rises is not uniform, but increases with the temperature. Sea-water, according to Muncke's Table, in rising for example from 32° to 42° expands ·00047, whereas in rising from 70° to 80° it expands no less than ·00152. But these higher temperatures affect only a small quantity of water near the surface; the great depth of water below is affected by the lower temperatures, which do not produce much expansion. As no reliable observations, so far as I am aware, have been made to ascertain the rate at which the temperature of the waters at the equator decreases from the surface downwards to great depths, it is impossible to determine with any thing like accuracy the height at which the ocean, in virtue of higher temperature, should stand above the level of the ocean at the poles. But one thing we are certain of is that it must be very much under 18 feet, and that the force acting on the waters of the ocean to impel them forward as a current resulting from the difference of specific gravity between the sea in intertropical and polar regions, is very much under one-fourth of a grain per cubic foot. And if the sea in intertropical regions is much salter than the sea in polar regions, as Maury strongly insists, then this will make the force still less; for this will go so far to neutralize the effects due to difference of temperature between the waters of equatorial and polar regions.

It is perfectly evident that a pressure of one-fourth of a grain on the cubic foot of water, were it even so great as that, would be totally inadequate to overcome the mere molecular resistance of the water to go into motion, far less to produce the great currents of the ocean. It is therefore certain that ocean-currents are in no way whatever due to differences of specific gravity.

But it must be observed that this force of one-fourth of a grain per cubic foot would affect only the water at the surface; a very short distance below the surface the force would be absolutely insensible.

If water were perfectly fluid, and offered no resistance to motion, it would not only flow down an incline, however small it might be, but would flow down with an accelerated motion.

But water is not a perfect fluid, and its molecules do offer considerable resistance to motion. Water flowing down an incline, however steep it may be, soon acquires a uniform motion. There must therefore be a certain inclination below which no motion can take place. Experiments were made by M. Dubuat, with the view of determining this limit *. He found that when the inclination was 1 in 500,000, the motion of the water was barely perceptible; and he came to the conclusion that when the inclination is reduced to 1 in 1,000,000, all motion ceases. But the inclination afforded by the difference of temperature between the sea in equatorial and polar regions does not exceed the half of this, and consequently it can have absolutely no effect whatever in producing currents, no, not even the "trifling surface-drift" which Sir John Herschel is willing to attribute to it.

There is an error into which some writers appear to fall to which I may here refer. Suppose that at the equator we have to descend 10,000 feet before water equal in density to that at the poles is reached. We have in this case a plain with a slope of 10,000 feet in 6200 miles, forming the upper surface of the water of maximum density. Now this slope exercises no influence in the way of producing a current, as some seem to suppose; for this is not a case of disturbed equilibrium, but the reverse. This slope is the condition of static equilibrium when there is a difference between the temperature of the water at the equator and the poles. The only slope that has any tendency to produce motion of the water is the slope formed by the surface of the ocean in the equatorial regions being higher than the surface at the poles; but this is a slope of only 18 feet in 6200 miles.

Objections to Dr. Carpenter's *theory of a general interchange of equatorial and polar waters.*

Lieut. Maury's theory of a general interchange of water between the equator and the poles resulting from a difference of specific gravity, caused by difference of temperature, has lately been advocated by Dr. Carpenter †. He considers that the great masses of warm water found by him and his colleagues in their late important dredging-expeditions in the depths of the North Atlantic must be referred, not to the Gulf-stream, but to a general movement of water from the equatorial regions. "The inference seems inevitable," he says,

* Dubuat's 'Hydraulique,' tome i. p. 64 (1816). See also British Association Report for 1834, pp. 422, 451.

† See Proceedings of the Royal Society for Dec. 1868, Nov. 1869. Lecture delivered at the Royal Institution: 'Nature,' vol. i. p. 490.

"that the bulk of the water in the warm area must have come thither from the south-west. The influence of the Gulf-stream proper (meaning by this the body of superheated water which issues through the 'Narrows' from the Gulf of Mexico), if it reaches this locality at all (which is very doubtful), could only affect the *most superficial* stratum; and the same may be said of the surface-drift caused by the prevalence of south-westerly winds, to which some have attributed the phenomena usually accounted for by the extension of the Gulf-stream to these regions. And the presence of the body of water which lies between 100 and 600 fathoms deep, and the range of whose temperature is from 48° to 42°, can scarcely be accounted for on any other hypothesis than that of a *great general movement of equatorial water towards the Polar area,* of which movement the Gulf-stream constitutes a peculiar case modified by local conditions. In like manner the Arctic stream which underlies the warm superficial stratum in our cold area constitutes a peculiar case, modified by the local conditions to be presently explained, of *a great general movement of polar water towards the equatorial area,* which depresses the temperature of the deepest parts of the great oceanic basins nearly to the freezing-point."

In support of this theory of a general movement of water between equatorial and polar regions, Dr. Carpenter adduces the authority of Humboldt and of Prof. Buff *. I have been unable to find any thing in the writings of either from which it can be inferred that they have given this matter special consideration. Humboldt merely alludes to the theory, and that in the most casual manner; and that Prof. Buff has not carefully investigated the subject is apparent from the very illustration quoted by Dr. Carpenter from the 'Physics of the Earth.' "The water of the ocean at great depths," says Prof. Buff, "has a temperature, even under the equator, nearly approaching to the freezing-point. This low temperature cannot depend on any influence of the sea-bottom. . . . . The fact, however, is explained by a continual current of cold water flowing from the polar regions towards the equator. The following well-known experiment clearly illustrates the manner of this movement. A glass vessel is to be filled with water with which some powder has been mixed, and is then to be heated at bottom. It will soon be seen, from the motion of the particles of powder, that currents are set up in opposite directions through the water. Warm water rises from the bottom up through the middle of the vessel, and spreads over the surface, while the colder and therefore heavier liquid falls down at the sides of the glass."

This illustration is evidently intended to show not merely the

* Proceedings of the Royal Society, vol. xvii. p. 187, xviii. p. 463.

form and direction of the great system of oceanic circulation, but also the way that the circulation is caused by heat. It is no doubt true that if we apply heat (say that of a spirit-lamp) to the bottom of a vessel filled with water, the water at the bottom of the vessel will become heated and rise to the surface; and if the heat be continued an ascending current of warm water will be generated; and this, of course, will give rise to a compensating under current of colder water from all sides. In like manner it is also true that, if heat were applied to the bottom of the ocean in equatorial regions, an ascending current of hot water would be also generated, giving rise to an undercurrent of cold water from the polar regions. But all this is the diametrically opposite of what actually takes place in nature. The heat is not applied to the bottom of the ocean, so as to make the water there lighter than the water at the surface, and thus to generate an ascending current; but the heat is applied to the surface of the ocean, and the effect of this is to prevent an ascending current rather than to produce one, for it tends to keep the water at the surface lighter than the water at the bottom. In order to show how the heat of the sun produces currents in the ocean, Prof. Buff should have applied the heat, not to the bottom of his vessel, but to the upper surface of the water. But this is not all, the form of the vessel has something to do with the matter. The wider we make the vessel in proportion to its depth, the more difficult is it to produce currents by means of heat. But in order to represent what takes place in nature, we ought to have the same proportion between the depth and the superficial area of the water in our vessel as there is between the depth and the superficial area of the sea. The mean depth of the sea, according to Sir John Herschel, may be taken at about four miles*. It may be somewhat more, or it may be somewhat less, than this; but that will not materially affect our result. The distance between pole and pole we shall take in round numbers as 12,000 miles. The sun may therefore be regarded as shining upon a circular sea 12,000 miles in diameter and four miles deep. The depth of the sea to its diameter is therefore as 1 to 3000. Suppose, now, that in our experiment we make the depth of our vessel 1 inch, we shall require to make its diameter 3000 inches, or 250 feet. Let us, then, take a pool of water 250 feet in diameter, and 1 inch deep. Suppose the water to be at 32°. Apply heat to the upper surface of the pool, so as to raise the temperature of the surface of the water to 80° at the centre of the pool, the temperature diminishing towards the edge, where it is at 32°. It is found that at a depth of two miles the tempera-

* Physical Geography, article 17.

ture of the water at the equator is about as low as that of the poles. We must therefore suppose the water at the centre of our pool to diminish in temperature from the surface downwards, so that at a depth of half an inch the water is at 32°. We have in this case a thin layer of warm water half an inch thick at the centre, and gradually thinning off to nothing at the edge of the pool. The lightest water, be it observed, is at the surface, so that an ascending or a descending current is impossible. The only way whereby the heat applied can have any tendency to produce motion is this:—The heating of the water expands it, consequently the surface of the pool must stand at a little higher level at its centre than at its edge, where no expansion takes place; and therefore, in order to restore the level of the pool, the water at the centre will tend to flow towards the sides. But what is the amount of this tendency? Is it sufficient to overcome the molecular resistance of the water to go into motion? The amount of this tendency depends upon the amount of the slope. We have already seen that unless the slope exceeds 1 in 1,000,000, no motion can take place; but the slope in the case under consideration amounts to only 1 in 1,820,000; consequently motion is absolutely impossible.

That the great masses of warm water found by Dr. Carpenter in the North Atlantic cannot be due to currents produced by difference of temperature, as he supposes, can be proved in another way.

According to his theory there ought to be as much warm water flowing from intertropical regions towards the Antarctic regions as towards the Arctic. We may therefore, in our calculations, consider that the heat which is received in tropical regions to the south of the equator goes to warm the southern hemisphere, and the heat which it receives on the north side of the equator goes to warm the northern hemisphere. The warm currents found in the North Atlantic in temperate regions we may conclude came from the regions lying to the north of the equator—or, in other words, from that part of the Atlantic lying between the equator and the tropic of Cancer. At least, according to Dr. Carpenter's theory, we have no reason to believe that the quantity of warm water flowing from the tropical regions to the temperate and polar in the Atlantic is greater than the area between the equator and the tropic of Cancer can supply—because he maintains that a very large proportion of the cold water found in the North Atlantic came, not from the Arctic, but from the Antarctic regions. But if the North Atlantic is cooled by a cold stream from the southern hemisphere, the southern hemisphere in turn must be heated by a warm current from the North Atlantic—unless we assume, which is very improbable, that the compensating current flowing from the Atlantic into the southern hemisphere is as cold as the Antarctic current. But Dr. Carpenter admits that the quantity of warm water flowing from the Atlantic in equatorial regions towards the south is even greater than towards the north. "The unrestricted communication," he says, "which exists between the Antarctic area and the great Southern Ocean-basins would involve, if the doctrine of a general oceanic circulation be admitted, a much more considerable interchange of waters between the Antarctic and the Equatorial areas than is possible in the northern hemisphere" *. And as a proof that this is actually the case, he adduces the fact known to navigators that in the Southern Ocean there is a perceptible "set" of warm surface-water towards the Antarctic Pole.

We have already seen that, were it not for the great mass of warm water which finds its way to the polar regions, the temperature of these regions would be enormously lower than they really are. It was seen also that the comparatively high temperature of North-eastern Europe was due also to the same cause. But if it is doubtful whether the Gulf-stream reaches our shores, and if it is true that, even supposing it did, it "could only affect the *most superficial* stratum," and that the great mass of warm water found by Dr. Carpenter in his dredging-expeditions came directly from the equatorial regions, and not from the Gulf-stream, then the principal part of the heating-effect must be attributed, not to the Gulf-stream, but to the general flow of water from the equatorial regions. It surely would not, then, be too much to assume that the quantity of heat conveyed from equatorial regions by this general flow of water into the North Atlantic is at least equal to that conveyed by the Gulf-stream. Let us, then, assume that the total quantity of heat conveyed from equatorial regions into the North Atlantic and Arctic Ocean by all the various processes, the Gulf-stream included, is equal to twice that conveyed by the Gulf-stream.

We shall now consider whether the area of the Atlantic to the north of the equator is sufficient to supply the amount of heat demanded by Dr. Carpenter's theory.

The entire area of the Atlantic, extending from the equator to the tropic of Cancer, including the Caribbean Sea and the Gulf of Mexico, is about 7,700,000 square miles. In a former part of this paper † it was shown that, even assuming the volume of the Gulf-stream to be considerably less than one half what either Sir John Herschel or Lieut. Maury estimates it to be, the quantity of heat conveyed by the stream through the Straits

* 'Nature,' vol. i. p. 541. Proc. Roy. Soc. vol. xviii. p. 473.
† Phil. Mag. S. 4. vol. xxxix. p. 89.

of Florida is equal to all the heat received from the sun by 1,560,935 square miles at the equator. The annual quantity of heat received from the sun by the torrid zone per unit surface, taking the mean of the whole zone, is to that received by the equator as 39 to 40, consequently the quantity of heat conveyed by the Gulf-stream is equal to all the heat received by 1,600,960 square miles of the Atlantic in the torrid zone.

Dr. Carpenter is mistaken in supposing that "all the calculations which have been made as to the quantity of water which issues from the Narrows, and the amount of heat which it conveys, are based upon the assumption that both its temperature and its rate of movement are the same throughout its depths as they are at its surface"*. The surface-temperature of the stream at the Narrows is somewhat about 85°; but I have taken the mean temperature of the water at this place as only 65°. The cold return current, according to Dr. Carpenter, has a temperature as low as 30° or 32°; but, not to overestimate the quantity of heat derived from the Gulf-stream, I have taken the return current at 40°. In this case the quantity of heat conveyed through the Narrows I estimate to be 25 thermal units per pound of water. But had I taken the surface-temperature of the stream and Dr. Carpenter's estimate as to the temperature of the cold return current, I should have had 53 or 55 thermal units per pound as the amount conveyed. My data were derived, not from popular treatises on physical geography, but from a careful analysis of the sections and charts of the United-States Coast Survey; and any one who will be at the trouble to examine these will easily satisfy himself that I have underestimated both the temperature and volume of the stream.

But if, according to Dr. Carpenter's views, the quantity of heat conveyed from the tropical regions is double that conveyed by the Gulf-stream, the amount of heat in this case conveyed into the Atlantic in temperate regions will be equal to all the heat received from the sun by 3,201,920 square miles of the Atlantic between the equator and the tropic of Cancer. This is $\frac{32}{77}$ of all the heat received from the sun by that area.

Taking the annual quantity received per unit surface at the equator at 1000, the quantities received by the three zones would be respectively as follows:—

Equator	1000
Torrid zone	975
Temperate zone	757
Frigid zone	454

Now, if we remove from the Atlantic in tropical regions $\frac{32}{77}$

* 'Nature,' vol. ii. p. 334.

of the heat received from the sun, we remove 405 parts from every 975 received from the sun, and consequently only 570 parts per unit surface remain.

It has been already shown that the quantity of heat conveyed by the Gulf-stream from the equatorial regions into the temperate regions is equal to $\frac{100}{412}$ of all the heat received by the Atlantic in temperate regions*. But according to the theory under consideration the quantity removed is double this, or equal to $\frac{100}{206}$ of all the heat received from the sun. But the quantity received from the sun is equal to 757 parts per unit surface; add then to this $\frac{100}{206}$ of 757, or 367, and we have 1124 parts of heat per unit surface as the amount possessed by the Atlantic in temperate regions. The Atlantic should in this case be much warmer in temperate regions than in tropical; for in temperate regions it possesses 1124 parts of heat per unit surface, whereas in tropical regions it possesses only 570 parts per unit surface. Of course the heat conveyed from tropical regions does not all remain in temperate regions; a very considerable portion of it must pass into the arctic regions. Let us, then, assume that one half goes to warm the Arctic Ocean, and the other half remains in the temperate regions. In this case 183·5 parts would remain, and consequently $757+183{\cdot}5=940{\cdot}5$ parts would be the quantity possessed by the Atlantic in temperate regions, a quantity which still exceeds by no less than 370·5 parts the heat possessed by the Atlantic in tropical regions.

As one half of the amount of heat conveyed from the tropical regions is assumed to go into the Arctic Ocean, the quantity passing into that ocean would therefore be equal to what passes through the Straits of Florida, which amount we have already found to be equal to all the heat received from the sun by 6,873,800 square miles of the arctic regions†. But taking the volume of the Gulf-stream, as already stated, at one half our original estimate, the quantity of heat passing into the Arctic Ocean would therefore be equal to all the heat received by 3,436,900 square miles of the Arctic Ocean. The entire area covered by sea beyond the arctic circle is under 5,000,000 square miles; but taking the Arctic Ocean in round numbers at 5,000,000 square miles, the quantity of heat conveyed into it by currents to that received from the sun would therefore be as 3,436,900 to 5,000,000.

The amount received on the unit surface of the arctic regions we have seen to be 454 parts. The amount received from the currents would therefore be 312 parts. This gives 766 parts of heat per unit surface as the quantity possessed by the Arctic

* Phil. Mag. S. 4, vol. xxxix. p. 90.
† Ibid. p. 84.

Ocean. Then the Arctic Ocean also would possess more heat than the Atlantic in tropical regions; for the Atlantic in these regions possesses only 570 parts, whereas the Arctic Ocean possesses 766 parts. It is true that more rays are cut off in arctic regions than in tropical; but still, after making due allowance for this, the Arctic Ocean, if Dr. Carpenter's theory be correct, ought to be as warm as, if not warmer than, the Atlantic in tropical regions.

We may therefore conclude that there can be no such large quantity of warm water, in addition to that of the Gulf-stream, as Dr. Carpenter supposes, flowing into the North Atlantic from the equatorial regions; for there is not heat in those regions sufficient to supply such a current. We may also conclude that, at least in respect of the Atlantic, it is not correct that there is more warm water flowing from the equatorial regions into the southern hemisphere than into the northern; for a very large proportion of the heat conveyed by the Gulf-stream is derived from the southern hemisphere. In fact the great equatorial current, the feeder of the Gulf-stream, comes from the southern hemisphere.

The entire area of 7,700,000 square miles of sea in equatorial regions lying to the north of the equator would not be sufficient to supply the current passing through the Narrows of Bahama. Were the heat of the Gulf-stream all derived from the northern hemisphere, the following would then represent the relative quantities of heat per unit surface possessed by the Atlantic in the three zones, assuming that one half of the heat of the Gulf-stream passes into the arctic regions, and the other half remains to warm the temperate regions:—

From the Equator to the Tropic of Cancer . .	773
From the Tropic of Cancer to the Arctic Circle	848
From the Arctic Circle to the North Pole . .	610

These figures show that, were it not that a very large proportion of the heat possessed by the Gulf-stream is derived from the southern hemisphere, the Atlantic, from the equator to the tropic of Cancer, would be as cold as from the tropic of Cancer to the North Pole.

The comparatively high temperature which prevails in the northern parts of the Atlantic and in the Arctic seas is therefore to a considerable extent due to heat derived from the southern hemisphere. And no doubt this transference of heat from the southern hemisphere to the northern by means of ocean-currents, as was mentioned on a former occasion *, is the cause

* Phil. Mag. vol. xxxix. p. 103.

why the mean temperature of the southern hemisphere is so much lower than that of the northern.

We shall now proceed to consider the objections which have been urged against the theory that ocean-currents are due to the impulse of the trade-winds.

[To be continued.]

Erratum.

In Part I. of this paper, vol. xxxix. p. 89, 8th line from bottom, *for* 9·83 *read* 9·08.

XXX. *On Statical and Dynamical Ideas in Chemistry.*—Part II. *Chemical Substance and Chemical Functions.* *By* Edmund J. Mills, *D.Sc.**

IN the preceding Part the history of the ideas connected with acid, alkali (base), and salt was concisely stated, and it was shown that while, on the one hand, those ideas are erroneous and self-contradictory when they designate something *particular*, so, on the other hand, the most consistent and general theory that has been stated with respect to them is that of Avogadro, who is their modern expounder in the sense of chemical polarity. These results were in harmony with the idea of motion, the criterion adopted in these papers. The practical result is that there is no such thing as *an* acid, base, or salt, though the use of the adjectives and qualitative nouns derived from these terms might probably be successfully defended. If any one deny this conclusion he is bound to give a satisfactory definition of *an acid*, for example—a task in which, as history clearly shows, success is unlikely to accrue.

Having thus pointed out the value of the idea of motion in the concrete sphere of external chemistry, I may now penetrate, or perhaps ascend, to the remoter regions of Chemical Substance and Chemical Functions, where the service of the same idea will prove available.

1. *Chemical Substance.*

We are accustomed, in the language of everyday chemistry, to say that such and such bodies or substances undergo certain operations; sulphur, hydric nitrate, aniline, &c. are spoken of as bodies or substances indifferently. In recording the facts of an analysis (even of a mechanical mixture), it is customary to say that so much *substance* contained or furnished so much of a product; and this product may be volatile *matter* or organic *matter*, which, in its turn, may become *substance* for analysis. *A* che-

* Communicated by the Author. For Part I. see Phil. Mag. 1869, vol. xxxvii. p. 461.