young men who reckoned John Goodsir, Edward Forbes, and many others of similar promise amongst their ranks. On leaving Edinburgh he at once came to London, and taking a house at the West End, attempted to establish himself as a pure physician. During these eight or nine years of his London life, Dr. Day laboured on with unwearying industry and patience, lecturing at the Middlesex and other metropolitan medical schools, writing for reviews, translating from German, and turning his versatile talents and his special knowledge of physiological chemistry to account in every way. The result of this heavy strain was a threatening of brain disease, which, according to the verdict of his medical advisers, could only be warded off by complete rest and cessation from the cares in which he was immersed.

At that moment the death of an old friend, Dr. John Reid, opened the prospect to him of obtaining the Chair of Medicine at St. Andrews. His success in this probably saved his life, for the removal from the turmoil of a struggling London career to the comparative ease of the Scottish University arrested the threatenings of disease, and enabled him to recover some of his old vigorous tone. During the 13 years that Dr. Day held the Chair of Medicine at St. Andrews, from 1850 to 1863, he made it his special duty to promote the honour and further the interests of the University by raising the character of medical degrees; and so successfully did he accomplish this task, that the discredit which had belonged in former days to the M.D. degree of St. Andrews was completely effaced under his presidency of the Examining Board. A new system of stringent *vivâ voce* and written examinations was then inaugurated, which justified those who graduated in his time in regarding their attainment of the M.D. degree of St. Andrews as a professional honour of which any man might be proud.

In 1857 Dr. Day's prospects of a more prosperous future than he had as yet been able to look forward to were completely destroyed by the accident to which we have already referred, and which befell him in the course of a vacation tour in the English Lake District. On a bright morning at the end of the August of that year, he had set forth from his hotel at Patterdale in full vigour and strength, bent on "learning a new wrinkle about Helvellyn," as he himself expressed it, by making his way to the summit along a recently opened path. He made the ascent as he had designed, but instead of returning by the same track, he struck off in the direction of the white lead mines; and while walking along what he mistook for a miner's path, the ground gave way under him, and he fell into what proved to be a horizontal chimney or culvert, constructed to carry off the sulphurous, arsenical, and other gases, whose deposits had proved injurious to the sheep grazing on the hill side. He was rescued after three hours of anxious suspense, but the proximate results of that accident were dislocation of the right elbow and two fractures of the same arm, the upper one in the surgical neck of the bone of the humerus, which never united. The subsequent effects were the complete destruction of his general health, which obliged him in 1863 to give up the Chair of Medicine at St. Andrews and retire from active life. A removal to the milder climate of Torquay had little effect in arresting the train of symptoms which year by year marked the progress of disease, and were, it is conjectured, the result of a jar to the spine sustained by his accident on Helvellyn, which had, in truth, proved to him the beginning of the end.

And such was the checkered career of this man of brilliant promise, unflinching bravery of spirit, clear judgment, and tender heart. Disappointed again and again, he always met his troubles manfully, and turned them to good account for himself or others. We have given no list of the various honours which he attained in his profession, or of his literary works, for the detailed reports of these particulars are contained in the various obituary notices which have appeared of Dr. Day in the medical and other journals, to whose pages, as well as to our own, he was a frequent contributor.

OCEAN CURRENTS

A NEW interest seems now to be taken in Ocean Currents, and much is being said and written upon the subject. In the investigation of this subject it is very important that we should understand well all the forces and agencies concerned in the production and maintenance of the currents, and that we should consider well all the principles, and theories based upon hypothetical forces, which have come down to us from preceding generations, however plausible and however much sanctioned by high authority they appear to be. As in the case of the winds, so also in ocean currents, the modifying force arising from the earth's rotation has a very important bearing, and should be well understood. There are certain erroneous views in connection with this force, which have come down to us from preceding generations, and which are contained in text-books, and are being taught in colleges and schools, which are liable to have, and do have, a mischievous bearing upon this subject. These are the more dangerous because they appear to have received at least the tacit sanction of past ages, so that almost any one is liable to adopt them without much consideration. Prof. Colding has in this way been unsuspectingly let into error in his recent paper on ocean currents. We are all familiar with the usual explanation of the trade-winds contained in text-books, which assuming that a particle of air at the equator, at rest relatively to the earth, and consequently having a lineal velocity in space of about 1,000 miles per hour, is forced to move toward the pole, it will, on arriving at the parallel of latitude where the earth's surface has a velocity of only 900 miles, still have its velocity of 1,000 miles per hour in the case of no friction, and consequently have a relative velocity of 100 miles per hour, and on arriving at the parallel of 60°, will still have its initial velocity of 1,000 miles, and consequently have a relative velocity of 500 miles per hour. But this is at variance with a fundamental and well-established principle in mechanics. The force in this case is a central force, or at least the compound perpendicular to the earth's axis can be neglected, since it can have nothing to do with any east or west motion. This being the case, the principle of the preservation of areas must be satisfied, and consequently the particle of air, when it arrives at the parallel where the earth's surface has a velocity of 900 miles, must have a velocity of more than 1,000 miles, and a relative velocity of more than 200 miles per hour, and on arriving at the parallel of 60°, where the earth's surface has a velocity of 500 miles, it must have a velocity of 2,000 miles, and consequently a relative velocity of 1,500 miles, instead of 500 miles per hour. Adopting thoughtlessly, and very naturally, the erroneous principle which is usually taught, that a particle of air or of water in moving toward or from the pole, tends to keep its initial lineal velocity relative to space, Prof. Colding estimates the amount of deflecting force due to the earth's rotation, eastward when the particle is moving towards the pole, and westward when moving from the pole, and the result is, that his force is just one half of what it really is. Consequently, all the results based upon his estimated amount of this force should be doubled. Prof. Colding has also entirely neglected one component of the force due to the earth's rotation. It has been shown by Prof. Everett, and also by the writer, that when a body moves east or west, there is also a similar deflecting force due to the earth's rotation, exactly equal to the former. Prof. Colding has, therefore, taken into account only the one-fourth part of the whole force. If he had taken in this latter component of the force also, and resolved it in the direction of the line of motion and perpendicular to it, as he did the former, he would have found that the parts in the direction of motion, arising from both components, exactly cancel one another in all cases, and that the resultant of both components is a force perpendicular to the direction of motion. This force then tends only to change the direction of the motion, and never to accelerate or retard it, in whatever direction it may be. Prof. Colding's result, therefore, that the velocity of the current is accelerated by the earth's rotation, when moving in certain directions, and retarded in others, is erroneous.

It is known that there are two theories with regard to the cause of Ocean Currents: the one, that they are caused by the winds acting upon the ocean, the other, advocated by Dr. Carpenter, that they are caused by a difference of density of the ocean between the equator and the poles, due to a difference of temperature. The tendency of both theories is in the same direction, and the currents, no doubt, are in some measure due to the forces belonging to each theory. The history of the former theory, and the high authority which can be appealed to in its support, are well known, but we have reason to think that the forces, and the effects of them, in the former theory, are quite subordinate to those of the latter. The well-known explanation of the Gulf Stream by the former theory assumes that there is a heaping up of the water of the ocean in the Gulf of Mexico by the action of the trade winds, sufficient to change the sea-level enough to cause the observed current passing through the Strait of Florida. But the trade winds cannot have much effect in causing a heaping up of the water on the coast of Mexico, since the force is applied to the surface merely, and tends to produce only a surface current, while all the great body of the water, except a little of the surface, is free to flow back. It is true there must be a slight change of sea-level to give rise to a force sufficient to overcome the resistances to this under tow, but these are extremely small since the velocity of this under tow, including all the great depth of the ocean, except the superficial westward current, is very small. That the merely superficial part of the equatorial current is mostly caused by the trade-winds may be true, but the Gulf Stream, which is not directly acted upon, except by the very gentle south-west winds, and which is not merely a surface-current, must be mostly accounted for by the other theory. Let us now see what can be learned upon this subject from observation. Instances of a great change of water-level in shallow canals have been cited to show the influence of the wind in causing a heaping up of the water at the one end; but the water in these cases being very shallow, the force may be regarded as applied somewhat to the whole body of the water, and the under counter-current is thus prevented, but the case is very different in a deep ocean. It is well known from the discussion of tidal observations that the influence of the wind in changing the sea-level is very small. If the force of the trade winds causes a higher sea-level in the Gulf of Mexico, we know that the west winds in higher latitudes must cause a similar rise of sea-level on the west coast of Europe, for the sum of the moments with reference to the earth's axis, of the forces, west between the tropics and east in higher latitudes, must exactly balance each other. If the explanation of the Gulf Stream requires that the level of the Gulf of Mexico should be raised about twelve feet, as shown by Prof. Colding, then there must be about an equal change of level on the west coast of Europe, if these changes are caused by the winds; for although the extent of coast receiving the west winds may be greater than that receiving the east winds, yet this is counterbalanced by the circumstance that the force of the west winds acts at a less distance from the earth's axis, which requires that they should be stronger. If, then, the west winds cause a change of sea-level on the coast of Europe, say of ten feet, then any change in the force of these winds at different seasons must cause a very perceptible change of sea-level. Now, we know that the force of the west wind on the Atlantic Ocean is considerably greater in the spring than the autumn. There should therefore be a corresponding difference in the mean level of the sea, and this mean level on the coast of Europe should be greatest in the spring. But the discussion of the tidal observations made at Brest, shows that the mean level of the sea, after being corrected for the barometer and a very small astronomical term affecting the mean level, is about four inches lower in the spring when the winds are strongest than in the autumn when they are weakest. (Proceedings of the American Academy of Sciences and Arts, vol. vii. p. 32.) The discussion, likewise, of the tides of Boston Harbour gives a similar result, except that the range of the monthly means is still less, being less than three inches. (U.S. Coast Survey Report for 1868.) These results should receive the attention of those who maintain that great changes of sea-level are caused by the winds.

In a paper by the writer, published in *Silliman's Journal* (second series, vol. xxxi. p. 45) there are several pages given to the subject of ocean currents, in which it is maintained that the principal agency in their production is difference of temperature of the sea-water between the equator and the poles. The principal effects of the earth's rotation are there given, which are too numerous to be recited here. In addition to the results there given, the following additional thought may be given here as being perhaps new. As the surface-water flows toward the poles the deflecting force of the earth's rotation presses it toward the east. In like manner as the water below flows toward the equator, there is a similar force pressing it toward the west. These forces are small, but they must nevertheless cause a gradual rising of the cold water at the bottom on the American coast, and this, perhaps more than the Greenland current, causes cold water there. The Gulf Stream of warmer water cuts its way through this cold water gradually rising from the bottom, and hence the cold walls observed by the U.S. Coast Survey.

Mr. Croll seems committed to the wind theory, and is unwilling to admit that the theory advocated by Dr. Carpenter can have even a subordinate effect. His principal argument is based upon an experiment of M. Dubuat. I know not under what circumstances this experiment was made, but of course it was with a comparatively shallow canal or stratum of water, and the result is no doubt correct for the depth of water with which the experiment was made. A much less force on each particle of a large body of water is sufficient to overcome the cohesion of the particles, and produce motion than upon a small one, just as a small drop of water remains suspended to a twig, while the same force of gravity causes a large one to drop off. The case therefore of the ocean is very different from that of a shallow canal. As Mr. Croll insists that Dr. Carpenter's experiment, to be applicable to the case, should have been made with a canal 120 feet long, and only one inch deep; so it might be insisted that M. Dubuat's experiment, to be applicable to Mr. Croll's case, should be made with a canal or body of water three or four miles deep. But there is no necessity for us to make any such experiments, for nature is performing the experiment regularly every six hours, and all that we have to do is to observe. The attraction of the moon changes the level due to the attraction of the earth alone, and puts the ocean, as it were, upon an inclined plane with a gradient of about two feet in the distance of a quadrant, and the water slides down, causing a rising of the tide at one place and a falling at another; and in six hours this gradient is reversed, and the reverse motion of the water follows, thus causing the regular ebbing and flowing of the tides. If M. Dubuat's experiment were applicable to the ocean, the moon could not cause a tide at all unless its mass were about fifteen times greater.

Cambridge, Mass., U.S.A. W. FERREL