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.

FEW 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

* Proceedings of the Royal Society, No. 138, p. 596, foot-note.

† 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. I 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.

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.

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 under-current 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 well-nigh 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 surface-current 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 Gulf-stream really has its origin in the propulsive force of the trade-winds, 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 seas would 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 surface-current 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 disappeared†. 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 Gulf-stream. Again, we have a cold and rapid current sweeping round the east and south of Spitzbergen, a current 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 yards 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 surface-temperature will doubtless be found to be as low as the bottom-temperature. 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 oceanic 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 South 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

* "Depths of the Sea." 'Nature' for July 28, 1870.

† "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 bottom. And it is also probable that the bottom-water is moving 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 Gulf-stream 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. North-east and east of Iceland there is a general flow of cold polar water in a south-east direction towards the left edge of the Gulf-stream. 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*†.

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 seas. This difference, in the very nature of things, must be as 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 the polar oceans. In fact, *equality of the two rates* is necessary 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.

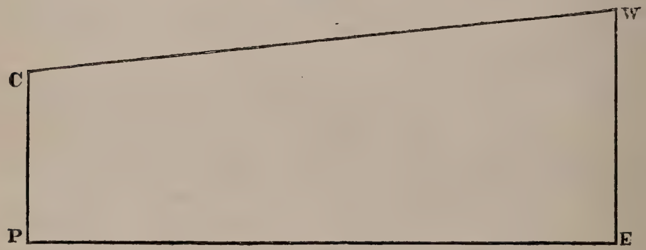
Circulation without Difference of Level.—There is another part of the theory which appears to me irreconcilable with mechanics. It is maintained that this general circulation takes place without any difference of level between the equator and the poles. Referring to the case of the two cylinders W and C, which represent 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 existence 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 W C 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 versâ,* 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 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 yet 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" (§ 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 mechanics. Again, his admission that the general oceanic circulation "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 "surface-heat;" 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* derived from a slope of 1 in 1,000,000; and therefore it is simply begging the question at issue to assert that it is *less*. The experiments of MM. Barlow, Rainey, and others to which

* 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 to the surface. The pull of gravity is always *downwards,* never *horizontally* nor upwards. Gravity will tend to pull the surface-water 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*.

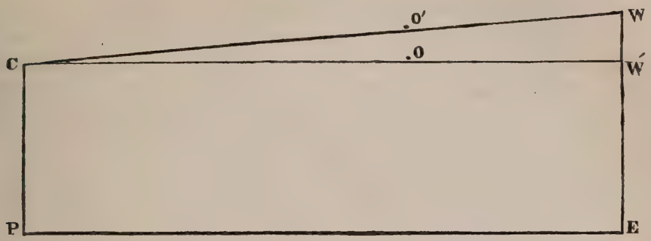
We come now to the most important part of the inquiry, viz. how is the surface-water impelled from the equator to the poles? Is pressure from behind the impelling force here as in the case of the bottom-water of the ocean? It seems to me that, in attempting to account for the surface-flow from the equator 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, P C represents the polar column and E W the equatorial column.

Fig. 2.



It will be observed that the water in that wedge-shaped portion W C W′ 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 C W′. 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 W C; 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 4½ 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 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 sink. Any movement which the ocean as a mass makes in order 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 W W′ 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.

AMONG 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.