gate resistances, the resistance of the latter does not appear in the expression for the resistance external to the galvanometer, which is not in any way affected by the battery resistance. Similarly for the resistance external to the battery, which, at a balance, is independent of the galvanometer resistance. I cannot agree with Mr. Brough that to find the resistance in either case at a balance is a mere mathematical problem destitute of physical meaning; for it is only when at a balance that the problem has any practical importance.

As Mr. Brough most truly observes, most Wheatstone's bridges are wrongly arranged. An excellent example of this once came under my notice. A gentleman informed me he was about to make a Wheatstone's bridge, a great improvement, and very economical. Instead of using three separate sets of resistance-coils (a, b, c) he would use only one (c); for he would make a and b equal to 0. Mr. Harris's arrangement appeared to succeed admirably. There was no difficulty whatever in getting a zero; in fact there was always a balance, whether the line under examination was long or short. There was only one drawback; and that was, the improvement afforded no information whatever as to the resistance of the line.

I am &c.,

#### OLIVER HEAVISIDE.

P.S.—The condition that the galvanometer should connect the junction of the two greatest with the junction of the two least of the resistances, is necessarily complied with by the equations I have given for the best arrangement with a given galvanometer and battery; else it would not be the best arrangement.

XIV. On Ocean-currents.—Part III. On the Physical Cause of Ocean-currents. By JAMES CROLL, of the Geological Survey of Scotland.

[Continued from vol. xlii. p. 280.]

Further Examination of the Gravitation Theory of Oceanic Circulation.

#### Introduction.

**T**<sup>EW</sup> subjects have excited more interest and attention than the cause of ocean circulation; and yet few are in a more imperfect and unsatisfactory condition, nor is there any question regarding which a greater diversity of opinion has prevailed. Our incomplete acquaintance with the facts relating to the currents of the ocean and the modes of circulation actually in operation, is no doubt one reason for this state of things. But doubtless the principal cause of such diversity of opinion lies in the fact that the question is one which properly belongs to the domain of physics and mechanics, while as yet no physicist of note (if we except Dr. Colding, of Copenhagen) has given, as far as I know, any special attention to the subject. It is true that in works of meteorology and physical geography reference is continually made to such eminent physicists as Herschel, Pouillet, Buff, and others; but when we turn to the writings of these authors we find merely a few remarks expressive of their opinions on the subject, and no special discussion or investigation of the matter, nor any thing which could warrant us in concluding that such investigations have ever been made. At present the question cannot be decided by a reference to authorities.

The various theories on the subject may be classed under two divisions: the first of these attributes the motion of the water to the impulse of the wind, and the second to the force of gravity resulting from difference of density. The latter may be subdivided into two classes. The first of these (of which Maury may be regarded as the representative) attributes the Gulf-stream and other sensible currents of the ocean to difference of specific gravity. The other class (at present the more popular of the two, and of which Dr. Carpenter may be considered the representative) denies altogether that such currents can be produced by difference of specific gravity\*, and affirms that there is a general movement of the upper portion of the ocean from the equator to the poles, and a counter movement of the under portion from the poles to the equator. This movement is attributed to difference of specific gravity between equatorial and polar water, resulting from difference of temperature.

The former theory I examined at some length in a paper in the Philosophical Magazine for October 1870, and the latter theory in a paper in the same journal for October 1871. Since then Dr. Carpenter has done me the honour, in a paper read before the Royal Society +, to discuss at considerable length the various objections advanced by me to his theory. He has also in this memoir stated and explained his views on several points more fully than on former occasions. He further restates at some length the various facts for which his theory is designed to account, facts which he considers I have never attempted to explain. This to a certain extent is true; for as yet I have not reached that part of my paper "On Ocean-currents" in which these points fall to be discussed. One of the objects of the present paper is to endeavour to show that all the facts to which Dr. Carpenter refers can be perfectly well explained without having recourse to any such general movement of the ocean as he

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<sup>\*</sup> Proceedings of the Royal Society, No. 138, p. 596, foot-note.

<sup>†</sup> See Proc. Roy. Soc. No. 138.

assumes to exist. I have also considered more in detail what seem to me to be the radical defects of his theory, and have again reviewed some matters regarding which he appears to have slightly misapprehended the drift of my argument. It was shown on a former occasion that, if the heat received by the ocean in intertropical regions were distributed over the globe, not by currents produced by the wind, but by means of a circulation due to difference of temperature between equatorial and polar waters, then there could be no secular changes of climate resulting from variations in the eccentricity of the earth's orbit-because such a mode of circulation would, as I have shown, tend to neutralize the effects which would otherwise result from an increase of eccentricity. For this reason I have been the more anxious to prove that intertropical heat is conveyed to temperate and polar regions by ocean-currents, and not by means of any general movement of the ocean resulting from difference of gravity. Ŧ have therefore on this account entered more fully into that part of the subject than I otherwise would have done. Irrespective of all this, however, the important nature of the whole question, and the very general interest it excites, may be regarded as sufficient excuse for the length of the present communication. Circumstances over which I had no control have delayed its publication for nearly a year.

#### The Facts and their Explanation.

"I have thought it desirable," says Dr. Carpenter, "to develope somewhat at length what I regard as the bearings of the results obtained by these inquiries upon the doctrine of a general oceanic circulation sustained by difference of temperature. . . . As no similarly comprehensive examination has been made, so far as I am aware, by any other scientific inquirer, and as the doctrine put forth on the subject by Mr. Croll is likely, if not thus scrutinized, to command the unquestioning assent of those who regard him as a high authority on the subject of oceanic currents and their bearings on geological questions, I venture to hope that the conclusion of its results as an appendix to this Report will not be deemed inappropriate" (p. 538).

The Facts to be explained.—He then commences by giving a restatement of the facts for the explanation of which his theory of a general oceanic circulation has been advanced. It is well known that, wherever temperature-observations have been made in the Atlantic, the bottom of that ocean has been found to be occupied by water of an ice-cold temperature. And this holds true not merely of the Atlantic, but also of the ocean in intertropical regions—a fact which has been proved by repeated observations, and more particularly of late by those of Commander Chimmo in the China

Sea and Indian Ocean, where a temperature as low as 32° Fahr. was found at a depth of 2656 fathoms. In short the North Atlantic, and probably the intertropical seas also, may be regarded, Dr. Carpenter considers, as divided horizontally into two great layers or strata—an upper warm, and a lower cold stratum. All these facts I, of course, freely admit; nor am I aware that their truth has been called in question by any one, no matter what his views may have been as to the mode in which they are to be explained.

The Explanation of the Facts.-We have next the explanation of the facts, which is simply this :- The cold water occupying the bottom of the Atlantic and of intertropical seas is to be accounted for by the supposition that it came from the polar regions. This is obvious, because the cold possessed by the water could not have been derived from the crust of the earth beneath : neither could it have come from the surface; for the temperature of the bottom water is far below the normal temperature of the latitude in which it is found. Consequently "the inference seems irresistible that this depression must be produced and maintained by the convection of cold from the polar towards the equatorial area." Of course, if we suppose a flow of water from the poles towards the equator, we must necessarily infer a counter flow from the equator towards the poles; and while the water flowing from equatorial to polar regions will be warm, that flowing from polar to equatorial regions will be cold. The doctrine of a mutual interchange of equatorial and polar water is therefore a necessary consequence from the admission of the foregoing facts. With this explanation of the facts I need hardly say that I fully agree; nor am I aware that its correctness has ever been disputed. Dr. Carpenter surely cannot charge me with overlooking the fact of a mutual interchange of equatorial and polar water, seeing that my estimate of the thermal power of the Gulf-stream, from which it is proved that the amount of heat conveyed from equatorial to temperate and polar regions is enormously greater than had ever been anticipated, was made a considerable time before he began to write on the subject of oceanic circulation\*. And in my paper "On Ocean-currents in relation to the Distribution of Heat over the Globe"+, I have endeavoured to show that, were it not for the raising of the temperature of polar and high temperate regions and the lowering of the temperature of intertropical regions by means of this interchange of water, these portions of the globe would not be habitable by the present existing orders of beings.

\* Trans. of Glasgow Geol. Soc. for April 1867. Phil. Mag. for Feb. 1867 and June 1867 (Supplement).

† Phil. Mag. for February 1870.

Phil, Mag. S. 4. Vol. 47. No. 310, Feb. 1874.

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The explanation goes further :--- "It is along the surface and upper portion of the ocean that the equatorial waters flow towards the poles, and it is along the bottom and under portion of the ocean that polar waters flow towards the equator; or, in other words, the warm water keeps the upper portion of the ocean and the cold water the under portion." With this explanation I to a great extent agree. It is evident that, in reference to the northern hemisphere at least, the most of the water which flows from intertropical to polar regions (as, for example, the Gulf-stream) keeps to the surface and upper portion of the ocean ; but, for reasons which I have stated in my last paper\*, a very large proportion of this water must return in the form of under currents; or, which is the same thing, the return compensating current, whether it consist of the actual water which originally came from the equator or not, must flow towards the equator as an under current. That the cold water which is found at the bottom of the Atlantic and of intertropical seas must have come as under currents is perfectly obvious, because water which should come along the surface of the ocean from the polar regions would not be cold when it reached intertropical regions.

The explanation hypothetical.—Here the general agreement between us in a great measure terminates; for Dr. Carpenter is not satisfied with the explanation generally adopted by the advocates of the wind theory, viz. that the cold water found in temperate and intertropical areas comes from polar regions as compensating under currents, but advances a hypothetical form of circulation to account for the phenomenon. He assumes that there is a general set or flow of the surface and upper portion of the ocean from the equator to polar regions, and a general set or flow of the bottom and under portion of the ocean from polar regions to the equator. Mr. Ferrel ('Nature,' June 13, 1872) speaks of that "interchanging motion of the water between the equator and the pole discovered by Dr. Carpenter." In this, however, Mr. Ferrel is mistaken; for Dr. Carpenter not only makes no claim to any discovery of the kind, but distinctly admits that none such has yet been made. Although in some of his papers he speaks of a "set of warm surface-water in the southern oceans toward the Antarctic pole" as being well known to navigators, yet he nowhere affirms, as far as I know, that the existence of such a general oceanic circulation as he advocates has ever been directly determined from observations. This mode of circulation is simply inferred or assumed in order to account for the facts referred to above. "At present," Dr. Carpenter says, "I claim for it no higher character than that \* Phil. Mag. for October 1871, p. 267.

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of a good working hypothesis to be used as a guide in further inquiry" (§ 16); and lest there should be any misapprehension on this point, he closes his memoir thus:---"At present, as I have already said, I claim for the doctrine of a general oceanic circulation no higher a character than that of a good working hypothesis consistent with our present knowledge of facts, and therefore entitled to be provisionally adopted for the purpose of stimulating and directing further inquiry."

I am unable to agree with Dr. Carpenter on this latter point. It seems to me that there is no necessity for adopting any hypothetical mode of circulation to account for the facts, as they can be quite well accounted for by means of that mode of circulation which does actually exist. It has been determined from direct observation that surface-currents flow from equatorial to polar regions; and their paths have been actually mapped out. But if it is established that currents flow from equatorial to polar regions, it is equally established that return currents flow from polar to equatorial regions; for if the one actually exists, the other of necessity must exist. We know also on physical grounds, to which I have already referred, and which fall to be considered more fully in a subsequent part of this paper, that a very large portion of the water flowing from polar to equatorial regions must be in the form of under currents. If there are cold under currents, therefore, flowing from polar to temperate and equatorial regions, this is all that we really require to account for the cold water which is found to occupy the bed of the ocean in those regions. It does not necessarily follow, because cold water may be found at the bottom of the ocean all along the equator, that there must be a direct flow from the polar regions to every point of the equator. Water brought constantly from the polar regions to various points along the equator by means of under currents will necessarily accumulate, and in course of time spread over the bottom of the intertropical seas. It must either do this, or the currents on reaching the equator must bend upwards and flow to the surface in an unbroken mass. Considerable portions of some of those currents may no doubt do so and join surface-currents; but probably the greater portion of the water coming from polar regions extends itself over the floor of the equatorial seas. In a letter in 'Nature,' Jan. 11, 1872, I endeavoured to show that the surface-currents of the ocean are not separate and independent of one another, but form one grand system of circulation, and that the impelling cause keeping up this system of circulation is not the trade-winds alone, as is generally supposed, but the prevailing winds of the entire globe considered also as one grand system. The evidence for this opinion, however, will be considered more fully in the next part of this paper.

Although the under currents are parts of one general system of oceanic circulation produced by the impulse of the system of prevailing winds, yet their direction and position are nevertheless to a large extent determined by different laws. The water at the surface, being moved by the force of the wind, will follow the path of greatest pressure and traction,—the effects resulting from the general contour of the land, which to a great extent are common to both sets of currents, not being taken into account; while, on the other hand, the under currents from polar regions (which to a great extent are simply "indraughts" compensating for the water drained from equatorial regions by the Gulf-stream and other surface-currents) will follow, as a general rule, the path of *least resistance*.

The Cause assigned for the hypothetical mode of circulation.— Dr. Carpenter assigns a cause for his mode of circulation ; and that cause he finds in the difference of specific gravity between equatorial and polar waters, resulting from the difference of temperature between these two regions. "Two separate questions," he says, " have to be considered, which have not, perhaps, been kept sufficiently distinct either by Mr. Croll or by myself :--first, whether there is adequate evidence of the existence of a general vertical oceanic circulation; and second, whether, supposing its existence to be provisionally admitted, a vera causa can be found for it in the difference of temperature between the oceanic waters of the polar and equatorial areas" (§ 17). It seems to me that the facts adduced by Dr. Carpenter do not necessarily require the assumption of any such mode of circulation as that advanced by him. The phenomena can be satisfactorily accounted for otherwise; and therefore there does not appear to be any necessity for considering whether his hypothesis be sufficient to produce the required effect or not.

An important consideration overlooked.—But there is one important consideration which Dr. Carpenter seems to have overlooked—namely, the fact that the sea is salter in intertropical than in polar regions, and that this circumstance, so far as it goes, must tend to neutralize the effect of difference of temperature. It is probable indeed that the effect produced by difference of temperature is thus entirely neutralized, and that no difference of density whatever exists between the sea in intertropical and polar regions, and consequently that there is no difference of level nor any thing to produce such a general motion as he supposes. This I am glad to find is the opinion of Professor Wyville Thomson.

"I am greatly mistaken," says that author, "if the low specific gravity of the polar sea, the result of the condensation and precipitation of vapour evaporated from the intertropical area,

do not fully counterbalance the contraction of the superficial film by arctic cold. . . . Speaking in the total absence of all reliable data, it is my general impression that if we were to set aside all other agencies, and to trust for an oceanic circulation to these conditions only which are relied upon by Dr. Carpenter, if there were any general circulation at all, which seems very problematical, the odds are rather in favour of a warm undercurrent travelling northwards by virtue of its excess of salt, balanced by a surface return-current of fresher though colder arctic water." ('The Depths of the Sea,' pp. 376 & 377.)

This is what actually takes place on the west and north-west of Spitzbergen. There the warm water of the Gulf-stream flows underneath the cold polar current. And it is the opinion of Dr. Scoresby, Clements Markham, and Lieut. Maury that this warm water, in virtue of its greater saltness, is denser than the polar water. Mr. Leigh Smith found on the north-west of Spitzbergen the temperature at 500 fathoms to be 52°, and once even 64°, while the water on the surface was only a degree or two above freezing\*. Mr. Aitken, of Darroch, in a paper lately read before the Royal Scottish Society of Arts, showed experimentally that the polar water in regions where the ice is melting is actually less dense than the warm and more salt tropical waters. Nor will it help the matter in the least to maintain that difference of specific gravity is not the reason why the warm water of the Gulf-stream passes under the polar stream-because if differences of specific gravity be not the cause of the warm water underlying the cold water in polar regions, then difference of specific gravity may likewise not be the cause of the cold water underlying the warm at the equator; and if so, then there is no necessity for the gravitation hypothesis of oceanic circulation.

There is little doubt that the superheated stratum at the surface of the intertropical seas, which stratum, according to Dr. Carpenter, is of no great thickness, is less dense than the polar water; but if we take a column extending from the surface down to the bottom of the ocean, this column at the equator will be found to be as heavy as one of equal length in the polar area. And if this be the case, then there can be no difference of level between the equator and the poles, and no disturbance of static equilibrium nor any thing else to produce circulation.

Under currents account for all the Facts better than Dr. Carpenter's Hypothesis.—Assuming, for the present, the system of prevailing winds to be the true cause of oceanic currents, it necessarily follows (as will be shown hereafter) that a large quantity of Atlantic water must be propelled into the Arctic Ocean; and such, as we know, is actually the case. But the Arctic

\* The Threshold of the Unknown Region, p. 95.

Ocean being, as Professor Wyville Thomson remarks, a wellnigh closed basin, not permitting of a free outflow into the Pacific Ocean of the water impelled into it, and the general tendency of the winds being to prevent or retard the return of the water into the Atlantic, the path of least resistance for the return currents must lie at a considerable distance below the surface. A large portion of the water from the polar regions no doubt leaves those regions as surface-currents; but a surfacecurrent of this kind, on meeting with some resistance to its onward progress along the surface, will dip down and continue its course as an under current. We have an example of this in the case of the polar current, which upon meeting the Gulf-stream on the banks of Newfoundland divides-a portion of it dipping down and pursuing its course underneath that stream into the Gulf of Mexico and the Caribbean Sea. And that this under current is a real and tangible current, in the proper sense of the term, and not an imperceptible movement of the water, is proved by the fact that large icebergs deeply immersed in it are often carried southward with considerable velocity against both the force of the wind and the Gulf-stream.

Dr. Carpenter refers at considerable length (§ 134) to Mr. Mitchell's opinion as to the origin of the polar current, which is the same as that advanced by Maury, viz. that the impelling cause is difference of specific gravity. But although Dr. Carpenter quotes Mr. Mitchell's opinion, he nevertheless does not appear to adopt it; for in §§ 90-93 and various other places he distinctly states that he does not agree with Captain Maury's view that the Gulf-stream and polar current are caused by difference of density. In fact Dr. Carpenter seems particularly anxious that it should be clearly understood that he dissents from the theory maintained by Maury. But he does not merely deny that the Gulf-stream and polar current can be caused by difference of density; he even goes so far as to affirm that no sensible current whatever can be due to that cause, and adduces the authority of Sir John Herschel in support of that opinion :-- "The doctrine of Captain Maury," he says, "was powerfully and convincingly opposed by Sir John Herschel; who showed, beyond all reasonable doubt, first, that the Gulfstream really has its origin in the propulsive force of the tradewinds, and, secondly, that the greatest disturbance of equilibrium which can be supposed to result from the agencies invoked by Captain Maury would be utterly inadequate to generate and maintain either the Gulf-stream or any other sensible current" (§ 92). This being Dr. Carpenter's belief, it is somewhat singular that he should advance the case of the polar current passing under the Gulf-stream as evidence in favour of his theory; for in reality he could hardly have selected a case more hostile to that theory. In short it is evident that, if a polar current impelled by a force other than that of gravity can pass from the banks of Newfoundland to the Gulf of Mexico (a distance of some thousands of miles) under a current flowing in the opposite direction and, at the same time, so powerful as the Gulf-stream, it could pass much more easily under comparatively still water, or water flowing in the same direction as itself. And if this be so, then all our difficulties disappear, and we satisfactorily explain the presence of cold polar water at the bottom of intertropical seas without having recourse to the hypothesis advanced by Dr. Carpenter.

But we have an example of an under current more inexplicable on the gravitation hypothesis than even that of the polar current, viz. the warm under current of Davis Strait.

There is a strong current flowing north from the Atlantic through Davis Strait into the Arctic Ocean underneath a surface-current passing southwards in an opposite direction. Large icebergs have been seen to be carried northwards by this under current at the rate of four knots an hour against both the wind and the surface-current, ripping and tearing their way with terrific force through surface-ice of great thickness. (See Physical Geography of the Sea, chap. ix. new edition, and Dr. A. Mühry 'On Ocean-currents in the Circumpolar Basin of the N. Hemisphere.') A current so powerful and rapid as this cannot, as Dr. Carpenter admits, be referred to difference of specific gravity. But even supposing that it could, still difference of temperature between the equatorial and polar sea swould not account for it; for the current in question flows in the wrong direction. Nor will it help the matter the least to adopt Maury's explanation, viz. that the warm under current from the south, in consequence of its greater saltness, is denser than the cold one from the polar regions. For if the water of the Atlantic, notwithstanding its higher temperature, is in consequence of its greater saltness so much denser than the polar water on the west of Greenland as to produce an under current of four knots an hour in the direction of the pole. then surely the same thing to a certain extent will hold true in reference to the ocean on the east side of Greenland. Thus instead of there being, as Dr. Carpenter supposes, an underflow of polar water south into the Atlantic in virtue of its greater density, there ought, on the contrary, to be a surface-flow in consequence of its lesser density.

The true explanation no doubt is, that the warm under current from the south and the cold upper current from the north are both parts of one grand system of circulation produced by the winds, difference of specific gravity having no share whatever

either in impelling the currents, or in determining which shall be the upper and which the lower.

The wind in Baffin's Bay and Davis Strait blows nearly always in one direction, viz. from the north. The tendency of this is to produce a surface- or upper current from the north down into the Atlantic, and to prevent or retard any surfacecurrent from the south. The warm current from the Atlantic, taking the path of least resistance, dips under the polar current and pursues its course as an under current.

Mr. Clement Markham, in his 'Threshold of the Unknown Region,' is inclined to attribute the motion of the icebergs to tidal action or to counter undercurrents. That the motion of the icebergs cannot reasonably be attributed to the tides is, I think, evident from the descriptions given both by Midshipman Griffin and by Captain Duncan, who distinctly saw the icebergs moving at the rate of about four knots an hour against a surface-current flowing southwards. And Captain Duncan states that the bergs continued their course northwards for several days, till they ultimately disappearedt. The probability is that this northward current is composed partly of Gulf-stream water and partly of that portion of polar water which is supposed to flow round Cape Farewell from the east coast of Greenland. This stream, composed of both warm and cold water, on reaching to about latitude 65° N., where it encounters the strong northerly winds, dips down under the polar current and continues its northward course as an under current.

We have on the west of Spitzbergen, as has already been noticed, a similar example of a warm current from the south passing under a polar current. A portion of the Gulf-stream which passes round the west coast of Spitzbergen flows under an Arctic current coming down from the north; and it does so no doubt because it is here in the region of prevailing northerly winds, which favour the polar current but oppose the Gulfstream. Again, we have a cold and rapid current sweeping round the east and south of Spitzbergen, a curreni of which Mr. Lamont asserts that he is positive he has seen it running at the rate of seven or eight miles an hour. This current, on meeting the Gulf-stream about the northern entrance to the German Ocean, dips down under that stream and pursues its course southwards as an under current.

Several other cases of under currents might be adduced which cannot be explained on the gravitation theory, and which must be referred to a system of oceanic circulation produced by the impulse of the wind; but these will suffice to show that the assumption that the winds can produce only a mere surface-drift is directly opposed to facts. And it will not do to affirm that a current which forms part of a general system of circulation produced by the impulse of the winds cannot possibly be an under current; for in the case referred to we have proof that the thing is not only possible but actually exists. This point, however, will be better understood after we have considered the evidence in favour of a general system of oceanic currents.

Much of the difficulty experienced in comprehending how under currents can be produced by the wind, or how an impulse imparted to the surface of the ocean can ever be transmitted to the bottom, appears to me to result, to a considerable extent at least, from a slight deception of the imagination. The thing which impresses us most forcibly in regard to the ocean is its profound depth. A mean depth of, say, three miles produces a striking impression; but if we could represent to the mind the vast area of the ocean as correctly as we can its depth, shallowness rather than depth would be the impression produced. If in crossing a meadow we found a sheet of water one hundred vards in diameter and only an inch in depth, we should not call that a deep pool, we should call it a very shallow pool. The probability is that we should speak of it as simply a piece of ground covered with a thin layer of water. Yet such a thin layer of water would be a correct representation in miniature of the ocean: for the ocean in relation to its superficial area is as shallow as the pool of our illustration. In reference to such a pool or thin film of water, we have no difficulty in conceiving how a disturbance on its surface would be transmitted to its bottom. In fact our difficulty is in conceiving how any disturbance extending over its entire surface should not extend to the bottom. Now if we could form as accurate a sensuous impression of the vast area of the ocean as we do of such a pool, all our difficulty in understanding how the impulses of the wind acting on the vast area of the ocean should communicate motion down to its bottom would disappear.

The known condition of the ocean inconsistent with Dr. Carpenter's hypothesis.—Dr. Carpenter says that he looks forward with great satisfaction to the results of the inquiries which are being prosecuted by the Circumnavigation Expedition, in the hope that the facts brought to light may establish his theory of a general oceanic circulation; and he specifies certain of these facts which, if found to be correct, will establish his theory. It seems to me, however, that the facts to which he refers are just as explicable on the theory of under currents as on the theory of a general oceanic circulation. He begins by saying, "If the views I have propounded be correct, it may be expected that near the border of the great Antarctic ice-barrier a temperature below 30° will be met with (as it has been by Parry, Martens, and Weyprecht near Spitzbergen) at no great depth beneath the surface, and that instead of rising at still greater depths, the thermometer will fall to near the freezing-point of salt water " (§ 39).

Dr. Carpenter can hardly claim this as evidence in favour of his theory; for near the borders of the ice-barrier the water, as a matter of course, could not be expected to have a much higher temperature than the ice itself. And if the observations be made during summer months, the temperature of the water at the surface will no doubt be found to be higher than that of the bottom; but if they be carried on during winter, the surfacetemperature will doubtless be found to be as low as the bottomtemperature. These are results which do not depend upon any particular theory of oceanic circulation.

"The bottom-temperature of the North Pacific," he continues, "will afford a crucial test of the truth of the doctrine. For since the sole communication of this vast occanic area with the Arctic basin is a strait so shallow as only to permit an inflow of warm surface-water, its deep cold stratum must be entirely derived from the Antarctic area; and if its bottom-temperature is not actually higher than that of the Scuth Pacific, the glacial stratum ought to be found at a greater depth north of the equator than south of it" (§ 39).

This may probably show that the water came from the Antarctic regions, but cannot possibly prove that it came in the manner which he supposes.

"In the North Atlantic, again, the comparative limitation of communication with the Arctic area may be expected to prevent its bottom-temperature from being reduced as low as that of the Southern Atlantic" (§ 39). Supposing the bottom-temperature of the South Atlantic should be found to be lower than the bottom-temperature of the North Atlantic, this fact will be just as consistent with the theory of under currents as with his theory of a general movement of the ocean. Indeed I fear that even although Dr. Carpenter's expectations should eventually be realized in the results of the Circumnavigation Expedition, yet the advocates of the wind theory will still remain unconverted. In fact the Director of this Expedition has already, on the wind theory, offered an explanation of nearly all the phenomena on which Dr. Carpenter relies\*; and the same has also been done by Dr. Petermann+, who, as is well known, is equally opposed to Dr. Carpenter's theory. Dr. Carpenter directs attention to the necessity of examining the broad and deep channel separating

<sup>\* &</sup>quot;Depths of the Sea." 'Nature' for July 28, 1870.

<sup>† &</sup>quot;Memoir on the Gulf-stream," Geographische Mittheilungen for vol. xvi. (1870).

Iceland from Greenland. The observations which have already been made, however, show that nearly the entire channel is occupied, on the surface at least, by water flowing southward from the polar area-a direction the opposite of what it ought to be according to the gravitation theory. In fact the surface of one half of the entire area of the ocean, extending from Greenland to the North Cape, is moving in a direction the opposite of that which it ought to take according to the theory under review. The western half of this area is occupied by water which at the surface is flowing southwards; while the eastern half, which has hitherto been regarded by almost everybody but Dr. Carpenter himself and Mr. Findlay as an extension of the Gulf-stream, is moving polewards. The motion of the western half must be attributed to the winds and not to gravity; for it is moving in the wrong direction to be accounted for by the latter cause; but had it been moving in the opposite direction, no doubt its motion would have been referred to gravitation. To this cause the motion of the eastern half, which is in the proper direction, is attributed\*; but why not assign this motion also to the impulse of the winds, more especially since the direction of the prevailing winds blowing over that area coincides with that of the water? If the wind can produce the motion of the water in the western half, why may it not do the same in the eastern half?

If there be such a difference of density between the equatorial and polar water as to produce a general flow of the upper portion of the ocean poleward, how does it happen that one half of the water in the above area is moving in opposition to gravity? How is it that in a wide open sea gravitation should act so powerfully in the one half of it and with so little effect in the other half? There is probably little doubt that the ice-cold water of the western half extends from the surface down to the bot-And it is also probable that the bottom-water is moving tom. southwards in the same direction as the surface-water. The bottom-water in such a case would be moving in harmony with the gravitation theory; but would Dr.Carpenter on this account attribute its motion to gravity? Would he attribute the motion of the lower half to gravity and the upper half to the wind? He could not in consistency with his theory attribute the motion of the upper half to gravity; for although the ice-cold water extended to the surface, this could not explain how gravity should move it southward instead of polewards, as according to theory it ought to move. He might affirm, if he chose, that the surface-water moves southwards because it is dragged forward by the bottom-water; but if this view be held, he is not entitled to

\* Dr. Carpenter "On the Gulf-stream," Proc. of Roy. Geog. Soc. for January 9, 1871, § 29.

affirm, as he does, that the winds can only produce a mere surface-drift. If the viscosity and molecular resistance of water be such that, when the lower strata of the ocean are impelled forward by gravity or by any other cause, the superincumbent strata extending to the surface are perforce dragged after them, then, for the same reason, when the upper strata are impelled forward by the wind or any other cause, the underlying strata must also be dragged along after them.

If the condition of the ocean between Greenland and the north-western shore of Europe is irreconcilable with the gravitation theory, we find the case even worse for that theory when we direct our attention to the condition of the ocean on the southern hemisphere; for according to the researches of Captain Duperrey and others on the currents of the Southern Ocean, a very large portion of the area of that ocean is occupied by water moving on the surface more in a northward than a poleward direction. Referring to the deep trough between the Shetland and the Faroe Islands, called by him the "Lightning Channel," Dr. Carpenter says, "If my view be correct, a current-drag suspended in the *upper* stratum ought to have a perceptible movement in the N.E. direction; whilst another, suspended in the *lower* stratum, should move S.W." (§ 40).

Any one believing in the north-eastern extension of the Gulfstream and in the Spitzbergen polar under current, to which I have already referred, would not feel surprised to learn that the surface-strata have a perceptible north-eastward motion, and the bottom strata a perceptible south-westward motion. Northeast and east of Iceland there is a general flow of cold polar water in a south-east direction towards the left edge of the Gulfstream. This water, as Professor Mohn concludes, "descends beneath the Gulf-stream and partially finds an outlet in the lower half of the Faroe-Shetland channel"\*.

#### The Mechanics of the Theory.

"I now proceed," says Dr. Carpenter, "to the second head of the discussion, viz. the demonstration which Mr. Croll considers himself to have given, that the difference of temperature between polar and equatorial water cannot possibly produce the effect I attribute to it" (§ 21).

"Mr. Croll's whole manner of treating the subject is so different from that which it appears to me to require, and he has so completely misapprehended my own view of the question, that I feel it requisite to present this in fuller detail, in order that physicists and mathematicians, having both sides fully before them, may judge between us" (§ 26).

\* Dr. Petermann's Mittheilungen for 1872, p. 315.

Dr. Carpenter then refers to a point so obvious as hardly to require consideration, viz. the effect which results when the surface of the entire area of a lake or pond of water is cooled. The whole of the surface-film being chilled at the same time, sinks through the subjacent water, and a new film from the warmer layer immediately beneath the surface rises into its place. This being cooled in its turn, sinks, and so on. He next considers what takes place when only a portion of the surface of the pond is cooled, and shows that in this case the surface-film which descends is replaced not from beneath, but by an inflow from the neighbouring area.

"That such must be the case," says Dr. Carpenter, "appears to me so self-evident that I am surprised that any person conversant with the principles of physical science should hesitate in admitting it, still more that he should explicitly deny it. But since others may feel the same difficulty as Mr. Croll, it may be worth while for me to present the case in a form of yet more elementary simplicity" (§ 29).

Then, in order to show the mode in which the general oceanic circulation takes place, he supposes two cylindrical vessels, W and C, of equal size to be filled with sea-water. Cylinder, W represents the equatorial column, and the water contained in it has its temperature maintained at 60°; whilst the water in the other cylinder C, representing the polar column, has its temperature maintained at 30° by means of the constant application of cold at the top. Free communication is maintained between the two cylinders at top and bottom; and the water in the cold cylinder being, in virtue of its low temperature, denser than the water in the warm cylinder, the two colums are therefore not in static equilibrium. The cold, and hence heavier column tends to produce an outflow of water from its bottom to the bottom of the warm column, which outflow is replaced by an inflow from the top of the warm column to the top of the cold column. In fact we have just a simple repetition of what he has given over and over again in his various memoirs on the subject. But why so repeatedly enter into the modus operandi of the matter? Who feels any difficulty in understanding how the circulation is produced?

Polar Cold considered by Dr. Carpenter the primum mobile. —It is evident that Dr. Carpenter believes that he has found in polar cold an agency the potency of which, in producing a general oceanic circulation, has been overlooked by physicists; and it is with the view of developing his ideas on this subject that he has entered so fully and so frequently into the exposition of his theory. "If I have myself done any thing," he says, "to strengthen the doctrine, it has been by showing that polar cold, rather than equatorial heat, is the *primum mobile* of this circulation "\*.

The influence of the sun in heating the waters of the intertropical seas is, in Dr. Carpenter's manner of viewing the problem, of no great importance. The efficient cause of motion he considers resides in *cold* rather than in *heat*. In fact he even goes the length of maintaining that, as a power in the production of the general interchange of equatorial and polar water, the effect of polar cold is so much superior to that of intertropical heat, that the influence of the latter may be *practically disregarded*.

"Suppose two basins of ocean-water," he says, "connected by a strait to be placed under such different climatic conditions that the surface of one is exposed to the heating influence of tropical sunshine, whilst the surface of the other is subjected to the extreme cold of the sunless polar winter. The effect of the surface-heat upon the water of the tropical basin will be for the most part limited (as I shall presently show) to its uppermost stratum, and may here be *practically disregarded*  $\dagger$ .

Dr. Carpenter's idea regarding the efficiency of cold in producing motion seems to me to be not only opposed to the generally received views on the subject, but wholly irreconcilable with the ordinary principles of mechanics. In fact there are so many points on which Dr. Carpenter's theory of a "General *Vertical* Oceanic Circulation" differs from the generally received views on the subject of circulation by means of difference of specific gravity, that I have thought it advisable to enter somewhat minutely into the consideration of the mechanics of that theory, the more so as he has so repeatedly asserted that eminent physicists agree with what he has advanced on the subject.

According to the generally received theory, the circulation is due to the *difference of density* between the sea in equatorial and polar regions. The real efficient cause is gravity; but gravity cannot act when there is no difference of specific gravity. If the sea were of equal density from the poles to the equator, gravity could exercise no influence in the production of circulation; and the influence which it does possess is in proportion to the difference of density. But the difference of density between equatorial and polar waters is in turn due not absolutely either to polar cold or to tropical heat, but to both-or, in other words, to the difference of temperature between the polar and equatorial This difference, in the very nature of things, must be as seas. much the result of equatorial heat as of polar cold. If the sea in equatorial regions were not being heated by the sun as rapidly as the sea in polar regions is being cooled, the difference of tempe-

\* Proc. Roy. Geog. Soc. January 9, 1871. † Ibid.

rature between them, and consequently the difference of density, would be diminishing, and in course of time would disappear altogether. As has already been shown, it is a necessary consequence that the water flowing from equatorial to polar regions must be compensated by an equal amount flowing from polar to equatorial regions. Now, if the water flowing from polar to equatorial regions were not being heated as rapidly as the water flowing from equatorial to polar regions is being cooled, the equatorial seas would gradually become colder and colder until no sensible difference of temperature existed between them and In fact, equality of the two rates is necessary the polar oceans. to the very existence of such a general circulation as that advocated by Dr. Carpenter. If he admits that the general interchange of equatorial and polar water advocated by him is caused by the difference of density between the water at the equator and the poles, resulting from difference of temperature, then he must admit also that this difference of density is just as much due to the heating of the equatorial water by the sun as it is to the cooling of the polar water by radiation and other means-or, in other words, that it is as much due to equatorial heat as to polar cold. And if so, it cannot be true that polar cold rather than equatorial heat is the "primum mobile" of this circulation; and far less can it be true that the heating of the equatorial water by the sun is of so little importance that it may be "practically disregarded."

Supposed influence of Heat derived from the Earth's Crust.-There is, according to Dr. Carpenter, another agent concerned in the production of the general oceanic circulation, viz. the heat derived by the bottom of the ocean from the crust of the earth (see §§ 20, 34; also Brit. Assoc. Report for 1872, p. 49, and other places). We have no reason to believe that the quantity of internal heat coming through the earth's crust is greater in one part of the globe than in another; nor have we any grounds for concluding that the bottom of intertropical seas receives more heat from the earth's crust than the bottom of those in polar regions. But if the polar seas receive as much heat from this source as the seas within the tropics, then the difference of density between the two cannot possibly be due to heat received from the earth's crust; and this being so, it is mechanically impossible that internal heat can be a cause in the production of the general oceanic circulation.

Circulation without Difference of Level.—There is another part of the theory which appears to me irreconcilable with mechanics. It is maintained that this general circulation takes place without any difference of level between the equator and the poles. Referring to the case of the two cylinders W and C, which re-

present the equatorial and polar columns respectively, Dr. Carpenter says :---

"The force which will thus lift up the entire column of water in W is that which causes the descent of the entire column in C, namely the excess of gravity constantly acting in C,—the levels of the two columns, and consequently their heights, being maintained at a *constant equality* by the free passage of surface-water from W to C."

"The whole of Mr. Croll's discussion of this question, however," he continues, "proceeds upon the assumption that the levels of the polar and equatorial columns are not kept at an equality, &c." (§ 30). And again, "Now, so far from asserting (as Captain Maury has done) that the trifling difference of level arising from inequality of temperature is adequate to the production of ocean-currents, I simply affirm that as fast as the level is disturbed by change of temperature it will be restored by gravity" (§ 23). See also to the same effect Brit. Assoc. Report, 1872, p. 50.

In order to understand more clearly how the circulation under consideration cannot take place without a difference of level, let W E (fig. 1) represent the equatorial column, and C P the

Fig. 1.



polar column. The equatorial column is warmer than the polar column because it receives more heat from the sun than the latter; and the polar is colder than the equatorial column because it receives less heat from the sun than the latter. The difference in the density of the two columns results from their difference of temperature; and the difference of temperature results in turn from the difference in the quantity of heat received from the sun by each. Or, to express the matter in other words, the difference of density (and consequently the circulation under consideration) is due to the excess of heat received from the sun by the equatorial over that received by the polar column; so that to leave out of account the superheating of the intertropical waters by the sun is to leave out of account the very thing of all others that is absolutely essential to the exist-

ence of the circulation. The water being assumed to be the same in both columns and differing only as regards temperature, and the equatorial column possessing more heat than the polar, and being therefore less dense than the latter, it follows, in order that the two columns may be in static equilibrium, that the surface of the equatorial column must stand at a higher level than that of the polar. This produces the slope WC from the equator to the pole. The extent of the slope will of course depend upon the extent of the difference of their temperatures. But, as was shown on a former occasion (Phil. Mag. for Oct. 1871), it is impossible that static equilibrium can ever be fully obtained, because the slope occasioned by the elevation of the equatorial column above the polar produces what we may be allowed to call a molecular disturbance of equilibrium. The surface of the ocean, or the molecules of water lying on the slope, are not in a position of equilibrium, but tend, in virtue of gravity, to roll down the slope in the direction of the polar column C. It will be observed that the more we gain of static equilibrium of the entire ocean the greater is the slope, and consequently the greater is the disturbance of molecular equilibrium; and, vice versa, the more molecular equilibrium is restored by the reduction of the slope, the greater is the disturbance of static equilibrium. It is therefore absolutely impossible that both conditions of equilibrium can be fulfilled at the same time so long as a difference of temperature exists between the two columns. And this conclusion holds true even though we should assume water to be a perfect fluid absolutely devoid of viscosity. It follows, therefore, that a general oceanic circulation without a difference of level is a mechanical impossibility.

In a case of actual circulation due to difference of gravity, there is always a constant disturbance of both *static* and molecular equilibrium. Column C is always higher and column W always lower than it ought to be were the two in equilibrium; but they never can be at the same level.

It is quite conceivable, of course, that the two conditions of equilibrium may be fulfilled alternately. We can conceive column C remaining stationary till the water flowing from column W has restored the level. And after the level is restored we can conceive the polar column C sinking and the equatorial column W rising till the two perfectly balance each other. Such a mode of circulation, consisting of an alternate surface-flow and vertical descent and ascent of the columns, though conceivable, is in reality impossible in nature; for there are no means by which the polar column C could be supported from sinking till the level had been restored. But Dr. Carpenter does not assume that the general oceanic circulation takes *Phil. Mag.* S. 4. Vol. 47. No. 310. Feb. 1874.

place in this intermitting manner; according to him, the circulation is constant. He asserts that there is a "continual transference of water from the bottom of C to the bottom of W, and from the top of W to the top of C, with a constant descending movement in C and a constant ascending movement in W" (§ 29). But such a condition of things is irreconcilable with the idea of "the levels of the two columns, and consequently their heights, being maintained at a constant equality" (§ 29).

Although Dr. Carpenter does not admit the existence of a permanent difference of level between the equator and the pole. he nevertheless speaks of a depression of level in the polar basin resulting from the contraction by cooling of the water flowing into it. This reduction of level induces an inflow of water from the surrounding area; "and since what is drawn away," to quote his own words, "is supplied from a vet greater distance. the continued cooling of the surface-stratum in the polar basin will cause a 'set' of waters towards it, to be propagated backwards through the whole intervening ocean in communication with it until it reaches the tropical area." The slope produced between the polar basin and the surrounding area, if sufficiently great, will enable the water in the surrounding area to flow polewards; but unless this slope extend to the equator, it will not enable the tropical waters also to flow polewards. One of two things necessarily follows : either the slope extends from the equator to the pole, or water can flow from the equator to the pole without a slope. If Dr. Carpenter maintains the former, he contradicts himself; and if he adopts the latter, he contradicts an obvious principle of mechanics.

A confusion of ideas in reference to the supposed agency of Polar Cold.—It seems to me that Dr. Carpenter has been somewhat misled by a slight confusion of ideas in reference to the supposed agency of polar cold. This is brought out forcibly in the following passage from his memoir in the 'Proceedings of the Royal Geographical Society,' vol. xv. p. 54.

"Mr. Croll, in arguing against the doctrine of a general oceanic circulation sustained by difference of temperature, and *justly maintaining* that such a circulation cannot be produced by the application of heat at the surface, has entirely ignored the agency of cold."

It is here supposed that there are two agents at work in the production of the general oceanic circulation. The one agent is *heat*, acting at the equatorial regions; and the other agent is *cold*, acting at the polar regions. It is supposed that the agency of cold is far more powerful than that of heat. In fact so trifling is the agency of equatorial heat in comparison with that of polar cold that it may be "practically disregarded"—left out of account altogether,—polar cold being the *primum mobile* of the circulation. It is supposed also that I have considered the efficiency of one of the agents, viz. heat, and found it totally inadequate to produce the circulation in question; and it is admitted also that my conclusions are perfectly correct. But then I am supposed to have left out of account the other agent, viz. polar cold, the only agent possessing real potency. Had I taken into account polar cold, it is supposed that I should have found at once a cause perfectly adequate to produce the required effect.

This is a fair statement of Dr. Carpenter's views on the subject; I am unable, at least, to attach any other meaning to his words. And I have no doubt they are also the views which have been adopted by those who have accepted his theory.

It must be sufficiently evident from what has already been stated, that the notion of there being two separate agents at work producing circulation, namely heat and cold, the one of which is assumed to have much more potency than the other, is not only opposed to the views entertained by physicists, but is also wholly irreconcilable with the ordinary principles of mechanics. But more than this, if we analyze the subject a little so as to remove some of the confusion of ideas which besets it, we shall find that these views are irreconcilable with even Dr. Carpenter's own explanation of the cause of the general oceanic circulation.

Cold and heat, considered as sensations, are very different things; but cold considered as a condition of a body means only a deficiency or absence of heat. When we say, for example, that the polar seas are colder than the equatorial, our meaning is that the polar seas possess less heat than the equatorial. And when we say that the equatorial seas are hotter than the polar, our meaning of course likewise is that the equatorial seas possess more heat than the polar. Or if we say that the equatorial seas are hot and the polar seas cold, we mean simply that both seas possess a certain amount of heat, the equatorial seas having more than the polar; or, judging them by our sensations, we call the one hot and the other cold.

How, then, according to Dr. Carpenter, does polar cold impart motion to the water? The warm water flowing in upon the polar column becomes chilled by cold, but it is not cooled below that of the water underneath; for, according to Dr. Carpenter, the ocean in polar regions is as cold and as dense underneath as at the surface. The cooled surface-water does not sink through the water underneath, like the surface-water of a pond chilled during a frosty night. "The descending motion in column C will not consist," he says, "in a successional descent of surface-films from above downwards, but it will be a downward movement of the entire mass, as if water in a tall jar were being drawn off through an orifice at the bottom" ( $\S 29$ ). There is a downward motion of the entire column, producing an outflow of water at the bottom towards the equatorial column W, which outflow is compensated by an inflow from the top of the equatorial column to the top of the polar column C. But what causes column C to descend? The cause of the descent is its excess of weight over that of column W. Column C descends and column W ascends, for the same reason that in a balance the heavy scale descends and the light scale rises. Column C descends not simply because it is cold, but because it is colder than column W. Column C descends not simply because in consequence of being cold it is dense and therefore heavy, but because in consequence of being cold it is denser and therefore heavier than column W. It might be as cold as frozen mercury and as heavy as lead; but it would not on that account descend unless it were heavier than column W. The descent of column C and ascent of column W, and consequently the general oceanic circulation, results, therefore, according to Dr. Carpenter's explanation, from the difference in the weights of the two columns; and the difference in the weights of the two columns results from their *difference* of density ; and the difference of density of the two columns in turn results from their difference of temperature. But it has already been proved that the difference of temperature between the polar and equatorial columns depends wholly on the difference in the amount of heat received by each from the sun. The equatorial column W possesses more heat than the polar column C, solely because it receives more heat from the sun than column C. Consequently Dr. Carpenter's statement that the circulation is produced by polar cold rather than by equatorial heat, is just as much in contradiction to his own theory as it is to the principles of mecha-Again, his admission that the general oceanic circulation nics. "cannot be produced by the application of heat to the surface," is virtually a giving up the whole point in debate; for according to his gravitation theory, and every form of that theory, the circulation results from *difference* of temperature between equatorial and polar seas; but this difference, as we have seen, is entirely owing to the difference in the amount of heat received from the sun at these two places. The heat received, however, is "surfaceheat;" for it is at the surface that the ocean receives all its heat from the sun; and consequently if surface-heat cannot produce the effect required, nothing else can.

M. Dubuat's *experiments*.—Referring to the experiments of M. Dubuat adduced by me to show that water would not run

down a slope of 1 in 1,820,000\*, he says, "Now the experiments of M. Dubuat had reference, not to the slow restoration of level produced by the motion of water on itself, but to the sensible movement of water flowing over solid surfaces and retarded by its friction against them" (§ 22). Dr. Carpenter's meaning, I presume, is that if the incline consist of any solid substance, water will not flow down it; but if it be made of water itself, water will flow down it. But in M. Dubuat's experiments it was only the molecules in actual contact with the solid incline that could possibly be retarded by friction against it. The molecules not in contact with the solid incline evidently rested upon an incline of water, and were at perfect liberty to roll down that incline if they chose; but they did not do so; and consequently M. Dubuat's experiment proved that water will not flow over itself on an incline of 1 in 1,000,000.

A begging of the question at issue.—" It is to be remembered," says Dr. Carpenter, "that, however small the original amount of movement may be, a momentum tending to its continuance must be generated from the instant of its commencement; so that if the initiating force be in constant action, there will be a progressive acceleration of its rate, until the increase of resistance equalizes the tendency to further acceleration. Now, if it be admitted that the propagation of the disturbance of equilibrium from one column to another is simply retarded, not prevented, by the viscosity of the liquid, I cannot see how the conclusion can be resisted, that the constantly maintained difference of gravity between the polar and equatorial columns really acts as a vis viva in maintaining a circulation between them" (§ 35).

If it be true, as Dr. Carpenter asserts, that in the case of the general oceanic circulation advocated by him "viscosity" simply retards motion, but does not prevent it, I certainly agree with him "that the constantly maintained difference of gravity between the polar and equatorial columns really acts as a vis viva in maintaining a circulation between them." But to assert that it merely retards, but does not prevent, motion, is simply begging the question at issue. It is an established principle that if the force resisting motion be greater than the force tending to produce it, then no motion can take place and no work can be performed. The experiments of M. Dubuat prove that the force of the molecular resistance of water to motion is greater than the force it is simply begging the question at issue to assert that it is less. The experiments of M.M. Barlow, Rainey, and others to which

<sup>\*</sup> The slope, however, taking Dr. Carpenter's own data, amounts only to little more than one half, viz. to 1 in 3,500,000. See Phil. Mag. for October 1871, p. 263.

he alludes, are scarcely worthy of consideration in relation to the present question, because we know nothing whatever regarding the actual amount of force producing motion of the water in these experiments, further than that it must have been enormously greater than that derived from a slope of 1 in 1,000,000.

Supposed argument from the tides.—Dr. Carpenter advances Mr. Ferrel's argument in regard to the tides. The power of the moon to disturb the earth's water, he aserts, is, according to Herschel, only one 11,400,000th part of gravity, and that of the sun not over one 25,736,400th part of gravity; yet the moon's attractive force, even when counteracted by the sun, will produce a rise of the ocean. But as the disturbance of gravity produced by difference of temperature is far greater than the above, it ought to produce circulation.

It is here supposed that the force exerted by gravity on the ocean, resulting from difference of temperature, tending to produce the general oceanic circulation, is much greater than the force exerted on the ocean by the moon in the production of the tides. But if we examine the subject we shall find that the opposite is the case. The attraction of the moon tending to lift the waters of the ocean acts directly on every molecule from the surface to the bottom ; but the force of gravity tending to produce the circulation in question acts directly on only a portion of the ocean. Gravity can exercise no direct force in impelling the underflow from the polar to the equatorial regions, nor in raising the water to the surface when it reaches the equatorial regions. Gravity can exercise no direct influence in pulling the water horizontally along the earth's surface, nor in raising it up The pull of gravity is always downwards, never to the surface. horizontally nor upwards. Gravity will tend to pull the surfacewater from the equator to the poles because here we have descent. Gravity will tend to sink the polar column because here also we have *descent*. But these are the only parts of the circuit where gravity has any tendency to produce motion. Motion in the other parts of the circuit, viz. along the bottom of the ocean from the poles to the equator and in raising the equatorial column, is produced by the pressure of the polar column; and consequently it is only *indirectly* that gravity may be said to produce motion in those parts. It is true that on certain portions of the ocean the force of gravity tending to produce motion is greater than the force of the moon's attraction, tending to produce the tides; but this portion of the ocean is of inconsiderable extent. The total force of gravity acting on the entire ocean tending to produce circulation is in reality prodigiously less than the total force of the moon tending to produce the tides.

It is no doubt a somewhat difficult problem to determine accurately the total amount of force exercised by gravity on the ocean; but for our present purpose this is not necessary. All that we require at present is a very rough estimate indeed. And this can be attained by very simple considerations. Suppose we assume the mean depth of the sea to be, say, three miles. The mean depth may yet be found to be somewhat less than this, or it may be found to be somewhat greater; a slight mistake, however, in regard to the mass of the ocean will not materially affect our conclusions. Taking the depth at 3 miles, the force or direct pull of gravity on the entire waters of the ocean tending to the production of the general circulation will not amount to more than  $\frac{1}{12,000,000,000}$  that of gravity, or only about  $\frac{1}{1053}$  that of the attraction of the moon in the production of the tides. Let it be observed that I am referring to the force or pull of gravity, and not to hydrostatic pressure.

The moon, by raising the waters of the ocean, will produce a slope of 2 feet in a quadrant; and because the raised water sinks and the level is restored, Mr. Ferrel concludes that a similar slope of 2 feet produced by difference of temperature will therefore be sufficient to produce motion and restore level. But it is overlooked that the restoration of level in the case of the tides is as truly the work of the moon as the disturbance of that level is. For the water raised by the attraction of the moon at one time is again, six hours afterwards, pulled down by the moon when the earth has turned round a quadrant.

No doubt the earth's gravity alone would in course of time restore the level; but this does not follow as a logical consequence from Mr. Ferrel's premises. If we suppose a slope to be produced in the ocean by the moon and the moon's attraction withdrawn so as to allow the water to sink to its original level, the raised side will be the heaviest and the depressed side the lightest; consequently the raised side will tend to sink and the depressed side will tend to rise, in order that the ocean may regain its static equilibrium. But when a difference of level is produced by difference of temperature, the raised side is always the lightest and the depressed side is always the heaviest; consequently the very effort which the ocean makes to maintain its equilibrium tends to prevent the level being restored. The moon produces the tides chiefly by means of a simple yielding of the entire ocean considered as a mass; whereas in the case of a general oceanic circulation the level is restored by a flow of water at or near the surface. Consequently the amount of friction and molecular resistance to be overcome in the restoration of level in the latter case is much greater than in the former. The moon, as the researches of Sir William Thomson show, will produce a tide in a globe composed of a substance where no currents or general flow of the materials could possibly take place.

Pressure as a Cause of circulation.—We shall now briefly refer to the influence of pressure (the indirect effects of gravity) in the production of the circulation under consideration. That which causes the polar column C to descend and the equatorial column W to ascend, as has repeatedly been remarked, is the difference in the weight of the two columns. The efficient cause in the production of the movement is, properly speaking, gravity; cold at the poles and heat at the equator, or, what is the same thing, the excess of heat received by the equator over that received by the poles is what maintains the difference of temperature between the two columns, and consequently is that also which maintains the difference of weight between them. In other words, difference of temperature is the cause which maintains the state of disturbed equilibrium. But the efficient cause of the circulation in question is gravity. Gravity, however, could not act without this state of disturbed equilibrium; and difference of temperature may therefore be called, in relation to the circulation, a necessary condition, while gravity may be termed the cause. Gravity sinks column C directly, but it raises column W indirectly by means of pressure. The same holds true in regard to the motion of the bottom-waters from C to W, which is likewise due to pressure. The pressure of the excess of the weight of column C over that of column W impels the bottom-water equatorwards and lifts the equatorial column. But on this point I need not at present dwell, as I have in my last paper entered into a full discussion as to how this takes place\*.

We come now to the most important part of the inquiry, viz. how is the surface-water impelled from the equator to the poles? Is pressure from behind the impelling force here as in the case of the bottom-water of the ocean? It seems to me that, in attempting to account for the surface-flow from the equator to the poles, Dr. Carpenter's theory signally fails. The force to which he appeals appears to be wholly inadequate to produce the required effect.

The experiments of M. Dubuat, as already noticed, prove that any slope which can possibly result from the difference of temperature between the equator and the poles is wholly insufficient to enable gravity to move the waters; but it does not necessarily prove that the *pressure* resulting from the raised water at the equator may not be sufficient to produce motion. This point will be better understood from the following figure, where, as

\* Phil. Mag. for October 1871.

before, PC represents the polar column and EW the equatorial column.



It will be observed that the water in that wedge-shaped portion WCW' forming the incline cannot be in a state of static equilibrium. A molecule of water at O, for example, will be pressed more in the direction of C than in the direction of W', and the amount of this excess of pressure towards C will depend upon the height of W above the line CW'. It is evident that the pressure tending to move the molecule at O towards C will be far greater than the direct pull of gravity tending to draw a molecule at O' lying on the surface of the incline towards C. The experiments of M. Dubuat prove that the direct force of gravity will not move the molecule at O'-that is, cause it to roll down the incline WC; but they do not prove that it may not yield to pressure from above, or that the pressure of the column W W' will not move the molecule at O. The pressure is caused by gravity, and cannot, of course, enable gravity to perform more work than what is derived from the energy of gravity; it will enable gravity, however, to overcome resistance, which it could not do by direct action. But whether the pressure resulting from the greater height of the water at the equator due to its higher temperature be actually sufficient to produce displacement of the water is a question which I am wholly unable to answer.

If we suppose 9 feet to be the height of the equatorial surface above the polar required to make the two columns balance each other, the actual difference of level between the two columns will certainly not be more than one half that amount, because, if a circulation exist, the weight of the polar column must always be in excess of that of the equatorial. But this excess can only be obtained at the expense of the surface-slope, as was shown at length in my last paper. The surface-slope probably will not exceed more than 4 feet or  $4\frac{1}{2}$  feet. Suppose the ocean to be of equal density from the poles to the equator, and that by some means or other the surface of the ocean at the equator is raised, say, 4 feet above that of the poles, then there can be little

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doubt that in such a case the water would soon regain its level; for the ocean at the equator being heavier than at the poles by the weight of a layer 4 feet in thickness, it would sink at the former place and rise at the latter until equilibrium was restored, producing, of course, a very slight displacement of the bottom-waters towards the poles. It will be observed, however, that restoration of level in this case takes place by a simple yielding, as it were, of the entire mass of the ocean without displacement of the molecules of the water over each other to any great extent. In the case of a slope produced by difference of temperature, however, the raised portion of the ocean is not heavier but lighter than the depressed portion, and consequently has no tendency to Any movement which the ocean as a mass makes in order sink. to regain equilibrium tends, as we have seen, rather to increase the difference of level than to reduce it. Restoration of level can only be produced by the forces which are in operation in the wedge-shaped mass W C W', constituting the slope itself. But it will be observed by a glance at the figure that, in order to the restoration of level, a large portion of the water WW' at the equator will require to flow to C, the pole.

According to the general vertical oceanic circulation theory, pressure from behind is not one of the forces employed in the production of the flow from the equator to the poles. This is evident; for there can be no pressure from behind acting on the water if there be no slope existing between the equator and the poles. Dr. Carpenter not only denies the actual existence of a slope, but denies the necessity for its existence. But to deny the existence of a slope is to deny the existence of pressure, and to deny the necessity for a slope is to deny the necessity for pressure. That in Dr. Carpenter's theory the surface-water is supposed to be *drawn* from the equator to the poles, and not *pressed* forward by a force from behind, is further evident from the fact that he maintains that the force employed is not vis a *tergo* but vis a fronte (Proc. Roy. Geog. Soc. Jan. 9, 1871, § 29).

[To be continued.]

XV. On Quartz, Ice, and Karstenite. By W. H. MILLER, M.A., F.R.S., Professor of Mineralogy in the University of Cambridge\*.

Quartz.

A MONG the minerals presented to the University by H. W. Elphinstone, Esq., are two crystals of quartz associated with chlorite, apparently from the same, but unknown, locality.

\* Communicated by the Author.

## THE

## LONDON, EDINBURGH, AND DUBLIN

# PHILOSOPHICAL MAGAZINE AND

# JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1874.

## XXI. On the Electric Resistance of Selenium. By the EARL OF ROSSE, D.C.L., F.R.S. &c.\*

THE recently discovered fact of the diminution of the electric resistance of selenium in the crystalline state when exposed to the action of light or, possibly, of radiant heat, is one which naturally excites some interest beyond that arising from the curious and unexpected nature of the phenomenon considered by itself; for the possibility of selenium being applied to the measurement of light or radiant heat invests the discovery with a very general importance.

Mr. Willoughby Smith seems to have satisfied himself that light, not heat, is the active agent; but I have spoken of the latter as possibly the cause of the effect observed, as Lieutenant Sale's paper in the Proceedings of the Royal Society (although it is therein stated that selenium is affected by *light*, and again, that the change of resistance is not due to an alteration of temperature) might lead one to infer that the observed effect was due to radiant heat, not to light; for he says that the *actinic* rays produce no effect, but that it is at a maximum in the red rays, or beyond them, near the maximum of the heat-rays; and inasmuch as he appears not to have determined by means of the thermopile the relative calorific power of the various rays of his spectrum, nor even to have reduced his results to what they would have been if the normal or diffraction spectrum had been employed, the experiment is inconclusive as to the comparative

\* Communicated by the Author. Phil. Mag. S. 4. Vol. 47. No. 311. March 1874.

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sensibility of selenium to the various rays of the solar spectrum.

As it was the opinion of one or two friends of much experience in kindred questions in physical science that heat, not light, is the active agent, and that selenium might with advantage replace the thermopile in the measurement of radiant heat, I procured the necessary apparatus for a preliminary trial.

Although my experiments are hardly sufficient to justify a decided opinion in favour of or against its applicability to photometric purposes, I think that they remove all hope of selenium replacing the thermopile in the measurement of heat of low refrangibility.

I exposed a rod of selenium during a sufficient interval to obtain the maximum effect from the light of a candle  $3\frac{1}{2}$  inches distant. The diminution of resistance produced amounted to 24.3 per cent. A second trial gave a diminution of 24.2 per cent., the candle being at 4 inches distance. A vessel of hot water of about 9 or 10 inches diameter and 9 inches height produced no perceptible change when it stood for several minutes with its centre at 13 inches distance, nor even when it was brought 6 inches nearer. On the other hand, by means of the thermopile a deviation to the extent of 136 divisions of the scale of a Thomson's galvanometer was produced by the radiation from the vessel of hot water whose centre was at 13 inches distance; and when it was replaced by the candle at 4 inches distance, the deviation was increased to only 315 divisions. Thus it was shown that the two instruments were not comparable as measurers of the radiation from the two sources of heat.

The change of resistance produced by exposure to the radiation from a candle when a sheet of glass was alternately interposed and removed was next measured. The effect due to absorption by the glass appeared to be small, certainly not much more, possibly even less than the average absorption of light by glass. Perhaps 90 per cent. was transmitted. Owing to the inconstancy of the light, much accuracy was not readily obtainable in this experiment. The piece of glass had been shown by the thermopile to transmit 80 per cent. of the solar rays, and under 1 per cent. of the radiant heat from a blackened tin vessel of hot water. It was now by the same means ascertained that 48 per cent. of the radiant heat from a candle was transmitted by it.

A glass cell filled with a solution of alum was now taken; and it was found that, while the exposure of the selenium bar to the radiation from a naked candle at  $3\frac{1}{4}$  inches distance produced a diminution of resistance amounting to 9.4 per cent., when the alum solution was interposed the decrease was still 8.95 per cent.; hence the quantity transmitted by the glass cell and the alum solution it contained was as much as 95.2 per cent., as measured by the rod of selenium.

The same, measured by the thermopile, was found to be 7.06 per cent.

Having satisfied myself as to the comparative, if not the absolute insensibility of a bar of selenium to radiant heat of low refrangibility, and therefore as to its being unsuitable for replacing the thermopile in the measurement of those rays, I made a few experiments with the view of finding whether it was likely to be a suitable instrument in photometry.

To obtain a more constant light I substituted a paraffin lamp for the candle; and by placing in front of the horizontal selenium bar a vertical slit whose breadth could be varied at pleasure, and which could be closed before and during every alternate measurement of the resistance, also by varying the distance of the lamp, it was found that while the decrease of resistance varied as the breadth of the slit, and therefore as the length of the portion of the bar exposed to light, it was far more nearly proportional to the reciprocal of the distance, and therefore to the square root of the intensity of the incident light than to the intensity simply, between the limits within which the observations were made. The decrease of resistance observed was :—

Distance from to centre of	De	Decrease.			
inches.				pe	er cent.
$2\frac{1}{2}$					38
5					27
7					22
9			200		21
$13\frac{1}{2}$					15
19	1 mil				11
$22\frac{1}{2}$					10
44 <u>1</u>					3.77
75					1.57

The length of the bar was  $2\frac{1}{4}$  inches.

No experiments were made to ascertain how far the sensibility was affected by the temperature of the room; but the presence of moisture rendered the action feeble and uncertain as the dewpoint was approached, probably owing to the deposit of a very slight film of moisture on the surface of the bar. Another effect, probably to be ascribed to the same cause, was noticed on two occasions—a diminution of the change of resistance after a certain duration of exposure to a screening from the light.

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			terminations.
	(1 min.)	e	(.663 : 1.0, mean of two de-
Die	2 ,,	after interpo-	·802:1.0 ,,
Ratio of <	3 ,,	> sing and the $<$	·817:1·0 "
resistances	4 ,,	removal of	•854 : 1.0, mean of three.
	5 ,, )	the screen,	·868:1.0 "

In a drier atmosphere the maximum, which was still as rapidly attained, was retained with tolerable constancy. Probably the diminution of the effect after the maximum had been reached may have been due to the slower dissipation of the film of moisture, and to its re-formation under the alternations of temperature which accompanied the alternations of light and darkness.

I regret that I shall not have leisure to pursue this subject for some time, and am therefore obliged to leave off these experiments in their present incomplete state.

January 1874.

# XXII. Note on the Composition of certain Mine Waters. By J. ARTHUR PHILLIPS, M.Inst.C.E., F.G.S., F.C.S., &c.\*

SINCE the publication of a paper in which I attempted to show that the waters of the thermal spring at Huel Seton Mine are probably derived from the sea<sup>†</sup>, two other waters from deep Cornish mines have been analyzed in my laboratory.

The first of these was from the 212-fathom level at the Phœnix Mines near Liskeard, where it issues from the lode at a temperature of  $65^{\circ}$  F. This mine is in granite, and at one time produced large quantities of copper ores, but has for the last ten years been principally worked for tin, which is obtained from the same veins which were formerly wrought for copper. A large portion of the vein above the point from which the water for analysis was collected had been removed some years previously.

The following results, in grammes per litre and grains per gallon. were obtained by analysis.

\* Communicated by the Author.

† Philosophical Magazine, July 1873

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## of certain Mine Waters.

# Water from the Phœnix Mines. Solid matter ·2130 gramme per litre, or 14·91 grains per gallon. Sp. gr. =1·0002. Analytical Results.

Film and a state	Gramme	per litre.	Grains per gallon.		
	I.	II.	I.	II.	
Carbonic acid	.0706	·0710	4.94	4.97	
Sulphuric acid	·0438	·0430	3.07	3.01	
Silica	·0304	·0310	2.13	2.17	
Chlorine	.0234	.0226	1.64	1.58	
Copper	.0008	.0008	.06	•06	
Manganous oxide	.0005	.0005	·03	•03	
Ferrous oxide	.0042	•0046	•29	•32	
Lime	.0142	·0146	.99	1.02	
Alkaline chlorides	.1642	.1618	11.49	11.33	
Potassa	.0129	·0125	•90	.87	
Soda	.0549	.0544	3.84	3.81	
Ammonia	•0001	.0001	.01	•01	
Nitric acid	·0016	.0016	.11	·11	

The foregoing results may be thus tabulated :---

	Gramme	per litre.	Grains per gallon.		
	I.	II.	I.	II.	
Calcium carbonate Ferrous carbonate Manganous carbonate Calcium sulphate Sodium sulphate Sodium chloride Cupric chloride Potassium silicate (K <sup>2</sup> SiO <sup>3</sup> ). Sodium silicate (Na <sup>2</sup> SiO <sup>3</sup> ) .	·0232 ·0068 ·0008 ·0029 ·0748 ·0367 ·0017 ·0003 ·0256 ·0415 ·0025	·0224 ·0074 ·0008 ·0051 ·0710 ·0356 ·0017 ·0003 ·0246 ·0447 ·0025	$     \begin{array}{r}       1 \cdot 62 \\       \cdot 48 \\       \cdot 06 \\       \cdot 20 \\       5 \cdot 24 \\       2 57 \\       \cdot 12 \\       \cdot 02 \\       1 \cdot 79 \\       2 \cdot 90 \\       \cdot 17 \\     \end{array} $	$ \begin{array}{r} 1.57 \\ .52 \\ .06 \\ .36 \\ 4.97 \\ 2.49 \\ .12 \\ .02 \\ 1.72 \\ 3.13 \\ .17 \\ \end{array} $	
Total by addition         Total found directly         Excess of carbonic acid	·2168 ·2130 ·0575	·2161 ·0580	15·17 14·91 4·03	15·13 4·07	

The second water examined was collected at the 302-fathom level at Dolcoath near Camborne; this mine also formerly produced large quantities of copper ores. These have in depth gradually given place to cassiterite, and Dolcoath is at the present time the most productive tin mine in the United Kingdom. The water analyzed was collected from the roof of a short crosscut, in granite, 25 fathoms east of the engine shaft and 15 feet south of the main lode. The water issued in considerable quantities,

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at a temperature of 92° F.; and a large portion of the upper parts of the vein had been removed by stoping.

The following results, in gramme per litre and grains per gallon, were obtained by analysis :---

#### Water from Dolcoath.

# Solid matter $\cdot 6710$ gramme per litre, or $46 \cdot 97$ grains per gallon. Sp. gr. = $1 \cdot 0007$ .

Gramme per litre. Grains per gallon. I. II. I. II. 4.12 Carbonic acid..... ·0588 ·0578 4.05 Sulphuric acid ..... ·1304 ·1294 9.13 9.06 Silica ..... ·0295 .03022.06 2.11 ·1906 ·1906 13.34 13.34 Chlorine ..... Arsenic ..... trace trace trace trace Alumina ..... ·0004 ·0004 .03 .03 .31 Ferric oxide ..... ·0044 ·0044 .31 .04 Manganous oxide ..... ·0006 .04 ·0006 .0007 .05 ·0007 .05 Copper ..... 7.28 7.20 ·1040 ·1028 Lime ..... Magnesia ..... trace trace trace trace 29.34 Alkaline chlorides ..... ·4192 ·4212 29.48Potassium ..... 1.92 .0275  $\cdot 0276$ 1.93 Sodium ..... ·1442 ·1449 10.09 10.14 Lithium ..... trace trace trace trace ·0003 ·0003 .02 .02 Ammonia ..... Nitric acid ..... ·0017 .12 .12 ·0017

Analytical Results.

#### The foregoing results may be thus tabulated :--

	Gramme	per litre.	Grains per gallon.		
	I.	II.	I.	II.	
Calcium carbonate	·0554	·0550	3.88	3.85	
Ferrous carbonate	·0064	·0064	.45	.45	
Manganous carbonate	·0010	·0010	.07	.07	
Ferric arsenate	trace	trace	trace	trace	
Calcium sulphate	·1773	·1749	12.41	12.24	
Magnesium sulphate	trace	trace	trace	trace	
Aluminium sulphate (3SO <sup>4</sup> Al <sup>2</sup> )	-0013	·0013	•09	0·9	
Sodium sulphate	-0448	·0454	3·14	3·18	
Sodium chloride	-3118	·3118	21·83	21·83	
Cupric chloride	-0015	·0015	•10	·10	
Lithium chloride	trace	trace	trace	trace	
Potassium silicate (K <sup>2</sup> SiO <sup>3</sup> )	•0543	•0545	3·80	3.81	
Sodium silicate (Na <sup>2</sup> SiO <sup>3</sup> )	•0171	•0183	1·20	1.28	
Sodium nitrate	•0027	•0027	·19	.19	
Ammonium chloride	•0009	•0009	·06	.06	
Total by addition Total found directly Excess of carbonic acid	·6745 ·6710 ·0316	·6737 ·0308	47·22 46·97 2·21	47·15 2·16	

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#### of certain Mine Waters.

The water issuing from the back of the crosscut at Dolcoath deposits in considerable quantities a soft reddish-grey precipitate which frequently assumes stalactitic forms. Similar incrustations, although generally less abundant, are found in nearly all deep mines wherever water issues from the vicinity of a vein and flows over the surface of the adjacent rock.

Three analyses made of air-dried specimens of this substance afforded the following results :---

and an anith that the is	I.	II.	III.
Ferric oxide Manganic oxide Arsenic acid Arsenious acid Sulphuric acid Water { combined hygroscopic	36·30 trace 32·47 trace 2·65 12·77 15·90	36.29 trace 32.52 trace 2.51 12.52 15.98	37.75 trace 32.55 .68 2.52 11.45 15.20
and and the Meddene	100.09	99.82	100.15

Nos. I. and II. are duplicate analyses of the same specimen, made in my laboratory; No. III. is an analysis of another specimen, made, at my request, by my friend Mr. Dugald Campbell.

It may appear somewhat remarkable that a plentiful deposit of arsenate of iron should be formed from water in which only traces of arsenic could be detected, and in which the amount of iron present is so inconsiderable. On examining, however, the jars in which the water had been collected for analysis, a small amount of a flocculent brownish deposit was found at the bottom of each; this, on being thrown on a filter and subsequently analyzed, was found to contain 31.52 per cent. of arsenic anhydride and 25.27 per cent. of ferric oxide. The ferric arsenate, together with a little clay, had been deposited before the clear water was siphoned off for analysis; this accounts for the small amounts of iron and arsenic retained in solution.

These waters differ materially from those of the lithium spring at Huel Seton, which contain above a thousand grains of solid matter per gallon, while those from the Phœnix Mines and from Dolcoath contain respectively but 14.91 and 46.97 grains per gallon. The Huel Seton water is believed to be derived from the sea by percolation through a fault; the waters, of which analyses are now given, are probably the result of the infiltration of surface-water through the workings of the mines, and through fissures in the respective veins. T

## XXIII. On Ocean-currents.—Part III. On the Physical Cause of Ocean-currents. By JAMES CROLL, of the Geological Survey of Scotland.

#### [With a Plate.]

[Concluded from p. 122.]

## The Gibraltar Current.

N my last paper I proved that it was only the water lying above the level of the submarine ridge which crosses the Strait of Gibraltar that could exercise any influence in producing circulation between the Atlantic and the Mediterranean\*. The water of the Atlantic below the level of this ridge might be as light as air, and that of the Mediterranean as heavy as molten lead; but this could produce no disturbance of equilibrium. It is only the waters lying above the level of this ridge in the two seas that require to balance each other; and if there is no difference of density between the Atlantic and the Mediterranean waters from the surface down to the level of the top of the ridge, then there is nothing that can produce the circulation which Dr. Carpenter supposes. This submarine ridge comes up to within very nearly 100 fathoms of the surface; and according to Dr. Carpenter's own admission, little or no difference of density exists down to that depth; it follows therefore that there is nothing to produce disturbance of equilibrium, or any such circulation as that which he infers. It is true that in his last expedition he found the bottom-water on the ridge somewhat denser than Atlantic water at the same depth, the former being 1.0292 and the latter 1.0265; but it also proved to be denser than Mediterranean water at the same depth. He found, for example, that "the dense Mediterranean water lies about 100 fathoms nearer the surface over a 300-fathoms bottom, than it does where the bottom sinks to more than 500 fathoms" ( $\S$  51). But any excess of density which might exist at the ridge could have no tendency whatever to make the Mediterranean column preponderate over the Atlantic column, any more than could a weight placed over the fulcrum of a balance have a tendency to make the one scale weigh down the other.

Although Dr. Carpenter has done me the honour to discuss nearly all the objections which I have advanced against his theory, he nevertheless makes no reference whatever to this objection; and this is the more singular, seeing that the expedition, of which his memoir is a report, was chiefly if not solely undertaken for the purpose of establishing the correctness of his theory of the Gibraltar current. If, therefore, there was any one objection advanced by me which he might have been expected to

\* Phil. Mag., October 1871, pp. 269-272.

† Proc. Roy. Geog. Soc. Jan. 9, 1871, § 13.

discuss, it was surely that relating to the Gibraltar current; for if the objection referred to be sound, it shows the mechanical impossibility of his theory. It proves that whether there be an under current or not, or whether the dense water lying in the deep trough of the Mediterranean be carried over the submarine ridge into the Atlantic or not, the explanation offered by Dr. Carpenter is one which cannot be admitted. It is incumbent on him to explain either (1) how the almost infinitesimal difference of density which exists between the Atlantic and Mediterranean columns down to the level of the ridge can produce the upper and under currents carrying the deep and dense water of the Mediterranean over the ridge, or (2) how all this can be done by means of the difference of density which exists below the level of the ridge.

We shall now pass to the consideration of Dr. Carpenter's objections to my conclusions regarding the influence of the Gulfstream on climate.

# Dr. Carpenter's Objections to my estimate of the Thermal power of the Gulf-stream.

"The doctrine of the extension of the Gulf-stream proper to the polar area, carrying with it a vast amount of equatorial heat, has been advocated with great ability by Mr. James Croll; who, employing the modern method of computing *units of heat*, essays to prove that the quantity of heat carried from the equatorial area by the Gulf-stream is so enormous, as to be competent not only to do all that Dr. Petermann attributes to it, but a great deal more" (§ 99).

"Without attempting to follow Mr. Croll through his calculations, I may stop to point out what appear to me to be the fallacies of his method; since if this can be proved erroneous, Mr. Croll's great array of figures is utterly valueless" (§ 100).

Now, in order to show that my "array of figures is utterly valueless," it is necessary to prove *either that I have overestimated the amount of heat conveyed by the Gulf-stream, or the effects produced by that heat.* This is evident; for if I am correct both as to the amount of heat conveyed and the effects produced by that heat, the figures must possess all the value which I claim for them.

We shall now consider how Dr. Carpenter manages to render my estimate of the heat conveyed by the stream "utterly valueless."

"In the *first* place, in Mr. Croll's preliminary comparison of the temperatures of the northern and southern hemispheres, he altogether ignores the influence on the distribution of heat over the globe which is exerted by the great relative preponde-

rance of land in the northern hemisphere ..... And to affirm, as Mr. Croll does, that the lower mean temperature of the southern hemisphere is due to the amount of heat transferred over from that hemisphere to the northern by ocean-currents, is to repudiate all that has been established by the researches of meteorologists, as to the relative effects of land and sea," &c. (§ 101).

But what has all this to do with my estimate of the quantity of heat conveyed by the Gulf-stream? Supposing it to be true that I "altogether ignore the influence on the distribution of heat over the globe which is exerted by the great relative preponderance of land in the northern hemisphere," and supposing it to be true that the lower mean temperature of the southern hemisphere is not due, as I have concluded, to the amount of heat transferred over from that hemisphere to the northern by ocean-currents, this cannot in any way affect the value of my figures regarding the amount of heat conveyed by the Gulfstream. It is not true, however, that I ignore and repudiate all that has been established as to the effect of land and sea in producing the difference of mean temperature between the two hemispheres. So far from this being the case, I have devoted an entire paper (Phil. Mag. Sept. 1869) to an examination of the arguments which have been advanced to explain the lower *mean* temperature of the southern hemisphere. I have given my reasons for concluding that an enormous amount of heat is transferred from the southern hemisphere to the northern by means of currents. These reasons may or may not be satisfactory; but nevertheless they are reasons, not assumptions. It would be needless as well as out of place to repeat these arguments; but I may be permitted simply to refer to one of them, viz. my reason for concluding that a great portion of the heat possessed by the Gulf-stream is derived from the southern hemisphere. If all that heat came from the northern hemisphere, it could only come from that portion of the Atlantic, Caribbean Sea, and Gulf of Mexico which lies to the north of the equator. The entire area of these seas, extending to the tropic of Cancer, is about 7,700,000 square miles. Were the heat which passes through the Straits of Florida derived exclusively from this area, the following Table would then represent the relative quantity 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 \* See Phil. Mag. for October 1870, p. 258.

If a very large proportion of the heat possessed by the Gulfstream be not derived from the southern hemisphere, these figures show that the Atlantic, from the equator to the tropic of Cancer, should be as cold as from the tropic of Cancer to the North Pole. But independently of this, a mere glance at a chart of ocean-currents will show that the Gulf-stream is chiefly fed by a current from the southern hemisphere. Without such a transference of heat it would be impossible to account for the N. Atlantic being actually 5 degrees warmer than the S. Atlantic.

Again, Dr. Carpenter remarks :--- "In computing the heat imparted by the sun to the equatorial area from which the Gulfstream is fed, Mr. Croll assumes that the heat, being wholly taken up by the water of the ocean, is transferred by its currents towards the polar regions; whilst of the heat which falls upon the land, a very large proportion is lost by radiation, passing off into the stellar spaces" (§ 102).

But this cannot in any way affect the correctness of the result of my computation of the amount of heat conveyed by the Gulfstream. What I have maintained in my papers is, that the quantity of heat conveyed by the winds from intertropical land is trifling to that which is conveyed by currents from intertropical seas. Dr. Carpenter says that "the heat lost by evaporation from the sea must be far greater than that lost by radiation from the land." According to the laws of radiation and absorption, all the heat received from the sun by the land must be either reflected or radiated from its surface, with the exception of the small portion which is communicated to the air in *contact* with that surface. In fact it is by radiation that the sea as well as the land loses the greater part of its heat—the only difference in the two cases being, that heat radiated from the sea is absorbed more readily by the air than heat radiated from the land, and consequently produces a greater influence on climate.

Dr. Carpenter continues :---" Mr. Croll leaves almost entirely out of the question the N.E. transportation of an enormous amount of heat from the general surface of the Atlantic by the agency of the aqueous vapour thus raised; although the importance of this agency has been insisted on by the most eminent authorities in meteorology" (§ 103).

Here again, however, my estimate of the heat conveyed by the Gulf-stream, or the effects which it produces, cannot possibly be affected by the above consideration. It seems to be forgotten in this objection, that, were it not for the Gulf-stream, the quantity of heat which could possibly be derived from the Atlantic would be so much the less by an amount equal to that conveyed by the stream. Besides all this, there may be other

sources of heat than those noticed by me; but the omission does not diminish the importance of those to which I have referred. Had I been writing a treatise on meteorology, I should no doubt have referred as well to the influence of aqueous vapour as to many other sources of heat which I have purposely omitted in my paper on Ocean-currents as being foreign to my inquiry.

Dr. Carpenter objects to my statement that "the greater part of the moisture received at the equator is condensed and falls as rain in those regions," and refers me to the case of the Red Sea, where, although evaporation is excessive, almost no rain falls. But is it not an established fact, that the greater part of the water evaporated in intertropical regions does actually fall as rain in those regions? The reason why the vapour raised from the Red Sea does not fall in that region as rain, is no doubt owing to the fact that this sea is only a narrow strip of water in a dry and parched land, the air overhead being too greedy of moisture to admit of the vapour being deposited as rain. But over a wide expanse of ocean, where the air above is kept to a great extent in a constant state of saturation, the case is totally different.

I continue my quotation :—" Until corrected by Mr. Findlay, Mr. Croll assumed that the whole of the true Gulf-stream continues to flow in a N.E. direction; whereas it is unquestionable that a considerable proportion of it (probably more than one half) turns southwards to the east of the Azores, and reenters the equatorial current" (§ 104).

I am not aware of having advanced any thing which could lead Dr. Carpenter or any one else to suppose that I was of opinion that the whole of the Gulf-stream flows in a N.E. direction, or that I was ignorant of the existence of the S.E. branch. Nor do I remember having seen the correction by Mr. Findlay to which he refers. To suppose that I knew of the existence of the N.E. but not of the S.E. branch, is to assume that I had never seen a chart of the Gulf-stream. If I had seen a chart, how could I possibly have observed the N.E. branch without at the same time perceiving the other? It would be just as possible to look one's friend in the face and notice his left eye without seeing his right.

In reference to these four reasons or arguments designed to show that my figures are valueless, there must be some confusion of ideas. The point to be proved is, that by some wrong method I have been led to form either an erroneous estimate of the quantity of heat conveyed by the Gulf-stream, or the effects resulting from that heat. This is what Dr. Carpenter proposes to do; but no sooner does he make this proposal than he com-

mences to prove something totally different, viz. that there are a great many important causes affecting climate which I entirely ignore or overlook, and that those causes which I ignore or overlook have a far greater influence on climate than the heat of the Gulf-stream. It may be perfectly true that there are a great many important causes affecting climate which I have not considered; and it may likewise be true that those causes, left out of consideration, have a far more powerful influence on climate than the heat conveyed by the Gulf-stream; nevertheless it may be true also that all my statements regarding the influence of the Gulf-stream on climate are perfectly correct. It does not necessarily follow that a horse may not possess a certain amount of strength, and be able to perform a certain amount of work, simply because there are other horses which possess a much greater amount of strength, and can perform a much greater amount of work.

What I have endeavoured to prove in reference to the Gulfstream is :—that the amount of heat conveyed by it is so enormous as to be equal to one fourth of all the heat received from the sun by the North Atlantic in temperate regions; and that were it not for the Gulf-stream and other ocean-currents, only a very small portion of the globe would be suited to the present orders of sentient beings \*—that London, instead of possessing a mean annual temperature of nearly 50°, would have a mean temperature of not over 10°. But I never argued that there were not other causes to which we are far more indebted than to the heat of the Gulf-stream. Were it not for those other causes, the temperature of London would not be simply 40°, but upwards of 500° below what it is at present.

#### The bearing which my estimate has on Dr. Carpenter's Theory.

There is one point to which I wish to direct special attention, viz. the bearing which my conclusions regarding the quantity of heat conveyed by the Gulf-stream have on Dr. Carpenter's theory of a general interchange of equatorial and polar water. But, in order better to understand this matter, it will be necessary to refer very briefly to a point which has already been discussed at considerable length in former papers. In my earlier paper on the amount of heat conveyed by the Gulf-stream †, I estimated the volume of that stream as *equal to that* of a current 50 miles broad and 1000 feet deep, flowing (from the surface to the bottom) at 4 miles an hour. Of course I did not mean, as Dr. Carpenter seems to suppose, that the stream at any par-

\* Phil. Mag. for Feb. 1870.

† Trans. of Geol. Soc. of Glasgow for April 1867; Phil. Mag. for June 1867 (Supplement).

ticular place is 50 miles broad and 1000 feet deep, or that it actually flows at the uniform rate of 4 miles an hour at surface and bottom. All I meant was, that the Gulf-stream is equal to that of a current of the above size and velocity. But in my recent papers on Ocean-currents I have taken the volume of the stream at one half this estimate, viz. equal to a current 50 miles broad and 1000 feet deep flowing at the rate of 2 miles an hour. I have estimated the mean temperature of the stream as it passes the Straits of Florida to be 65°, and have supposed that the water in its course becomes ultimately cooled down on an average to 40° \*. In this case each pound of water conveys 25 units of heat from the Gulf of Mexico, to be employed in warming temperate and polar regions. Assuming these data to be correct, it follows that the amount of heat transferred from the Gulf of Mexico by this stream per day amounts to 77,479,650,000,000,000,000 foot-pounds. This enormous quantity of heat is equal to one fourth of all that is received from the sun by the whole of the Atlantic Ocean from the tropic of Cancer up to the Arctic Circle.

This is the amount of heat conveyed from intertropical to temperate and polar regions by the Gulf-stream. What now is the amount conveyed by means of the general oceanic circulation? If this general interchange of equatorial and polar water be, as Dr. Carpenter supposes, the great agency employed in distributing heat over the globe, then surely it is not too much to expect that the quantity of intertropical heat carried into the North Atlantic and Arctic seas must be at least equal to that carried by the Gulf-stream.

If we assume this to be the case, then the combined amount of heat conveyed by the two agencies into the Atlantic from intertropical regions will of course be equal to twice that conveyed by the Gulf-stream alone. Taking the annual quantity of heat received from the sun per unit surface at the equator at 1000, the quantities received by the three zones will be respectively as follows :---

Equator		 0.0	1000
Torrid zone			975
Temperate zone			757
Frigid zone	3.3		454

\* It is probable that a large proportion of the water constituting the south-eastern branch of the Gulf-stream is never cooled down to  $40^{\circ}$ ; but, on the other hand, the north-eastern branch, which passes into the Arctic regions, will be cooled far below  $40^{\circ}$ , probably below  $30^{\circ}$ . Hence I cannot be overestimating the extent to which the water of the Gulf-stream is cooled down in fixing upon  $40^{\circ}$  as the average minimum temperature.

Now it will be seen, by referring to what has been shown on a former occasion (Phil. Mag. for Oct. 1870, p. 257), that the Gulf-stream and general oceanic circulation would, in such a case, remove from the torrid zone 405 parts of the 975 received from the sun; and this transferred to the Atlantic in temperate regions, would add 367 to the 757 already possessed by it. In this case the Atlantic in temperate regions would possess 1126 parts of heat, whereas the intertropical region would possess only 570 parts; or, in other words, the Atlantic in temperate regions would have twice the amount of heat possessed by it in intertropical regions. But if we assume that one half of this heat goes into the Arctic Ocean, and the other half remains in the temperate regions, the relative quantities of heat possessed by the three zones will be as follows:—

Atlantic,	in	Torrid zone .		570
,,	in	Temperate zone		940
29	in	Frigid zone .		766

It is here assumed, however, that none of the heat possessed by the Gulf-stream is derived from the southern hemisphere, which, we know, is not the case. But supposing that as much as one half of the heat possessed by the stream came from the southern hemisphere, and that the other half was obtained from the seas lying between the equator and the tropic of Cancer, the relative proportions of heat possessed by the three zones per given area would be as follows :—

Atlantic,	in	Torrid zone .			671
"	in	Temperate zone			940
"	in	Frigid zone .	1.1		766

This proves incontestably that, supposing there is such a circulation as Dr. Carpenter maintains, the quantity of heat conveyed by means of it into the North Atlantic and Arctic Oceans must be trifling in comparison with that conveyed by the Gulfstream; for if it nearly equalled that conveyed by the Gulfstream, then not only the North Atlantic in temperate regions; but even the Arctic Ocean itself would be much warmer than the intertropical seas. In fact, so far as the distribution of heat over the globe is concerned, it is a matter of indifference whether there really is or is not such a thing as this general oceanic circulation. The enormous amount of heat conveyed by the Gulf-stream alone puts it beyond all doubt that oceancurrents are the great agents employed in distributing over the globe the excess of heat received by the sea in intertropical regions.

It is therefore, so far as concerns the theory of a General Oceanic Circulation, of the utmost importance that the advo-

cates of that theory should prove that I have overestimated the thermal power of the Gulf-stream. This, however, can only be done by detecting some error either in my computation or in the data on which it is based; yet neither Dr. Carpenter nor any one else, as far as I know, has challenged the accuracy of my figures. The question at issue is the correctness of the data; but the only part of the data which can possibly admit of being questioned is my estimate of the volume and temperature of the stream. Dr. Carpenter, however, does not maintain that I have overestimated the temperature of the stream; on the contrary, he affirms that I have really underestimated it. "If we assume," remarks Dr. Carpenter, "the limit of the stratum above 60° as that of the real Gulf-stream current, we shall find its average temperature to be somewhat higher than it has been stated by Mr. Croll, who seems to have taken 65° as the average of the water flowing through the entire channel. The average surface-temperature of the Florida channel for the whole year is 80°; and we may fairly set the average of the entire outgoing stream, down to the plane of 60°, at 70°, instead of  $65^{\circ}$  as estimated by Mr. Croll" (§ 141). It follows, then, that every pound of water of the Gulf-stream actually conveys 5 units of heat more than I have estimated it to do-the amount conveyed being 30 units instead of 25 units as estimated by me. Consequently, if the Gulf-stream be equal to that of a current of merely  $4l\frac{1}{2}$  miles broad and 1000 feet deep, flowing at the rate of 2 miles an hour, it will still convey the estimated quantity of heat. But this estimate of the volume of the stream, let it be observed, scarcely exceeds one third of that given by Herschel, Maury, and Colding (Phil. Mag. Oct. 1871, p. 272), and little more than one half that assigned to it by Mr. Laughton, and but very little exceeds that given by Mr. Findlay \*, an author whom few will consider likely to overrate either the volume or heating-power of the stream.

The important results obtained during the 'Challenger' expedition have clearly proved that I have neither overestimated the temperature nor the volume of the Gulf-stream. Between Bermuda and Sandy Hook the stream is 60 miles broad and 600 feet deep, with a maximum velocity of from  $3\frac{1}{2}$  to 4 miles an hour. If the mean velocity of the entire section amounts to  $2\frac{1}{4}$  miles an hour, which it probably does, the volume of the stream must equal that given in my estimate<sup>†</sup>. But we have no

\* Mr. Findlay considers that the daily discharge does not exceed 333 cubic miles (Brit. Assoc. Rep. 1869, p. 160). My estimate makes it 378 cubic miles. Mr. Laughton's estimate is 630 cubic miles (Paper "On Ocean-currents," Journ. of Royal United-Service Institution, vol. xv.).

† Dr. Carpenter states (§ 140) that 48 miles per day is the mean

evidence that all the water flowing through the Straits of Florida passes through the section examined by the officers of the 'Challenger.' Be this, however, as it may, the observations made between St. Thomas and Sandy Hook reveal the existence of an immense flow of warm water, 2300 feet deep, entirely distinct from the water included in the above section of the Gulf-stream proper. As the thickest portion of this immense body of water joins the warm water of the Gulf-stream, Captain Nares considers that "it is evidently connected with it, and probably as an offshoot." At Sandy Hook, according to him, it extends 1200 feet deeper than the Gulf-stream itself, but off Charleston, 600 miles nearer the source, the same temperature is found at the same depth. But whether it be an offshoot of the Gulfstream or not, one thing is certain, it can only come from the Gulf of Mexico or from the Caribbean Sea; and that it is a moving stream is proved by the fact that at some places its velocity was found to be as great as 18 miles per day. This mass of water, after flowing northwards for about 1000 miles, turns to the right and crosses the Atlantic in the direction of the Azores, where it appears to thin out.

If, therefore, we take into account the combined heat conveyed by both streams, my estimate of the heat transferred from intertropical regions into the North Atlantic will be found rather under the truth than above it. The quantity of heat thus borne into the Atlantic is enormous compared with that which can possibly be conveyed by a "General Vertical Oceanic Circulation." It follows, therefore, that, so far as the distribution of heat is concerned, it is a matter of perfect indifference whether such a circulation does or does not exist.

## THE WIND THEORY OF OCEANIC CIRCULATION.

Ocean-currents not due alone to the Trade-Winds.—The generally received opinion amongst the advocates of the wind theory of oceanic circulation is that the Gulf-stream and other currents of the ocean are due to the impulse of the trade-winds. The tendency of the trade-winds is to impel the intertropical waters along the line of the equator from east to west; and were those regions not occupied in some places by land, this equatorial current would flow directly round the globe. Its westward progress, however, is arrested by the two great continents, the old and the new. On approaching the land the current bifurcates, one

annual rate of the Gulf-stream in the "Narrows;" but in the Admiralty's Current-chart, published October 1872, the minimum rate is stated to be 32, and the maximum rate 120 nautical miles per day. This gives 87 statute miles per day, or fully  $3\frac{1}{2}$  miles per hour, as the mean rate.

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portion trending northwards and the other southwards. The northern branch of the equatorial current of the Atlantic passes into the Caribbean Sea, and after making a circuit of the Gulf of Mexico, flows northward and continues its course into the Arctic Ocean. The southern branch, on the other hand, is deflected along the South-American coast, constituting what is known as the Brazilian current. In the Pacific a similar deflection occurs against the Asiatic coast, forming a current somewhat resembling the Gulf-stream, a portion of which (Kamtschatka current) in like manner passes into the Arctic regions. In reference to all these various currents, the impelling cause is supposed to be the force of the trade-winds.

It is, however, urged as an objection by Maury and other advocates of the gravitation theory, that a current like the Gulfstream, extending as far as the Arctic regions, could not possibly be impelled and maintained by a force acting at the equatorial regions. But this is a somewhat weak objection. It seems to be based upon a misconception of the magnitude of the force in operation. It does not take into account that this force acts on nearly the whole area of the ocean in intertropical regions. If, in a basin of water, say three feet in diameter, a force is applied sufficient to produce a surface-flow one foot broad across the centre of the basin, the water impelled against the side will be deflected to the extremes of the vessel. And this result does not in any way depend upon the size of the basin. The same effect which occurs in a small basin will occur in a large one, provided the proportion between the breadth of the belt of water put in motion and the size of the vessel be the same in both cases. It does not matter, therefore, whether the diameter of the basin be supposed to be three feet, or three thousand miles, or ten thousand miles.

There is a more formidable objection, however, to the theory. The trade-winds will account for the Gulf-stream, Brazil, Japan, Mozambique, and many other currents; but there are currents, such as some of the polar currents, which cannot be so accounted Take, for example, the great Antarctic current flowing for. northward into the Pacific. This current does not bend to the left under the influence of the earth's rotation and continue its course in a north-westerly direction, but actually bends round to the right and flows eastward against the South-American coast, in direct opposition both to the influence of rotation and to the trade-winds. The trade-wind theory, therefore, is insufficient to account for all the facts. But there is yet another explanation, which satisfactorily solves our difficulties. The currents of the ocean owe their origin, not to the trade-winds alone, but to the prevailing winds of the globe (including, of course, the tradewinds).



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Phil. Mag. S.4. Vol. 47. Pl. II

Mintern Bros. Lith.

Ocean-currents due to the System of Winds.-If we leave out of account a few small inland sheets of water, the globe may be said to have but one sea, just as it possesses only one atmosphere. We have accustomed ourselves, however, to speak of parts or geographical divisions of the one great ocean, such as the Atlantic and the Pacific, as if they were so many separate And we have likewise come to regard the currents of oceans. the ocean as separate and independent of one another. This notion has no doubt to a considerable extent militated against the acceptance of the theory that the currents are caused by the winds, and not by difference of specific gravity; for it leads to the conclusion that currents in a sea must flow in the direction of the prevailing winds blowing over that particular sea. The proper view of the matter, as I hope to be able to show, is that which regards the various currents merely as members of one grand system of circulation produced, not by the trade-winds alone, nor by the prevailing winds proper alone, but by the combined action of all the prevailing winds of the globe, regarded as one system of circulation.

If the winds be the impelling cause of currents, the direction of the currents will depend upon two circumstances, viz.:-(1) the direction of the prevailing winds of the globe, including, of course, under this term the prevailing winds proper and the tradewinds; and (2) the conformation of land and sea. It follows, therefore, that as a current in any given sea is but a member of a general system of circulation, its direction is determined, not alone by the prevailing winds blowing over the sea in question, but by the general system of prevailing winds. It may consequently sometimes happen that the general system of winds may produce a current directly opposite to the prevailing wind blowing over the current. The accompanying Chart (Plate II.) shows how exactly the system of ocean-currents agrees with the system of the prevailing winds. The fine lines indicate the paths of the prevailing winds, and the fine arrows the direction in which the wind blows along those paths. The large arrows show the direction of the principal ocean-currents.

The directions and paths of the prevailing winds have been taken from Messrs. Johnston's small physical Atlas, which, I find, agrees exactly with the direction of the prevailing winds as deduced from the four quarterly wind charts lately published by the Hydrographic Department of the Admiralty. The direction of the ocean-currents has been taken from the Currentchart published by the Admiralty.

In every case, without exception, the direction of the main currents of the globe agrees exactly with the direction of the prevailing winds. There could not possibly be a more convincing

proof that those winds are the cause of the ocean-currents than this general agreement of the two systems as indicated by the chart. Take, for example, the North Atlantic. The Gulf-stream follows exactly the path of the prevailing winds. The Gulf-stream bifurcates in mid Atlantic; so does the wind. The left branch of the stream passes north-eastwards into the Arctic regions, and the right branch south-eastwards by the Azores; so does the wind. The south-eastern branch of the stream, after passing the Canaries, reenters the equatorial current and flows into the Gulf of Mexico; the same, it will be observed, holds true of the wind. A like remarkable agreement exists in reference to all the other leading currents of the ocean. This is particularly seen in the case of the great Antarctic current between long. 140° W. and 160° W. This current, flowing northwards from the Antarctic regions, instead of bending to the left under the influence of rotation, turns to the right when it enters the regions of the westerly winds, and flows eastwards towards the South-American shores. In fact all the currents in this region of strong westerly winds flow in an easterly or north-easterly direction.

Taking into account the effects resulting from the conformation of sea and land, the system of ocean-currents agrees precisely with the system of the winds. All the principal currents of the globe are in fact moving in the exact direction in which they ought to move, assuming the winds to be the sole impelling cause: In short, so perfect is the agreement between the two systems, that, given the system of winds and the conformation of sea and land, and the direction of all the currents of the ocean, or more properly the system of oceanic circulation, might be determined à priori. Or given the system of the ocean-currents together with the conformation of sea and land, and the direction of the prevailing winds could also be determined à priori. Or, thirdly, given the system of winds and the system of currents, and the conformation of sea and land might be roughly determined. For example, it can be shown by this means that the Antarctic regions are probably occupied by a continent and not by a number of separate islands, nor by sea.

While holding that the currents of the ocean form one system of circulation, we must not be supposed to mean that the various currents are connected end to end, having the same water flowing through them all in succession like that in a heating-apparatus. All that is maintained is simply this, that the currents are so mutually related that any great change in one would modify the conditions of all the others. For example, a great increase or decrease in the easterly flow of Antarctic water in the Southern Ocean would decrease or increase, as the case might be, the strength of the West-Australian current; and this change

would modify the equatorial current of the Indian Ocean, a modification which in like manner would affect the Agulhas current and the Southern Atlantic current-this last leading in turn to a modification of the equatorial current of the Atlantic, and consequently of the Brazilian current and the Gulf-stream. Furthermore, since a current impelled by the winds, as Mr. Laughton in his exceent paper on ocean-currents justly remarks, tends to leave a vacancy behind, it follows that a decrease or increase in the Gulfstream would affect the equatorial current, the Agulhas current, and all the other currents back to the Antarctic currents. Again, a large modification in the Great Antarctic drift-current would in like manner affect all the currents of the Pacific. On the other hand, any great change in the currents of the Pacific would ultimately affect the currents of the Atlantic and Indian Oceans, through its influence on the Cape-Horn current, the South-Australian current, and the current passing through the Asiatic archipelago; and vice versa, any changes in the currents of the Atlantic or Indian Oceans would modify the currents of the Pacific.

I may now consider the cause of the Gibraltar current. There can be little doubt that this current owes its origin (as Mr. Laughton points out) to the Gulf-stream. "I conceive," that author remarks, "that the Gibraltar current is distinctly a stream formed by easterly drift of the North Atlantic, which, although it forms a southerly current on the coast of Portugal, is still strongly pressed to the eastward and seeks the first escape it can find. So great indeed does this pressure seem to be, that more water is forced through the Straits than the Mediterranean can receive, and a part of it is ejected in reverse currents, some as lateral currents on the surface, some, it appears, as an under current at a considerable depth"\*. The funnel-shaped nature of the strait through which the water is impelled helps to explain the existence of the under current. The water being pressed into the narrow neck of the channel tends to produce a slight banking up; and as the pressure urging the water forward is greatest at the surface and diminishes rapidly downwards, the tendency to the restoration of level will cause an underflow towards the Atlantic, because below the surface the water will find the path of least resistance. It is evident indeed that this underflow will not take place toward the Mediterranean, from the fact that that sea is already filled to overflowing by the current received from the outside ocean.

If we examine the Current-chart published by the Hydrographic Department of the Admiralty, we shall find the Gibraltar current represented as merely a continuation of the S.E. flow of Gulf-stream water. Now, if the arrows shown upon this

\* Journal of Royal United-Service Institute, vol. xv.

chart indicate correctly the direction of the flow, we must become convinced that the Gulf-stream water cannot possibly avoid passing through the Gibraltar Strait. Of course the excess of evaporation over that of precipitation within the Mediterranean area would of itself produce a considerable current through the Strait; but this of itself would not fill that inland sea to overflowing.

The Atlantic may, in fact, be regarded as an immense whirlpool with the Saragossa Sea as its vortex; and although it is true, as will be seen from an inspection of the Chart, that the wind blows round the Atlantic along the very path taken by the water, impelling the water forward along every inch of its course, yet nevertheless it must hold equally true that the water has a tendency to flow off in a straight line at a tangent to the circular course in which it is moving. But the water is so hemmed in on all sides that it cannot leave this circular path except only at two points; and at these two points it actually does flow outwards. On the east and west sides the land prevents any such outflow. Similarly, in the south the escape of the water is frustrated by the pressure of the opposing currents flowing from that quarter; while in the north it is prevented by the pressure exerted by polar currents from Davis Strait and the Arctic Ocean. But in the Strait of Gibraltar and in the north-eastern portion of the Atlantic between Iceland and the north-eastern shores of Europe there is no resistance offered; and at these two points an outflow does actually take place. In both cases, however, especially the latter, the outflow is greatly aided by the impulse of the prevailing winds.

The consideration that ocean-currents are simply parts of a system of circulation produced by the system of prevailing winds, and not by the impulse of the trade-winds alone, helps to remove the difficulty which some have in accounting for the existence of under currents without referring them to difference of specific Take the case of the Gulf-stream, which passes under gravity. the polar stream on the west of Spitzbergen, this latter stream passing in turn under the Gulf-stream a little beyond Bear The polar streams have their origin in the region of Island. prevailing northerly winds, which no doubt extend to the Pole. The current flowing past the western shores of Spitzbergen, throughout its entire course up to near the point where it disappears under the warm waters of the Gulf-stream, lies in the region of these same northerly winds. Now why should this current cease to be a surface-current as soon as it passes out of the region of northerly into that of south-westerly winds? The explanation seems to be this: when the stream enters the region of prevailing south-westerly winds, its progress southwards along

the surface of the ocean is retarded both by the wind and by the surface-water moving in opposition to its course; but being continually pressed forward by the impulse of the northerly winds acting along its whole course back almost to the pole perhaps, or as far north at least as the sea is not wholly covered with ice, the polar current cannot stop when it enters the region of opposing winds and currents; it must move forward. But the water thus pressed from behind will naturally take the path of least resistance. Now in the present case this path will neces-Had the sarily lie at a considerable distance below the surface. polar stream simply to contend with the Gulf-stream flowing in the opposite direction, it would probably keep the surface and continue its course along the side of that stream; but it is opposed by the winds, from which it cannot escape except by dipping down under the surface; and the depth to which it will descend will depend upon the depth of the surface-current flowing in the opposite direction. There is no necessity for supposing a heaping up of the water in order to produce by pressure a force sufficient to impel the under current. The pressure of the water from behind is of itself enough. The same explanation, of course, applies to the case of the Gulf-stream passing under the polar stream. And if we reflect that these under currents are but parts of the general system of circulation, and that in most cases they are currents compensating for water drained off at some other quarter, we need not wonder at the distance which they may in some cases flow, as, for example, from the banks of Newfoundland to the Gulf of Mexico. The under currents of the Gulf-stream are necessary to compensate for the water impelled southwards by the northerly winds; and again, the polar under currents are necessary to compensate for the water impelled northward by the south and south-westerly winds.

No accurate observations, as far as I know, have been made regarding the amount of work performed by the wind in impelling the water forward; but when we consider the great retarding effect of objects on the earth's surface, it is quite apparent that the amount of work performed on the surface of the ocean must be far greater than is generally supposed. For example, Mr. Buchan, Secretary to the Scottish Meteorological Society, has shown\* that a fence made of slabs of wood 3 inches in width and 3 inches apart from each other is a protection even during high winds to objects on the lee side of it, and that a wire screen with meshes about an inch apart affords protection during a gale to flower-pots. The same writer was informed by Mr. Addie that such a screen put up at Rockville was torn to pieces by a \* Paper read to the Edipherek Potenied Society as the protection of the start of the s

\* Paper read to the Edinburgh Botanical Society on January 8, 1874.

storm of wind, the wire screen giving way much in the same way as sails during a hurricane at sea.

Ocean-currents in relation to Change of Climate.—In my attempts to prove that oceanic circulation is produced by the winds and not by difference of specific gravity, and that oceancurrents are the great distributors of heat over the globe, my chief aim has been to show the bearing which these points have on the grand question of secular changes of climate during geological epochs, more particularly in reference to that mystery the cause of the glacial epoch.

In concluding this series of papers, I may therefore be allowed briefly to recapitulate those points connected with the subject of ocean-currents which seem to shed most light on the question of changes of climate, referring the reader for fuller details to former papers on the question.

The complete agreement between the systems of ocean-currents and winds not only shows that the winds are the impelling cause of the currents, but it also indicates to what an extent the *directions* of the currents are determined by the winds, or, more properly, to what an extent their directions are determined by the *direction* of the winds.

We have seen in a former part of this paper (Phil. Mag. Feb. 1870) to what an enormous extent the climatic conditions of the globe are dependent on the distribution of heat effected by means of ocean-currents. It has been there pointed out that, if the heat conveyed from intertropical to temperate and polar regions by oceanic circulation were restored to the former, the equatorial regions would then have a temperature about 55° warmer, and the high polar regions a climate  $83^{\circ}$  colder than at present. It follows, therefore, that any cause which will greatly affect the currents or greatly change their paths and mode of distribution, will of necessity seriously affect the climatic condition of the globe. But as the existence of these currents depends on the winds, and their direction and form of distribution depend upon the direction and form of distribution of the winds, any cause which will greatly affect the winds will also greatly affect the currents, and consequently will influence the climatic condition of the globe. Again, as the existence of the winds depends mainly on the difference of temperature between equatorial and polar regions, any cause which will greatly affect this difference of temperature will likewise greatly affect the winds; and these will just as surely react on the currents and climatic conditions of the globe. A simple increase or decrease in the difference of temperature between equatorial and polar regions, though it would certainly produce an increase or a decrease, as the case might be, in the strength of the winds, and consequently in the

strength of the currents, would not, however, greatly affect the mode of distribution of the winds, and, as a consequence, the mode of distribution of the currents. But although a simple change in the difference of temperature between the equator and the poles would not produce a different distribution of aërial, and consequently of ocean-currents, nevertheless a difference in the difference of temperature between the equator and the two poles would do so; that is to say, any cause that should increase the difference of temperature between the equator and the pole on the one hemisphere, and decrease that difference on the other, would effect a change in the distribution of the aërial currents, which change would in turn produce a corresponding change in the distribution of ocean-currents.

It has been shown\* that an increase in the eccentricity of the earth's orbit tends to lower the temperature of the one hemisphere and to raise the temperature of the other. It is true that an increase of eccentricity does not afford more heat to the one hemisphere than to the other; nevertheless it brings about, as I have already shown, a condition of things which tends to lower the temperature of the one hemisphere and to raise the temperature of the other. Let us imagine the eccentricity to be at its superior limit, 07775, and the winter solstice in the aphelion. The midwinter temperature, owing to the increased distance of the sun, would be lowered enormously; and the effect of this would be to cause all the moisture which now falls as rain during winter in temperate regions to fall as snow. Nor is this all; the winters would not merely be colder than now, but they would also be much longer. At present the summer half year exceeds the winter half year by nearly 8 days; but at the period in question the winters would be longer than the summers by upwards of 36 days. The heat of the sun during the short summer, for reasons which have already been explained, would not be sufficient to melt the snow of winter; so that gradually, year by year, the snow would continue to accumulate on the ground.

On the southern hemisphere the opposite condition of things would obtain. Owing to the nearness of the sun during the winter of that hemisphere, the moisture of the air would be precipitated as rain in regions where at present it falls as snow. This and the shortness of the winter would tend to produce a decrease in the quantity of snow. The difference of temperature between the equatorial and the temperate and polar regions would therefore be greater on the northern than on the southern hemisphere; and, as a consequence, the aërial \* Phil Mag August 1864 February 1867 March 1870 and other

\* Phil. Mag. August 1864, February 1867, March 1870, and other places.

currents of the former hemisphere would be stronger than those of the latter. This would be more especially the case with the trade-winds. The N.E. trades being stronger than the S.E. trades would blow across the equator, and the medial line between them would therefore be at some distance to the south of the equator. Thus the equatorial waters would be impelled more to the southern than to the northern hemisphere; and the warm water carried over in this manner to the southern hemisphere would tend to increase the difference of temperature between the two hemispheres. This change, again, would in turn tend to strengthen the N.E. and to weaken the S.E. trades, and would thus induce a still greater flow of equatorial waters into the southern hemisphere—a result which would still more increase the difference of temperature between the northern and southern hemisphere, and so on-the one cause so reacting on the other as to increase its effects, as was shown at length on a former occasion (Phil. Mag. March 1870).

It was this mutual reaction of those physical agents which led, as I have pointed out (Phil. Mag. March 1870), to that extraordinary condition of climate which prevailed during the glacial epoch.

There is another circumstance to be considered which perhaps more than any thing else would tend to lower the temperature of the one hemisphere and to raise the temperature of the other; and this is the displacement of the great equatorial current. During a glacial period in the northern hemisphere the medial line between the trades would be shifted very considerably south of the equator; and the same would necessarily be the case with the great equatorial currents, the only difference being that the equatorial currents, other things being equal, would be deflected further south than the medial line. For the water impelled by the strong N.E. trades would be moving with greater velocity than the waters impelled by the weaker S.E. trades, and, of course, would cross the medial line of the trades before its progress southwards could be arrested by the counteracting influence of the S.E. trades. Let us glance briefly at the results which would follow from such a condition of things. In the first place, as was shown on former occasions (Phil. Mag. for August 1864, February 1867, March 1870), were the equatorial current of the Atlantic (the feeder of the Gulf-stream) shifted considerably south of its present position, it would not bifurcate, as it now does, off Cape St. Roque, owing to the fact that the whole of the waters would strike obliquely against the Brazilian coast and thus be deflected into the Southern Ocean. The effect produced on the climate of the North Atlantic and North-western Europe by the withdrawal of the water forming the Gulf-stream,

may be conceived from what has already been stated concerning the amount of heat conveyed by that stream. The heat thus withdrawn from the North Atlantic would go to raise the temperature of the Southern Ocean and Antarctic regions. A similar result would take place in the Pacific Ocean. Were the equatorial current of that ocean removed greatly to the south of its present position, it would not then impinge and be deflected upon the Asiatic coast, but upon the continent of Australia; and the greater portion of its waters would then pass southward into the Southern Ocean, while that portion passing round the north of Australia (owing to the great strength of the N.E. trades) would rather flow into the Indian Ocean than turn round, as now, along the east coast of Asia by the Japan Islands. The stoppage of the Japan current, combined with the displacement of the equatorial current to the south of the equator, would greatly lower the temperature of the whole of the North Pacific and adjoining continents, and raise to a corresponding degree the temperature of the South Pacific and Southern Ocean. Again, the waters of the equatorial current of the Indian Ocean (owing to the opposing N.E. trades), would not, as at present, find their way round the Cape of Good Hope into the North Atlantic, but would be deflected southwards into the Antarctic sea.

We have in the present state of things a striking example of the extent to which the medial line between the two trades may be shifted, and the position of the great equatorial currents of the ocean may be affected by a slight difference in the relative strength of the two aërial currents. The S.E. trades are at present a little stronger than the N.E.; and the consequence is that they blow across the equator into the northern hemisphere to a distance sometimes of 10 or 15 degrees, so that the mean position of the medial line lies at least 6 or 7 degrees north of the equator.

And it is doubtless owing to the superior strength of the S.E. trades that so much warm water crosses the equator from the South to the North Atlantic, and that the main portion of the equatorial current flows into the Caribbean Sea rather than along the Brazilian coast. Were the two trades of equal strength, the transference of heat into the North Atlantic from the southern hemisphere by means of the Southern Atlantic and Equatorial currents would be much less than at present. The same would also hold true in regard to the Pacific.

Ocean-currents in relation to the Distribution of Plants and Animals.—In the fifth and last editions of the 'Origin of Species,' Mr. Darwin has done me the honour to express his belief that the foregoing view regarding alternate cold and warm periods

in north and south during the glacial epoch explains a great many facts in connexion with the distribution of plants and animals which have always been regarded as exceedingly puzzling.

There are certain species of plants which occur alike in the temperate regions of the southern and northern hemispheres. At the equator these same temperate forms are found on elevated mountains, but not on the lowlands. How, then, did these temperate forms manage to cross the equator from the northern temperate regions to the southern, and *vice versá*? Mr. Darwin's solution of the problem is (in his own words) as follows :—

"As the cold became more and more intense, we know that Arctic forms invaded the temperate regions; and from the facts just given, there can hardly be a doubt that some of the more vigorous, dominant, and widest-spreading temperate forms invaded the equatorial lowlands. The inhabitants of these hot lowlands would at the same time have migrated to the tropical and subtropical regions of the south; for the southern hemisphere was at this period warmer. On the decline of the Glacial period, as both hemispheres gradually recovered their former temperatures, the northern temperate forms living on the lowlands under the equator would have been driven to their former homes or have been destroyed, being replaced by the equatorial forms returning from the south. Some, however, of the northern temperate forms would almost certainly have ascended any adjoining high land, where, if sufficiently lofty, they would have long survived like the Arctic forms on the mountains of Europe."

"In the regular course of events the southern hemisphere would in its turn be subjected to a severe glacial period, with the northern hemisphere rendered warmer; and then the southern temperate forms would invade the equatorial lowlands. The northern forms which had before been left on the mountains would now descend and mingle with the southern forms. These latter, when the warmth returned, would return to their former homes, leaving some few species on the mountains, and carrying southward with them some of the northern temperate forms which had descended from their mountain fastnesses." Thus we should have some few species identically the same in the northern and southern temperate zones and on the mountains of the intermediate tropical regions." (P. 339, sixth edition.)

Additional light is cast on this subject by the results already stated in regard to the enormous extent to which the temperature of the equator is affected by ocean-currents. Were there no transference of heat from equatorial to temperate and polar regions, the temperature of the equator, as has been remarked, would probably be about 55 degrees warmer than at present. In such a case no plant existing on the face of the globe could live

at the equator unless on some elevated mountain-region. On the other hand, were the quantity of warm water which is being transferred from the equator to be very much increased, the temperature of intertropical latitudes might be so lowered as easily to admit of temperate species of plants growing at the equator. A lowering of the temperature at the equator some 20 or 30 degrees is all that would be required; and only a moderate increase in the volume of the currents proceeding from the equator, taken in connexion with the effects flowing from the following considerations, might suffice to produce that result. During the Glacial epoch, when the one hemisphere was under ice and the other enjoying a warm and equable climate, the medial line between the trades may have been shifted to almost the tropical line of the warm hemisphere. Under such a condition of things the warmest part would probably be somewhere about the tropic of the warm hemisphere, and not, as now, at the equator; for since all, or nearly all, the surface-water of the equator would then be impelled over to the warm hemisphere, the tropical regions of that hemisphere would be receiving nearly double their present amount of warm water.

Again, as the equatorial current at this time would be shifted towards the tropic of the warm hemisphere, the surface-water would not, as at present, be flowing in equatorial regions parallel to the equator, but obliquely across it from the cold to the warm hemisphere. This of itself would tend greatly to lower the temperature of the equator.

It follows, therefore, as a necessary consequence, that during the glacial epoch, when the one hemisphere was under snow and ice and the other enjoying a warm and equable climate, the temperature of the equator would be lower than at present. But when the glaciated hemisphere (which we may assume to be the northern) began to grow warmer and the climate of the southern or warm hemisphere to get colder, the medial line of the trades and the equatorial currents of the ocean also would begin to move back from the southern tropic towards the equator. This would cause the temperature of the equator to rise and to continue rising until the equatorial currents reached their normal position. When the snow began to accumulate on the southern hemisphere and to disappear on the northern, the medial line of the trades and the equatorial currents of the ocean would then begin to move towards the northern tropic as they had formerly towards the southern. The temperature of the equator would then again begin to sink, and continue to do so until the glaciation of the southern hemisphere reached its maximum. This oscillation of the line of maximum temperature to and fro across

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the equator would continue so long as the alternate glaciation of the two hemispheres continued.

This lowering of the temperature of the equator during the severest part of the glacial epoch will help to explain the former existence of glaciers in intertropical regions at no very great elevation above the sea-level, evidence of which appears recently to have been found by Mr. Belt and others.

# XXIV. On the Number and Weight of the Molecules of Æther contained in Electric Conductors. By HERMANN HERWIG\*.

IN the following I will briefly indicate a way in which, certain hypotheses presupposed, very remarkable explanations of the relations of the æther might possibly be reached.

I start from the comparison of the expressions, on the one hand, for the thermal effect of a galvanic current, and, on the other hand, for the vis viva represented therein at any moment by the motion of the electric particles. I may mention beforehand that it is quite immaterial of what kind we suppose the motion of the electrical masses to be. Even a not uniform, somehow oscillating motion would only introduce into the calculation additional simple factors which would be quite insignificant for the final result. Such motions will therefore be left out of consideration.

This presupposed, and holding fast the notion of only one fluid, let e be, in electrostatic measure, the quantity of electricity in motion in 1 millim. length of the conductor, and v its velocity, the unit of time being the second. Then, in mechanical measure, the current-intensity is ev. If, further, also expressed in mechanical measure, R is the resistance of 1 millim. of the conductor, and L millim. the length of the latter, in it there is produced in 1 second, according to Joule's law, a quantity of heat which has in mechanical measure the expression

$$e^2 v^2 \mathrm{RL}$$
. . . . . . (I.)

The motion of the electric masses in this current represents a permanently constant vis viva of the magnitude

$$\frac{\mathrm{L}e}{n}\cdot\frac{v^2}{2}, \quad \ldots \quad \ldots \quad \ldots \quad (\mathrm{II.})$$

where the masses are reckoned in the usual weights; therefore  $\frac{e}{n}$  signifies the quantity of electricity in milligrammes in the unit of length.

\* Translated from a separate impression, communicated by the Author, from Poggendorff's Annalen, vol. cl. pp. 381-385.