PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY A MATHEMATICAL, PHYSICAL & ENGINEERING SCIENCES

Bakerian Lecture: On the Grand Currents of Atmospheric Circulation

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Phil. Trans. R. Soc. Lond. A 1892 **183**, 653-684
doi: 10.1098/rsta.1892.0017

XVII. Bakerian Lecture.—*On the Grand Currents of Atmospheric Circulation.*

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Received March 10,—Read March 10, 1892.

[Professor Thomson died on May 8, 1892, before this Lecture was printed.]

In the early times of the Royal Society (a little more than 200 years ago) a spirit of inquiry and of speculation as to the causes of the Trade Winds arose among its members. The papers which we may presume to have first brought the subject into special notice in the Society, and which were published in the 'Transactions,' offered views which, in the light of subsequent knowledge and theory, show themselves as being untenable, and in part even grotesque. But those papers were soon followed by, and probably had an effect in leading to, a much more important paper by the eminent astronomer Edmund Halley; and this was followed 49 years later by one, more important still, by George Hadley, in which we may with confidence judge that a substantially true theory of a large part of the system of Atmospheric Circulation in its grandest and most dominant conditions was for the first time offered to the world through the pages of the 'Philosophical Transactions.'

Further speculations on the subject and advances in our knowledge of it have been made in later times and have been brought into notice in various ways. I believe that I have myself arrived at some improved considerations which are to a large extent trustworthy and go far towards completing the true theory of the grand currents of atmospheric circulation, and I entertain the ambition to have my views placed on record by this Society—the Society in which the subject had its most important beginnings.

With this in view it appears indispensable that some historical recital should be adduced of the progress made by others previously: but still, for those who may at any time wish to direct their attention specially to the physical conditions irrespective of the history of the progress of thought or of discovery on the subject, it appears desirable that an exposition of the resultant theory which I have devised should be presented without being itself encumbered by historical details of the courses through which it has been ultimately arrived at. I propose, therefore, to present, in a first section, a historical sketch of all the speculations and theories which, as far as known to me, have conduced in any important way towards the resulting theory that I have to offer as being tenable and trustworthy; and then to set forth that new theory itself divested as far as possible of historical or personal

references; and to conclude with some considerations as to the reasons for or against the views put forward by various persons.

The first opening up of considerations and discussions in the Royal Society on the subject of Atmospheric Circulation appears to have been made in a paper submitted to the Society, in 1684, by Dr. MARTIN LISTER, Doctor of Physic of the University of Oxford, and published in the 'Transactions.'* As an illustration of the scanty and crude condition of knowledge and of thought on this great subject at that time—the middle period of the life of Sir ISAAC NEWTON—I may be permitted to cite the views of Dr. LISTER in his own words as offered briefly in that paper:—

"Among the known *Sea Plants*, the *Sargosse*, or *Lenticula Marina*, is not to be forgot; this grows in vast quantities from 36 to 18 *Degrees Northern Latitude*, and elsewhere, upon the deepest Seas. And I think (to say something by the by of that great *Phenomenon* of the *Winds*) from the daily and constant breath of that *Plant*, the *Trade* or *Tropick Winds* do in great part arise: because the matter of that *Wind*, coming (as we suppose) from the breath of only one *Plant* it must needs make it constant and uniform: Whereas the great variety of *Plants* and *Trees* at Land must needs furnish a confused matter of *Winds*: Again the *Levant Breezes*† are briskest about *Noon*, the *Sun* quickening the *Plant* most then, causing it to *breathe* faster, and more vigorously; and that *Plants* mostly languish in the *night* is evident from many of them which contract themselves and close at that time; also from the effects of our winters upon them, which cause them to cast both fruit and leaves too; whereas they are said (the same *Plants* for kind) universally to flourish all the year alike within the *Tropicks*.

"As for the *direction* of this *Breeze* from *East* to *West*, it may be owing to the *General current* of the *Sea*, for a gentle *Air* will still be led with the *stream* of our *Rivers*, for example. Again every Plant is in some measure an Heliotrope, and bends itself, and moves after the *Sun*, and consequently emits its vapours thitherward, and so its direction is in that respect also owing in some measure to the *Course* of the *Sun*."

[NOTE.—The above is the whole passage given by Dr. LISTER about Trade Winds. The rest of his paper relates to entirely different subjects, chiefly to salt springs and brines.]

In scrutinizing these utterances of Dr. LISTER, we may notice that he must have been in possession of some information, more or less vague, to the effect that over extensive regions of the great oceans between the Tropics, or near to them, winds blowing from east towards west are prevalent; and that he has attempted to explain this prevalence by attributing it to the breath of a plant floating on the sea and turning "as an heliotrope" so as to blow its breath westward according to the

* 'Phil. Trans.,' No. 156, p. 494. Date February, 1683–84.

† By "Levant Breezes," here Dr. LISTER obviously means breezes from the east, in fact, the Trade Winds of the tropics.—JAMES THOMSON.

direction of the Sun's diurnal relative motion through the sky from its rising in the east to its setting in the west. He does not indicate any knowledge of the fact that on the two sides of the Equator in tropical regions there are two Trade-Wind zones, one on each side, in each of which the wind prevails from east to west, with an accompanying motion in each case towards the Equator.

We may, indeed, suppose, that such knowledge was only gradually acquired, chiefly by mariners, and was but vaguely and imperfectly intercommunicated among them, and was spread very little among others during a long period of time. I do not suppose that any remarkable step in the discovery and promulgation of knowledge of the prevalent courses of the winds in those seas in and about the Torrid Zone is to be attributed to any one person in particular, nor that there was, indeed, any very important and clear promulgation of the floating knowledge on the subject until the time when the astronomer, HALLEY, collected and systematized a large amount of valuable information, and presented it to the Royal Society, in his paper in the 'Transactions' of 1686, to which I shall make particular reference a little further on.

It may be well at the present stage, before going further into the history of speculations, to draw attention to the chief features of the Trade Winds and other perennially prevalent air currents, as they present themselves very manifestly to the notice of mariners.

The mariners on board a ship at sea, it is to be observed, however, have direct cognizance only of the wind blowing at the spot on the ocean's expanse where for the time being their ship is situated. They can make no observations on the winds blowing at the same moment 100 miles away, and the vault of the sky above them presents to their eyes no adequate indication of the upper currents, or of the places whence these come or whither they are going in their circuits. But even long ago, by the collation among navigators of facts contemporaneously observed by various seamen, important knowledge was acquired gradually as to the general character of contemporaneously existing air currents at the surface of the sea, without the aid of any trustworthy theory as to the continuations of such currents in circulation through the upper regions of the atmosphere. It is further to be noticed that the geographical distribution of sea and land, presenting as it does great regions of ocean, and large continents themselves varied with mountain ranges and low-lying plains, introduces great local variations in the conditions determining the courses of winds, and prevents the institution of any complete uniformity in the character of the air currents all round the Equator, or throughout zones between any parallels of latitude.

But in the Atlantic and Pacific Oceans there are extensive regions within, and adjacent to, the Torrid Zone, in which the winds blow with remarkable constancy from the east while converging also from north and south at the two sides, towards a medial belt of calms and rains which is situated along, or very near to, the Equator.

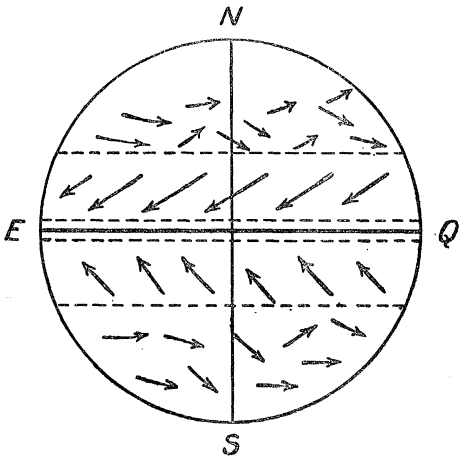
These remarkably persistent winds blowing in the northern hemisphere from the north-east, and in the southern from the south-east, are called the Trade Winds. The

outer limits of the two Trade Winds vary in different seasons of the year, and are affected by casually varying conditions of the atmosphere in other parts of the world, and by the geographical configurations of the surrounding continents affecting them unequally in different parts; but, without minute exactitude, they may be regarded as occupying some such breadth as perhaps 25° or 30° on each side of the Equator.

It was also found by mariners in those early times previous to the development of theories of atmospheric circulation, that in the great oceans, in the higher latitudes, outside of the trade-wind bands, west winds are prevalent in frequency and strength over winds in other directions. It became the practice of traders when going on a voyage from east to west to make their way into the trade-wind region, where they were sure of finding favouring breezes, and on their return voyage to get into higher latitudes, so as to take advantage of the prevailing west winds there.

Until recent years no information was definitely gathered from observations or otherwise as to whether or not there be any prevalent general average tendency in those west winds to blow in their variations more towards the Pole or towards the Equator; and I avoid entering on any statements on the subject at the present historical stage, as that matter will be better associated with the subsequent progress of theories than with the early history.

Fig. 1.



The explanations just given in words as to the chief features of the trade winds and of the west winds of higher latitudes may be supplemented so as to come more vividly before the imagination by aid of fig. 1.

This figure is sketched without regard to the disturbing influences of continents and mountain ranges. It may be regarded as being suggestive of the most remarkable features which would probably present themselves in the winds if the surface of the world were all ocean, or were ocean mottled very uniformly with small islands.

Now, to revert to the historical sketch already entered on, of speculations and theories as to perennially prevalent winds, and to variable winds which manifest perennial prevalence in special directions, the next theory to which I have

to refer, is that of Dr. GARDEN of Aberdeen, which, about one year after that of Dr. LISTER, was placed on record in the 'Transactions of the Royal Society.' Dr. GARDEN, in his paper,* attributes the east to west motion of the Trade Winds of the Atlantic and Pacific Oceans to the supposed vortices of a supposed ether, or all-pervading atmosphere, which, according to the planetary system proposed by DES CARTES, and at that period still believed in by some, were imagined to be the agents carrying on or sustaining the revolutions of the Planets round the Sun, and of the Moon round the Earth, and of the Earth round its own axis. Dr. GARDEN's paper gives indication of his having some knowledge, not only of prevalence of winds from the east within the tropics, but also of prevalence of winds from the west in higher latitudes outside of the tropical regions. He gives no indication of knowledge of the Trade Winds having, along with their westward motion, also motions towards the Equator from both sides; and is, in this respect, apparently on an equality with Dr. LISTER. They had both made praiseworthy exertions in collecting and bringing into notice important results from the observations of mariners and other travellers. When, however, Dr. GARDEN offers explanations of his supposed reasons for the blowing from east to west within the Tropics, and from west to east in latitudes higher than those of the tropical regions, his statements, in their meaninglessness, quite transcend the inadequacy of the explanations in the amusing attempt of Dr. LISTER.

The papers of these two men may probably have had a beneficial effect in instigating HALLEY to prepare, for the Royal Society, a paper presenting the results of his researches as to the observable facts of the winds, and his speculations to account for the prevalent directions of their motions. In 1686, about one year after Dr. GARDEN's paper, HALLEY, then at the age of thirty, submitted to the Society an elaborate and very clear account of the information as to the winds in different parts of the world which he could collect from numerous sources, including observations carefully made by himself on voyages and on land between the Tropics. The title of his paper in the 'Transactions,' is "An Historical Account of the Trade Winds, and Monsoons observable in the Seas between and near the Tropicks, with an attempt to assign the Physical Causes of the said Winds."† His description of the observed facts and his theoretical considerations on the subject, have constituted an important step in the development of the science of that subject, even though his theory in its most important part—that which relates to the east to west motion of the Trade Winds—turns out to be fundamentally untenable. He adduced, no doubt, in his explanation, an important part of the real truth as to causes of the wind, a part which, if not first suggested by him, was clearly either not generally known or not generally adopted at the time.

This true element in his theory consisted in his assigning as the primary motive cause of the winds, the expansion of the air of hot regions, accompanied by its outflow in its upper parts from those regions towards places of less heat and entailing a

* 'Phil. Trans.,' vol. 15, No. 175. September and October, 1685.

† 'Phil. Trans.,' No. 183, p. 153.

diminished pressure at the base of the ascending heated current, and consequently entailing an influx at bottom from the lower part of the atmosphere at the colder places where descending currents are generated. In applying this general principle further to the explanation of the observed winds, he rightly explained the influx of the air from both sides towards the Equator or some medial part of the trade-wind region as being due to the more intense heating effects of the Sun in the Equatorial regions. But in the more important, because less obvious, element for explanation of the Trade Winds and of atmospheric circulation generally—that which is requisite for explaining the east to west motions of the Trade Winds, and the prevalence of winds from west to east in higher latitudes—he quite missed the true explanation. He attributed the east to west flow of the Trade Winds to the diurnal revolution round the equatorial zone from east to west of the maximum of accumulation of heating effect from the daily sunshine, which gives an accumulation of heat in the afternoon in each successive locality. Briefly, he said to the effect that as the maximum of accumulated heat runs round the Torrid Zone from east to west, passing each place at a few hours after noon of that place, and as the maximum of heat in travelling round always causes an indraught towards itself, so the atmosphere of the Torrid Zone must be brought into flowing round from east to west likewise. But this conclusion from the submitted premises is really quite inconsequential.

In reference to this speculation, and treating for the present the direction which we will call the forward direction round the Torrid Zone as being that of the Sun's progress from east to west, we may entertain considerations such as the following:—That consequent on the indraught from all sides towards the hot region, where the barometric pressure is most reduced, the backward-tending forces acting on the air in front of the maximum may be acting as much in respect to time and duration backward on the air in front of the maximum as do the forward-tending forces on the air behind that maximum, and that, through this consideration by itself, we might not be entitled to suppose that any resultant tendency to the generation of a current round the Torrid Zone one way or other, east to west, or west to east, would be produced. But when we further consider the unsymmetrical character of the conditions of the two influxes towards the maximum region from before and from behind, and the to us very unknown accompanying frictional conditions between these unsymmetrically conditioned currents of air and the surface of the earth or sea over which they pass, we may be led to think it very unlikely that the forwarding and backwarding influences would exactly counteract one another; and I certainly think they would not do so, and I think some resultant flow from east to west, or from west to east would be produced, but in which way, east to west or west to east, it would occur I am quite unprepared to say.*

* As a matter of curiosity I think it might be interesting in a time of comparative leisure for some person to make experiments with a spirit lamp or other heater kept revolving slowly round in a circular path under a circular tray filled with water, the path being of a little smaller radius than the tray. The

The theory or speculation in the terms in which it was set forth by its author makes no reference to the inertial conditions of the atmosphere concerned in its diurnal revolution along with the Earth, to which, as a matter of fact, it clings so as to have at all times and all places almost the same revolutional speed or angular velocity of diurnal rotation as the Earth has. In fact, HALLEY's theory would be equally applicable to the case of the world being non-rotative and having the Sun, or an equivalent source of heat, revolving round it from east to west.

But, in view of the very powerfully influencing conditions subsequently brought to light in the theory of HADLEY which will next be adduced, any such feeble causes as those relied on by HALLEY must fall practically into insignificance, the indubitable cause shown in HADLEY's theory being such as to be dominant.

In 1735 GEORGE HADLEY (brother of the JOHN HADLEY who invented the instrument commonly known as HADLEY's Quadrant) submitted to the Royal Society the paper of which I have made mention already as supplying for the first time a substantially true theory of the primarily dominant conditions of atmospheric circulation.* The paper is entitled "Concerning the Cause of the General Trade-Winds," and it is right here to notice that HADLEY applied the name General Trade-Winds, not merely to those winds of equatorial regions to which the name Trade Winds is ordinarily restricted, but uses it as including also the west to east winds known to be prevalent in higher latitudes, and used in trade by mariners for ocean passages from west to east. Thus the scope of his theory must be understood as being much wider than what would be conveyed in ordinary nomenclature by the name, Theory of the Trade-Winds.

In his paper, HADLEY commences by adopting, as a part of the whole truth, the view already in his time currently held by others, that the Sun's heat, intensely applied and greatly accumulated in the equatorial regions of the Earth, conjointly with the cooler temperatures of the regions in higher latitudes, is the main and primary cause of the Trade Winds and other currents of the atmosphere. In this way he supposes that at the Equator or near to it there is a belt of air ascending because of its high temperature and consequent rarefaction, and an influx from both sides towards a zonal region of diminished pressure at its base; and that from its upper part currents float away to both sides, northward and southward, and that these continue in the upper regions of the atmosphere advancing pole-ward until, by cooling in the higher latitudes, their substance gradually becoming less buoyant sinks down gradually and returns towards the equatorial regions as a lower current along the Earth's surface, thence to renew the circulation by ascent again in the equatorial region. While indicating virtually that such atmospheric circulation would be generated, whether in an irrotative world with a source of heat revolving round it

question being, would or would not the water be set into revolutional motion, and if so would it revolve in the same direction as the lamp or other source of heat does?

* 'Phil. Trans.,' vol. 39, No. 437, for April, May, and June, 1735, p. 58.

corresponding to the Sun in its apparent diurnal revolution, or in a world revolving on its axis as does the Earth, he shows that in the latter case—the case, namely, of the revolving Earth—in addition to such circulation as has just been described, east-to-west and west-to-east motions relative to the Earth's surface would necessarily come into being for reasons which may be stated or suggested as follows :—

If we consider the air in a nearly calm region at the outer limits of the trade-wind zone, and regard the air at that place as being at rest relatively to the Earth's surface, and if we consider it to be drawn over the surface by indraught towards the Equator without application to it of any other force than that of the indraught, except what it may receive by friction from the surface of the Earth, be that land or ocean, this air in arriving at places always lower and lower in latitude (and consequently further and further out from the Earth's axis) is coming to places in succession each moving eastward quicker than the previous one ; and thus the air is arriving successively at places each going quicker eastward than the air itself was going when at the previous place ; consequently the air in arriving at each new place must obviously have a slower motion eastward than the Earth's surface at that place has.

Thus throughout that course the Earth must be rushing forward under the air eastward quicker than the air goes, and that is the same as to say that the air must be blowing westward over the surface of the Earth.

In connection with this part of his theory he brings into notice that, while the surface of the Earth at the outer edges of the Trade Winds has much less of absolute velocity eastward in diurnal revolution round the Earth's axis than the surface at or near the Equator has, yet the trade-wind air, on arriving at the foot of the equatorial belt of rising air after its course from those outer parts in higher latitudes, has become imbued with eastward velocity little less than that of the equatorial surface of the Earth, the only deficiency in this eastward velocity from that of the equatorial surface being what is manifested as wind blowing westward over the Earth's surface, or having in relation to that surface a moderate westward velocity. He shows, for an example, that the eastward velocity of the Earth at either of the tropic circles is less than that at the Equator by about 87 miles per hour, but yet that the air which comes from calm regions near the tropic circles to the equatorial belt has, on its arrival at that belt, an eastward absolute velocity which is only a few miles per hour in defect of the velocity of the Earth there, the actual defect being manifested in the relative velocity with which the wind at the equatorial parts blows westward over the surface of the land or sea. He explains that this result is brought about by reason that the air, during its course from the outer edge of the trade-wind zone to the foot of the equatorial rising belt, is perpetually being dragged forward eastward by the quicker-moving land or sea below it, and so its velocity is kept nearly assimilated to that of the part of the Earth over which, for the time being, it exists, and is allowed only to be a little less than that velocity.

Such, then, is HADLEY'S theory, in so far as it relates to the origin of the Trade

Winds of the equatorial regions on both sides of the Equator. His theory further extends to explain the cause of the prevalence of winds from west to east in latitudes higher than those of the winds of equatorial regions, to which, except in the nomenclature of HADLEY himself, the name *Trade Winds* has been usually restricted; and this part of his theory may be represented as follows:—

The equatorial surface of the Earth has a velocity of diurnal revolution from west to east of about 1000 miles per hour. The air of the land and sea at and near the Equator participates nearly in the same velocity. The ascending equatorial belt of heated air retains as it ascends an absolute velocity from west to east nearly the same as that of the equatorial surface of the Earth. He supposes, then, in his theory, that the air floating out from the upper part of the rising belt to north and south over the equatorial zones of Trade Winds, and thence, still in the upper parts of the atmosphere, spreading over extensive regions of land and sea in latitudes higher than those of the Trade Winds, will, on reaching those regions whose velocities of diurnal revolution are much slower, be rushing forward from west to east quicker than do the portions of the Earth's surface over which it successively arrives in floating poleward; that greater speed of eastward motion of the air than of the Earth beneath being, however (as he indicates with a fair approach to clearness), kept in moderation by influences from the surface of the land or sea offering resistance to relative motions of the air above it. Further, he supposes that this upper air, while moving eastward quicker than does the Earth below it, gradually loses a great part of its previously acquired heat, and becomes less buoyant, and consequently descends gradually towards the surface of the Earth, the supply above being always maintained by fresh arrivals from the equatorial regions; and he supposes that the descending air brings from aloft perpetually new supplies of west-to-east motion relative to the surface, and so maintains winds blowing over the surface from west to east. The air then, after its descent from the sky towards the surface throughout extensive regions, must, I think, necessarily, under his theory—although he does not explicitly mention this—be supposed to flow gradually back in the lower levels of the atmosphere towards the Equator, while also blowing prevalently from west to east, till it reaches again the outer border of the trade-wind region, thence to go forward repeating such a circulation as has just been described.

HADLEY concludes his paper with a short passage which, considered in reference to the crude condition of progressive opinions prevalent in respect to atmospheric circulation up to the time of the promulgation of his theory, is to be regarded as suggesting, though in somewhat vague and not entirely correct expression, a very notable and important principle.

The passage is as follows:—"That the N.E. and S.E. Winds within the Tropicks must be compensated by as much N.W. and S.W. in other Parts, and generally all Winds from any one Quarter must be compensated by a contrary wind somewhere or other; otherwise some Change must be produced in the Motion of the Earth round its Axis."

The really important idea which it appears to me is suggested in this passage, is that in respect to the Earth's rotation round its axis the sum of all the forward turning-force-influences applied by the winds to the surface of the Earth, land and sea included, must be equal to the sum of all the backward turning-force-influences likewise applied to the Earth's surface; so that these force influences may be such as conjointly to produce no acceleration or retardation in the revolution of the Earth round its axis.

In putting forward this idea he was doubtless assuming as a principle that we are not to attribute to the thermal influence of the Sun any effects in altering the rotation of the Earth by producing winds blowing upon the Earth more effectually on the whole forward than backward, or the reverse. He did not, nor probably did anyone else till long after his time, notice the now known principle that the Sun and Moon can, by their attractions, apply to fluids on the Earth—to the sea or to the atmosphere—turning forces* which these fluids must communicate to the solid earth, and which must, in very long periods of time, make changes on the Earth's rotation. Such influences, however, are certainly so very small comparatively to those HADLEY had under consideration as occurring in the action of the equatorial Trade Winds from the east, and the winds of higher latitudes from the west, that his not knowing of them is not to be regarded as derogating from the practical or substantial truth and validity of his Theory of the Winds in its main features.

In the account I have given of HADLEY'S theory of the primarily important perennial features of atmospheric circulation, I have endeavoured faithfully to give a fair and favourable account of the truths which he brought to light. I have not held it as a duty to bring under review every statement or phrase to which objection might be taken by an adverse critic. There is one mistake, however, into which HADLEY fell, and which is too important to be passed over without notice. This error, although incorporated by himself along with his true explanations in respect to the causes of the equatorial Trade Winds and of prevalent westerly winds of higher latitudes is quite separable from those true explanations; and its elimination does not make any break down in any essential part of his reasoning as to the real conditions of the atmospheric motions. His error pertained not to his suppositions as to the actual motions of the real air, but to supposed motions and behaviour of air in an ideal case which he adduced as a simplified illustration intended to be helpful to the consideration of the more complex conditions of the real case. The two cases—the ideal and the real—are not explicitly and distinctively specified by himself, but they are brought implicitly under consideration in his statements to the following effect:—

Firstly.—That air having been in an approximately calm condition at one of the Tropic Circles, and having moved thence in the Trade Wind to the Equator, will, on arriving at the Equator, retain still the same absolute eastward velocity that it had

* Any system of forces which can be balanced by what under the nomenclature of POINSOT is called a *couple*, may be described as a turning-force-influence, and may now with advantage be called a *torque*.

when at the Tropic, and so will at the Equator have less velocity of absolute eastward motion than the Earth there has, by 2083 miles per day, or 87 miles per hour, and that so it will be moving relatively to the Earth there as a wind blowing at the rate of 2083 miles per day from east to west.

And *Secondly*:—That as an amendment on the previous statement, it is to be considered that "before the air from the Tropicks can arrive at the Equator, it must have gained some motion eastward from the surface of the earth or sea, whereby its relative motion will be diminished, and in several successive circulations may be supposed to be reduced to the strength it is found to be of."

In reading these two statements conjointly we may with confidence judge that the first of them is not meant to convey the actual truth in respect to the real behaviour of the atmosphere, but that it is only a theoretical utterance as to an ideal case, in which the frictional drag between the surface of the ocean and the atmosphere is left out of account, and that the second is that which is meant to convey the real truth. Now the important error into which he has here fallen, consists in his supposing that in an ideal case, in which the trade-wind air is regarded as frictionless and free from receiving any eastward or westward force-influences from the ocean below it, or as I will add, from the atmosphere immediately above it, it ought to be expected on arriving at the Equator, from a calm at the tropic circle, to retain the same amount of eastward absolute motion which it had when at the tropic. Instead of that, in the ideal case, if fully specified with due limitations, such as we may suppose were tacitly contemplated, without being fully thought out, the true averment would have been that the air on arriving at the Equator would have a velocity of eastward absolute motion less than that at the tropic, in ratio inverse of that of the distances of the two places respectively from the Earth's axis. What I mean here to say, may, perhaps, without elaborate definitions and specifications, be tolerably well suggested in brief words, by saying that, in a vortex of free mobility, with circular motions round an axis, the velocities at different distances from the axis must be inversely as those distances.

But now, in truth, the ideal case which HADLEY touched upon, was quite outside of the scope of the real conditions of the atmospheric motions, which he professed to explain better than had been done in the attempts of others before his time. He had amply sufficient reason for his averment, to the effect that the real trade-wind air in its approach from the tropic to the Equator, under the influence of indraught towards the Equator, should be expected at each new place nearer to the Equator than the previous one, to have a less velocity of eastward absolute motion than the surface of the sea has at that new place, and that the frictional drag eastward applied to the air by the sea surface will only act towards assimilation of the eastward velocity of the air to that of the water, while still in principle, as in fact, leaving the air to go slower eastward than does the water—that is, to blow as a westward wind relative to the ocean. If what he professed to do had been to bring into notice a special

variety of vortex motion, constituting what we may call a vortex of free mobility in a frictionless fluid, and to offer a dynamic theory of its motions, and if his theory had included such an error as the one in question; then his theory would have been fundamentally erroneous. But such was not at all what he professed to do. He proposed to explain certain large and very remarkable phenomena of the observed winds. This he did well, and in doing so he made a very important advance in development of true theory in respect to atmospheric motions.

I have touched in some detail on these matters, because I think that remarks making inadequate recognition of the importance of HADLEY's true discoveries have sometimes been put forward in our own times.

During a period of more than a century from the time of the promulgation of HADLEY's theory, in 1735, there was, I consider, little if any remarkable progress made in development of new speculations for better or for worse in respect to the grand or perennial currents of atmospheric circulation. A long time elapsed, in which there seems to have been little or no vigorous spirit of investigation into the significances or the relative merits of the speculations which had been propounded, or of effort to amend the existing theories, or to discover new truths on the subject. In confirmation of this it may be noticed that we find that 58 years after the publication of HADLEY's paper, DALTON arrived independently at substantially the same theory as that part of HADLEY's which dealt with the equatorial Trade Winds, and in his book entitled "Meteorological Observations and Essays,"* which in 1793 he was preparing for publication, he gave an account of his theory, supposing it to be original, but he discovered, before the book was issued to the public, that he had been completely anticipated by HADLEY's paper, of the existence of which he had not been previously aware. In his preface to that book, after making recognition of HADLEY's priority, he goes on to say:—"I cannot help observing here, that the following fact appears to be one of the most remarkable that the history of the progress of natural philosophy could furnish.—Dr. HALLEY published in the 'Philosophical Transactions' a theory of the trade-winds which was quite inadequate and immechanical, as will be shown, and yet the same has been almost universally adopted; at least I could name several modern productions of great repute in which it is found and do not know of one that contains any other." . . . "On the other hand G. HADLEY, Esq., published in a subsequent volume of the said 'Transactions' a rational and satisfactory explanation of the trade-winds, but where else shall we find it?"

It is right here to remark further that DALTON in his own speculations did not touch at all upon the prevalence of west winds in extra-tropical regions, either as to its explanation or even as to its existence: and that he does not seem to have noticed or appreciated the great importance of HADLEY's theory in this respect.

* "Meteorological Observations and Essays," by JOHN DALTON, D.C.L., F.R.S., 1793. Of this work there is also a second edition, which is a verbatim reprint issued by Dr. DALTON himself, in 1834.

Not only before, but also after this episode of DALTON's speculations and researches so published, the theory of HADLEY must certainly have remained but little read in its author's original paper.

Within the first half of the present century writings on the winds, including the Trade Winds and general circulation of the atmosphere, have been very numerous, some of these have appeared in our encyclopædias, and others in works on meteorology and navigation, and have been widely diffused in atlases containing maps and charts on physical geography.

In such ways many sketches have been presented to the public as explanations of the Trade Winds and other currents of the atmosphere related to them, embodying more or less of the fundamental principles of HADLEY's theory, but often without reference to his name, and usually without due appreciation of the meaning and importance of his theory. In many of these cases we may suppose that the authors had never seen his own original paper, but had obtained their information indirectly through the writings of others.

On the other hand, within the period just mentioned—the first half of the present century—real progress was made in many ways, in the gaining of new knowledge and the making of a few new discoveries, chiefly in connection with the temporary and local disturbances of the atmosphere, and in the bringing together of information of various kinds to help in the elucidation of the subject of the winds. The influence of moisture in air of any given temperature and pressure in rendering the fluid more buoyant was brought effectually into consideration.

The attainment of information from the practical observations of mariners and travellers, and especially explorers of the polar regions, and also from meteorological observatories, was making gradual but important progress. Considerable progress was made in the collecting and correlating by many persons of observational results as to winds and weather and barometric pressures in various latitudes, and in the presenting for practical use among navigators and others of the generalized conclusions so derived.

In that course of progressive labours there were included various speculations or theories as to great storms, commonly designated as hurricanes, tornadoes, or cyclones. In beginning to touch on this subject I have to mention that from among the many persons who may have taken part in researches and speculations regarding cyclones, those whom I deem the most noteworthy are CAPPER, DOVE, REDFIELD, THOM, REID, and PIDDINGTON.

Now, within the period which we have at present under consideration—the first half of the present century—by a very gradual course of experience, chiefly maritime, and of speculation based on such experience, it came to be promulgated that violent storms were generally great whirlwinds; and so the old name *tornado*, of Portuguese origin, suggestive of turning, and the new name *cyclone*, used in the sense not merely of circular form, but also of revolving motion, came to be accepted as well suited for the designation.

Also it was found that in the centre of a cyclone there is a region of comparative calm, and that the centre does not remain stationary, but travels at some moderate speed, taking generally a curved course over the surface of sea or land.

The discovery also was established beyond room for doubt that cyclones in the northern hemisphere revolve in the direction opposite to that of the hands of a watch situated in their locality with its face up; while in the southern hemisphere they revolve in the same direction as do the hands of a watch situated in their locality with its face up.

Also it was discovered and promulgated that in the central region of a cyclone the barometric pressure is remarkably diminished as compared with that of the general surrounding atmosphere, and that this condition must necessarily subsist as a concomitant of the centrifugal tendency or "centrifugal force" of the revolving air, but whether the diminished pressure was to be regarded as a result of the centrifugal force of the revolving air, or as one of the primary causes of the institution of the cyclonic revolution, seems commonly to have been left unnoticed or to have been adverted to under erroneously imperfect views.

Dove, for instance, when discussing the tremendously violent whirling motion which is met with in the inner part of a cyclone immediately around the central region of remarkable calm, says, "*the diminution of barometrical pressure is not the cause of the violent disturbance of the air, but rather a secondary effect of it,*"* and through that passage with its context it seems doubtless that, while entertaining the view that the rapid revolving motion of the air somehow instituted maintains by centrifugal tendency the diminished pressure in the central region, he fails to notice the more complete truth, that without the actual occurrence of centripetal motion caused by predominating influence of inward suction the rapid revolving motion would not institute itself at all.

This being said, however, there is yet, of the whole truth, another element which must be brought into notice, and which I here briefly describe, with some perhaps new ideas that have occurred to myself.

It is, that while for a beginning an accumulation of buoyant air at bottom elongates itself upwards into a shape approaching to a columnar form, and so effects an abatement of pressure at its base; and this abatement of pressure (or suction) induces a centripetal flow towards that place from outer regions where some slight, though it may be almost imperceptible, motions having revolutional momentum (or, in other words, moment of momentum) round that place may already exist, and the revolving mass of air through the action of the centreward forces applied to it, takes an increasingly rapid revolving motion; and further, this rapid motion reacts on the buoyant central column, *keeping that from scattering through the air around it*, and so institutes a very lofty continuous column of the buoyant air.

* Dove, 'Law of Storms,' English translation by Robert H. Scott, M.A., p. 198.

To make this clearer we may notice that if a buoyant central column were for a moment existing surrounded by non-rotative air having greater pressure in its lower parts than that in the column at the same level, that column could not continue its existence. The outer air with its greater pressure would press in on the column, and would increase the pressure in its substance instantly, but the weight of the upper portion of the buoyant column would be inadequate to resist the upward thrust so produced in the lower part, and so the lower parts would shoot those above them upwards with violently accelerating motion. Through the rushing upwards so generated a breaking up of the column would supervene, and its substance would scatter itself in rolling masses among the surrounding air; and the two commingling would ascend gradually, and at the same time the pressure of the surrounding air would communicate itself to the region where the base of the column had been.

But now, on the other hand, if the mass of air around the central buoyant column be whirling, it will keep itself out by the centrifugal tendency accompanying its own rapid revolution, and so will not press in upon and break up that central column of air of diminished pressure, and thus the abatement of pressure at the foot of the column will be maintained and will become further intensified.

To REDFIELD is due much credit for his able and long-sustained labours in collecting and correlating observed facts as to cyclones and the smaller kinds of whirlwinds. He gathered and published* a very interesting collection of accounts of violent columnar whirlwinds which formed themselves over large fires of circular masses of brushwood, the flame and smoke in each case ascending as a lofty rotating column; and this has had part in suggesting to me some elements in the theoretical considerations here briefly sketched out. In his remarks on these whirlwinds he emphatically brought into contrast the distinction between the flames and smoke ascending without whirling motion from hot furnaces and various ordinary fires, on the one hand, and, on the other hand, the revolving columns of flame and smoke often met with in those great fires of brushwood in the open air. By his various researches into the actions and effects of great storms, REDFIELD contributed more, perhaps, than any other man to the advancement of observationally-derived knowledge of their cyclonic character and features.

Wild and fantastic notions were, however, afloat in those times as to the origin of cyclones. Thus PIDDINGTON, in his well-known work entitled the 'Sailor's Hornbook,' even in the edition so late as 1860,† in stating his resultant opinions and conclusions, makes such statements as the following:—

That he considers cyclones to be flat circular disks which may be formed at the sides and upper and lower surfaces of clouds, and which, once formed, may either rise

* "Some account of Violent Columnar Whirlwinds which appear to have resulted from the action of large Circular Fires," by W. C. REDFIELD. Read before the Connecticut Academy of Arts and Sciences, Jan. 22, 1839. Printed in the 'American Journal of Science and Arts" (SILLIMAN'S), 1839, vol. 36. p. 50.

† The 'Sailor's Hornbook,' third edition.

higher or descend downwards, and may extend themselves greatly or contract in diameter, and which may be "parallel to the surface of the globe" or "inclined forwards;" he goes on to say: "It appears to me that a simple flattened spiral stream of electric fluid generated above in a broad disk, and descending to the surface of the Earth, may amply, and simply, account for the commencement of a Cyclone."*

After making careful search through numerous writings on the subject of cyclones, I have to say that I have no reason to think that the investigators who took part in the discovery of the directions of turning of cyclones in the northern and southern hemispheres had generally, or that any of them in particular had, any clear dynamic theory explanatory of the connection between these modes of turning and the rotation of the Earth, nor even of the origin of the very rapid whirling motion itself, but I have found strong indications of deficiency of such knowledge. Even Herschel, so late as 1857, in his article on "Meteorology," in the 'Encyclopædia Britannica,'† stated that a complete account of the phenomena of cyclones had been afforded "by Hadley's theory as developed by Dove in his 'Law of Rotation,' and applied to this specific class of aërial movements by Professor Taylor," and then went on to give what we may presume to be that explanation, but the explanation he gives, although containing enough of truth to prove the connection between the direction of the Earth's rotation and that of the mode of turning of cyclones in each hemisphere, is incomplete, and is vitiated by important errors of principle.

Mr. Wm. Ferrel, of Nashville, Tennessee, in a paper of date 1856 (to be referred to further on in connection with other matters), adduced dynamic considerations of more advanced character for explanation of causes of the gyratory motions of cyclones; but his treatment, although in some respects usefully suggestive and indicating sufficient reason for the direction of turning in each hemisphere, I cannot regard as being on the whole to very good effect.

Also, as a further result of the researches and scrutinies and efforts towards generalization told of already, it came gradually into notice and into acceptance as an established truth that in the latitudes outside the limits of the Trade Winds extending far towards the poles, sometimes for brevity called the middle latitudes, the wind, while prevailing from the west as had been long previously known, prevails also for each hemisphere more from the Equator towards the Pole than from the Pole towards the Equator, so that, on the whole, to take for simplicity the case of the northern hemisphere, the prevalent average atmospheric current at the surface of the Earth in those latitudes was judged to be from the south-west; or, rather, without particularizing one exact point of the compass, and with allowance for great variations in different localities, and at different times, we may better say from south of west towards north of east.

* 'Sailor's Hornbook,' third edition, p. 338, section 410.

† 'Encyc. Brit.,' eighth edition, vol. 14, p. 650.

To account for the component from the south in these westerly winds of our middle latitudes, it came to be supposed, for instance, by LEOPOLD VON BUCH* prominently, as also by many others, that the air departing for the northern hemisphere from the top of the Equatorial Belt of buoyant air, while flowing northward still in the lofty regions of the atmosphere and over the Trade-wind zone, soon becomes a current from the south-west, and continues after descending to the Earth's surface at the northern border of the trade-wind region still to move forward in continuation of its old course as a current from the south-west. But why in the lower regions a pole-ward motion should be maintained rather than a return flow towards the Equator, and how the return from higher to lower latitudes to compensate for this supposed pole-ward surface current should be accomplished, are questions which appear to have been scarcely mooted or to have been left enshrouded in vagueness.

Many examples might be cited indicating the wide currency which such conclusions attained to, but one or two may suffice. Thus, for instance, in JOHNSTON'S 'National Atlas,' of date 1843, we have a map of the winds by Dr. HEINRICH BERGHAUS, of Berlin, on which the zone of south-westerly winds of middle latitudes is described in mysteriously poetic words more captivating to the imagination than satisfying to the reason, as "*Region of South-Westerly Currents of Air, or of the downward returning North-Eastern Trade Wind in Triumphal Conflict with the Northern Polar Currents.*"

HERSCHEL, in his 'Astronomy,'† of date 1850, gives an account for explanation of the south-west winds of middle latitudes substantially to the same effect as that of LEOPOLD VON BUCH and BERGHAUS, and with like vagueness as to the return currents from higher towards lower latitudes.

But from the shelter of that prevalent vagueness, MAURY, in 1855, stepped out and boldly offered a scheme of the general currents of atmospheric circulation which he supposed to prevail, in courses extending from Pole to Pole, and traversing in different ways the lower regions of the atmosphere next the surface of the Earth, and the upper regions which present themselves less directly to the observation of men. Fig. 2 is a copy of his diagram which, in conjunction with his printed explanations, sets forth his scheme of supposed circulation.‡

That figure shows a hemisphere of the Earth's surface taken from Pole to Pole.

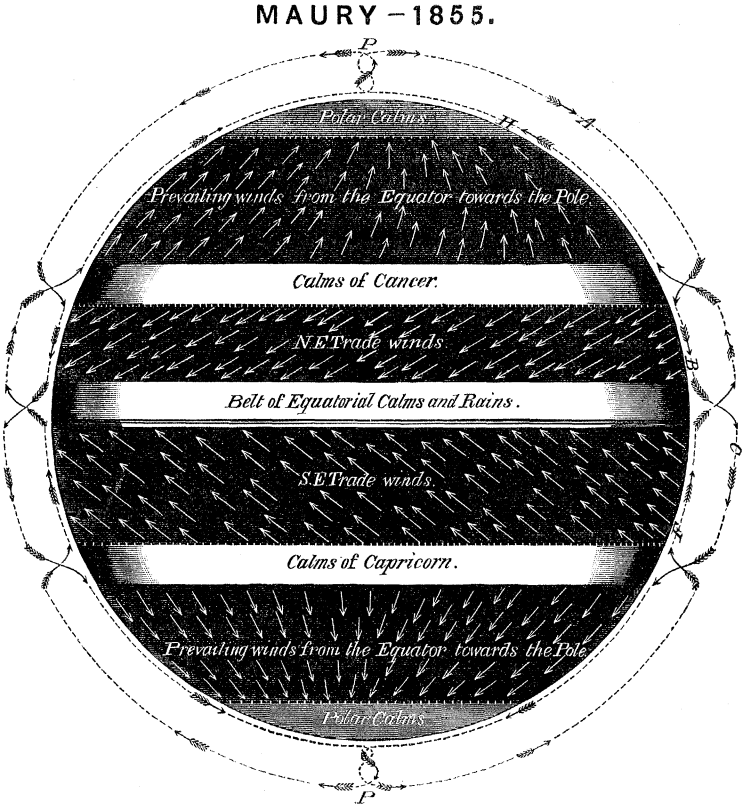
* LEOPOLD VON BUCH, 'Gesammelte Schriften,' vol. 3, Berlin, 1877, where there is to be found his 'Physikalische Beschreibung der Canarischen Inseln,' Berlin, 1825, chapter 2, 'Bemerkung über das Klima der Canarischen Inseln,' pp. 288, 289, and 290. A slightly abbreviated translation of the passage in question is given in DOVE'S 'Law of Storms,' SCOTT'S translation, 1862, p. 39, in a chapter entitled "The Upper Return Trade Wind."

† Third edition.

‡ MAURY'S 'Physical Geography of the Sea.' The first and second editions appeared in 1855. The statements here made apply alike to his 2nd and 6th editions, and presumably also to other editions.

The continents and lands generally are not exhibited, and their disturbing effects on the atmospheric motions are left almost entirely out of consideration. The circulation imagined and described by MAURY in connection with the diagram is meant to be a fair representation of what he would suppose likely to be realised in case of local and temporary disturbances and irregularities being only in a small degree effective. His supposed circulation may be thus described:—He supposes that the air entering the Belt of Equatorial Calms from the southern hemisphere rises there to the lofty regions of the atmosphere, and flows thence as an upper current to the Belt of the Calms of Cancer where it descends to the bottom, from whence it travels on as a south-west wind over the surface of the sea to the high latitudes round the Pole;

Fig. 2.



and that then ascending at and near the Pole, it flows as an upper current out to the Calms of Cancer, where it sinks again to the bottom of the atmosphere crossing the current already mentioned as descending there, and then passes along the surface of the sea as a bottom current forming the north-east Trade Wind, and then enters the Belt of Equatorial Calms, rises there, crossing the previously mentioned rising current there, and thence departs as an upper current towards the Calms of Capricorn, to go through a circulation in the Southern Hemisphere which is an exact counterpart of that already described for the Northern Hemisphere. The supposed currents are further indicated by arrows in the diagram, which, on inspection, may easily be

understood. It is to be understood that the diagram shows a hemisphere of the surface of the Earth with the two Trade Wind Zones exhibited one on each side of the Equator, and separated by the Equatorial Belt of Calms and Rains, which is often also called the Doldrum Belt. And that it also shows the two Border Belts, or Calms of Cancer and Capricorn; and also, in the Northern Hemisphere, the zonal region of wind prevailing from south of west, and, in the Southern Hemisphere, the corresponding zone of prevalent winds from north of west. The arrows shown on the surface of the globe throughout these various zones indicate the directions of motion of the bottom currents of the atmosphere constituting the winds blowing on the surface of the sea. Around the representation of the globe the atmosphere is shown in section with arrows to indicate the north and south, and up and down motions in the circulation, which has just now been described in words.

In offering this scheme of atmospheric circulation, MAURY himself, in respect to the part of it which he propounds as taking place in the regions between the Trade Wind Zones and the Poles, confesses that it is "for some reason which does not appear to have been very satisfactorily explained by philosophers" that the currents he supposes do take place instead of their contraries. In short, he admits that he does not think reason has been found why in those regions the lower current should be towards the Pole and the upper towards the Equator, instead of what we might more obviously expect—namely, a flow towards the Pole in the upper regions of the atmosphere, and a return current towards the Equator in the lower regions close upon the surface of the sea. He even describes the known prevalent motion of the bottom layers of the atmosphere towards the Pole in extra-tropical latitudes as being seemingly paradoxical as to its reason, and although he offers an argument for abatement of the paradox, that argument on the slightest consideration may readily be seen to be futile.

In 1856—the year following after the publication of MAURY's scheme of circulation in his book entitled 'The Physical Geography of the Sea'—quite a new theory was put forward by FERREL in a paper on "The Winds and the Currents of the Ocean," published in the 'Nashville Journal of Medicine and Surgery.'* The scheme of circulation which he then proposed and upheld by mathematical reasonings is illustrated in his paper by a diagram, from which fig. 3 here is taken as a copy. This scheme, as may be noticed by reference to the diagram, and as may be further ascertained by reference to the original paper, includes for each hemisphere three zonal rings of atmosphere, making six in all, each having a separate circulation for itself, except that some small amount of commingling would necessarily take place at each narrow annular interface of meeting between two contiguous zonal rings. For either hemisphere one of these zonal rings of atmosphere covers the trade-wind region

* October and November, 1856. This essay is to be found reprinted in 'Professional Papers of the U.S.A. Signal Service,' No. 12, published by authority of the Secretary of War, Washington, Office of the Chief Signal Officer, 1882.

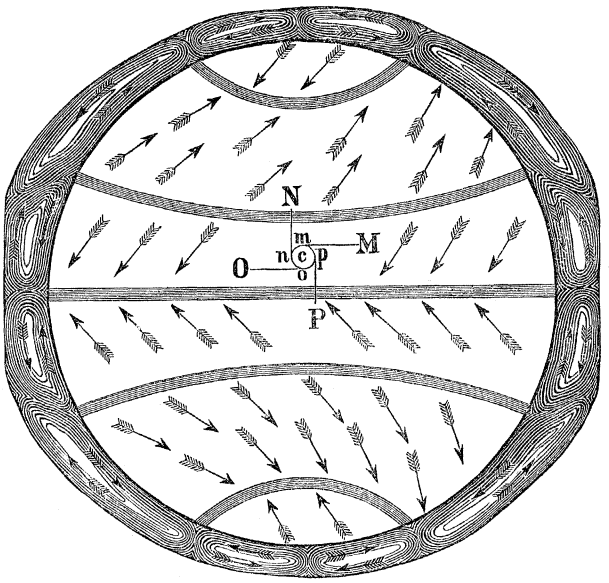
of that hemisphere, another covers the middle latitudes in which winds prevail from south of west in the northern hemisphere, and north of west in the southern, and the third covers the polar region.

Now, attention for simplicity being confined to the northern hemisphere, explanations of the scheme may be continued as follows :—

In the trade-wind zonal ring the bottom current flows from the Calms of Cancer as the Trade Wind to the Equatorial Belt and rises there, and flows then in the upper regions of the atmosphere till it comes to a situation aloft nearly over the Calms of Cancer, and thence it descends obliquely to the Calms at bottom to flow again towards the Equator, and so to begin another circuit alike in character to the one now described. Next in the zonal ring of the middle latitudes, according to the scheme, the current of air taken as beginning at the Calms of Cancer advances in the lower regions over the surface of the sea as a wind from south of west till it comes to

Fig. 3.

FERREL—1856.



about the Arctic Circle where it ascends to the upper regions, to begin a return course proceeding southwards as an upper current till it comes to places aloft nearly over the Calms of Cancer, thence to descend to those Calms below, and so to complete its circulation from some part of that belt back again to the same belt. Next as to the supposed circulation in the zonal ring of the Arctic Regions, it may suffice to say briefly that the lower current is asserted to be from the Pole and the upper current towards the Pole, the ascent from the lower to the upper being at or near to the Arctic Circle, and the descent being in a region closely surrounding the Pole, all as may be seen by inspection of the diagram.

Ferrel, in setting forth in his paper his scheme of circulation and his theoretical reasonings on the subject, introduces as a fundamental principle in it the assertion

that there must be a heaping up of the top layers of the atmosphere to a maximum height at about the parallel of 28° and a "depression" of them over the Equator, and also a "depression" of them at and around the poles and in high latitudes generally, and his diagram is purposely drawn to represent these features.

What has now been said is enough to give a good general idea of FERREL'S scheme of Atmospheric Circulation of 1856. His assumptions, his reasonings, and his conclusions are, I may say with confidence, pervaded by impossibilities and incongruities. But notwithstanding this his paper is deserving of credit for the praiseworthy efforts it manifests towards a more complete consideration of important principles bearing on the subject, which had previously been unknown or neglected or imperfectly touched upon by others.

While I have told of this paper by Mr. FERREL at the present stage in order of dates, yet I deem it right to explain here that I had no knowledge of its existence, nor of any of its author's views, until some years after the publication of the new theory by myself, about which I have to tell forthwith in the present paper.

Through a paper* read before the Natural History and Philosophical Society of Belfast in 1856, by Mr. JOSEPH JOHN MURPHY, of that town, interest was strongly aroused in my mind, in the question of what ought to be supposed to be the true state of the case as to the courses of atmospheric circulation in the zonal regions situated between the trade-wind zone and the Pole in each hemisphere. In that paper Mr. MURPHY brought under notice of the Society the scheme of currents of atmospheric circulation set forth by MAURY, as the truth; and gave a theory or course of reasoning formed by himself, for explaining on dynamic principles how those supposed motions should be accounted for.

On the subject so presented for consideration, I had to judge that Mr. MURPHY'S course of reasoning was not valid for sustaining MAURY'S theory of the atmospheric motions, and I had to judge moreover, that MAURY'S theory was itself, in so far as it dealt with the circulation outside of the trade-wind zones, entirely untenable and impossible.

Mr. MURPHY'S course of reasoning, however, included within it one important element not limited in its scope to the application made of it in that particular course of reasoning. It was the supposition that the low barometric pressure of Polar regions and other high latitudes, already discovered as a fact, through observations of voyagers and others, was to be regarded as due to the centrifugal force of the air revolving from west to east throughout the great cap of atmosphere covering the middle and high latitudes.

Having rejected MAURY'S theory, and having got the benefit of the valuable suggestion just referred to in Mr. MURPHY'S paper, I succeeded in framing a new theory for the circulation in the regions outside of the trade-wind zones. That new theory I put forward in a paper read by me at the meeting of the British Association,

* On the 'Circulation of the Atmosphere,' by Mr. JOSEPH JOHN MURPHY, Belfast Natural History and Philosophical Society, 27th February, 1856.

held at Dublin, in the following year, 1857; and a clear account of it is to be found in the Abstract of the paper published in the British Association volume for that year.

The verbal explanations given in the reading of that paper before the meeting were illustrated by a drawing showing the scheme of circulation described in the paper. Fig. 4, here given, is an accurate copy of that drawing, differing from it only in some unimportant matters, such as in the number of arrows shown, and in its being drawn with abatement of some exaggerations which were made in the original in order to render small features more readily visible at a distance in a large room. The full significance of the original in all respects is retained unchanged in the copy here.

In endeavouring to penetrate the mystery as to what the courses of circulation might be in the middle and higher latitudes, I was in preliminary ways fully satisfied that HADLEY's theory* in its main features—those, in fact, which in the present paper I have already described with commendation—must be substantially true, and must form the basis of any tenable theory that could be devised.

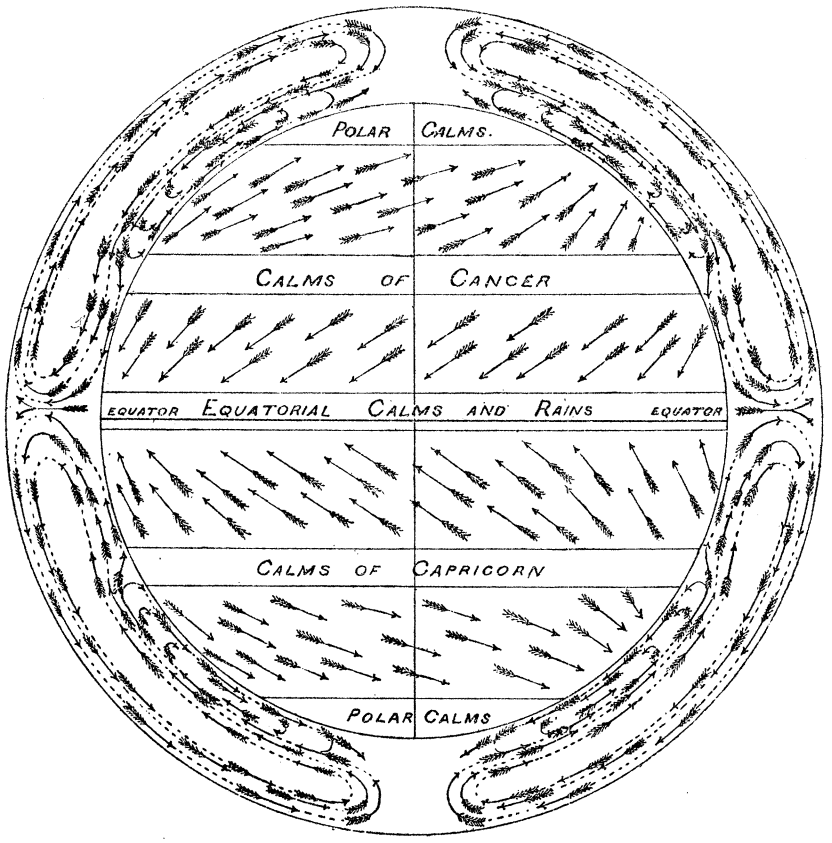
Now, under HADLEY's theory, when we come to consider what may be the courses of circulation that we should attribute to the atmosphere in the latitudes outside of the trade-wind zones, we should naturally be led to expect (as I have pointed out in some detail in an earlier part of the present paper in describing his theory) that the great sheet of air floating out from the Equator in the upper regions of the atmosphere towards either Pole, while having a motion towards the east also, would gradually cool in advancing to higher latitudes, and would therefore descend in middle and high latitudes to the Earth's surface and would next, as a bottom current, flow back towards the Equator while also flowing eastward, and so would be a current towards the Equator, not towards the Pole. But, on the other hand, it had been brought out through accumulated observational results that the winds of middle latitudes while blowing towards the east, which so far is in agreement with HADLEY's theory, do, in opposition to what would be expected under that theory, blow more towards the Pole than from the Pole. Thus the facts and theory seemed to be at variance. It then occurred to me that facts and theory could be reconciled by supposing that the great circulation brought into probability under HADLEY's theory does actually occur, but occurs subject to this modification, that a thin stratum of air on the surface of the Earth in the latitudes higher than about 30°—a stratum in which the inhabitants of those latitudes have their existence, and of which the movements constitute the observed winds of those latitudes—being by friction and impulses on the surface of

* Reference having been made in the text here to my paper read at the British Association Meeting for 1857, on the "Grand Currents of Atmospheric Circulation," and to the Abstract of it printed in the volume of the Association for that year, I have to mention as a correction that the theory here described and correctly designated as HADLEY's Theory, was in the printed Abstract erroneously named as HALLEY's Theory. I was led into that mistake as to authorship of the commonly accepted explanation of the trade winds, through my finding it designated as "HALLEY's theory of the trade winds" by MAURY, in his "Physical Geography of the Sea," to whose newly proposed views in that book my attention was at the time specially applied.

the Earth retarded with reference to the rapid whirl or vortex motion from west to east of the great mass of air above it, tends to flow towards the Pole, and actually does so flow under the indrawing influence of the partial void in the central parts of that vortex, due to the centrifugal force of its revolution. Thus it appeared to me that in temperate latitudes there are three currents at different heights :—That the uppermost moves towards the Pole and is part of a grand primary circulation between Equatorial and Polar Regions ;—that the lowermost moves also towards the Pole, but is only a thin stratum forming part of a secondary circulation ;—that the middle

Fig. 4.

THOMSON – 1857.



current moves from the Pole and constitutes the return current for both the preceding ;—and that all these three currents have a prevailing motion from west to east in advance of the Earth. This was the substance of the new theory which I framed and which, in 1857, I submitted to the British Association at its Dublin meeting. The atmospheric currents supposed under this theory are indicated by arrows in the diagram, fig. 4, and may be traced out readily on inspection. This drawing it is to be understood is not intended to offer any indications of supposed variations in height from bottom to top of the atmosphere in different latitudes.

I exhibited at the meeting, as an illustration, a simple experiment easily extem-

porizable on any occasion. It is mentioned in the printed abstract briefly in the following words :—"If a shallow circular vessel with flat bottom, be filled to a moderate depth with water, and if a few small objects, very little heavier than water, and suitable for indicating to the eye the motions of the water in the bottom, be put in, and if the water be set to revolve by being stirred round, then, on the process of stirring being terminated, and the water being left to itself, the small particles in the bottom will be seen to collect in the centre. They are evidently carried there by a current determined towards the centre along the bottom in consequence of the centrifugal force of the lowest stratum of the water being diminished in reference to the strata above, through a diminution of velocity of rotation in the lowest stratum by friction on the bottom. The particles being heavier than the water, must, in respect of their density, have more centrifugal force than the water immediately in contact with them; and must, therefore, in this respect have a tendency to fly outwards from the centre, but the flow of water towards the centre overcomes this tendency and carries them inwards; and thus is the flow of water towards the centre in the stratum in contact with the bottom palpably manifested."

The general hydraulic principle intended thus to be illustrated by the exhibition of an easily conducted simple case of it is, that if water were lying on a revolving flat-bottomed circular plate or tray, and were revolving at each part quicker than the tray immediately below that part, a flow would institute itself in the bottom layer towards the centre, and that this would occur alike for different speeds of revolution of the tray, and would still take place, likewise, in the case of the speed of revolution of the tray being abated to zero. The case of the non-rotative tray was taken for illustration of the more general proposition simply because of the facility which that particular case presents for being brought into visible manifestation, so as to form to an intelligent mind a help to the imagination in considering the action of the great cap of air lying on the middle and higher latitudes, and revolving prevalently at each part quicker than the Earth below that part does. I offer these explanatory remarks here because in a paper by Mr. FERREL, to be told of a little further on, my illustration by means of the non-revolving tray has been made a point of adverse criticism as to both the nature and the value of the theory I had offered.

Now, before passing quite away from the subject of the original framing of my own theory; I feel it right to make special reference to two considerations which were put forward by Mr. FERREL in his paper of October, 1856.

Firstly.—Ideas were put forward in that paper by Mr. FERREL to the effect, that the low barometric pressure found observationally to exist in polar regions and other high latitudes, is due to the centrifugal force or tendency of the air of the surrounding middle latitudes revolving from west to east quicker than does the earth below: but his views on the matter being unknown to Mr. MURPHY and to myself, did not happen to influence my considerations.

And *secondly*, Mr. FERREL in that paper adduced in connection with other suppo-

sitions an idea which, taking it in a wider scope than that in which he applied it, and with congruity in application not pertaining to the case for which he adduced it, I may describe as implying considerations to the effect that in an atmosphere covering a zonal region such as that of the middle latitudes, and having eastward motion relative to the Earth's surface or, what is the same, having a speed of eastward revolution quicker than that of the Earth below it, a layer at bottom retarded by friction on the Earth's surface, and so having less centrifugal tendency than has the quicker eastward-going air above will be caused to take, along with its eastward motion, a motion also towards the Pole.

The principle is an important one in its applicability to atmospheric circulation; but Mr. FERREL did not apply it to good account. He applied it only in reference to a system of motions already assumed by him, but which in the actual atmosphere are impossible as to causes for their origin and maintenance, and are incongruous in their mutual relations. His purpose in this matter was to show reason for the bottom current flowing towards the Pole while he had the upper current assumed as flowing towards the Equator. He assumed throughout the whole depth from bottom to top in his zonal ring of the atmosphere a motion eastward relative to the Earth, and thereby explained that the frictionally resisted bottom part should flow towards the Pole. But now we have to observe that the only reason why under his theory he can be entitled to assume eastward motion in the lower portion is because of that portion having been previously assumed to flow towards the Pole; and as to the upper portion which he assumes to flow from the Pole, that reason does not hold at all, and the upper portion should rather be supposed, under his theory, to flow westward than eastward. Thus it comes out that he explained the motion towards the Pole in the lower part of the atmosphere by first assuming, for no valid reason, a motion towards the Pole of that lower part. But now, for the primary assumption of that motion towards the Pole in the lower portion of the atmosphere, the reason which he assigned, and which I have just now treated as being not valid, was his supposed heaping up of the atmosphere at top, and consequent increased pressure at bottom at about the parallel of 28°; but, for the heaping up of the atmosphere there he needs in the upper region of the atmosphere over the middle latitudes a speed of revolutional motion greater than that of the Earth's surface immediately below, briefly a relative eastward motion, so that there may be the necessary centrifugal tendency for producing the heaping up, and that is incongruous with the flow in those upper regions taking place, as under his theory he made it do from higher to lower latitudes—from the Arctic Circle to about the parallel of 28°.

He has not thereby anticipated the new and, I think I may say, the true theory offered by me, in which the great body of the lower half of the atmosphere is already shown for good reason to have motion towards the Equator along with motion from west to east, but that a comparatively thin lamina at bottom of it, in virtue of frictional retardation of its eastward motion and consequent abatement of centrifugal

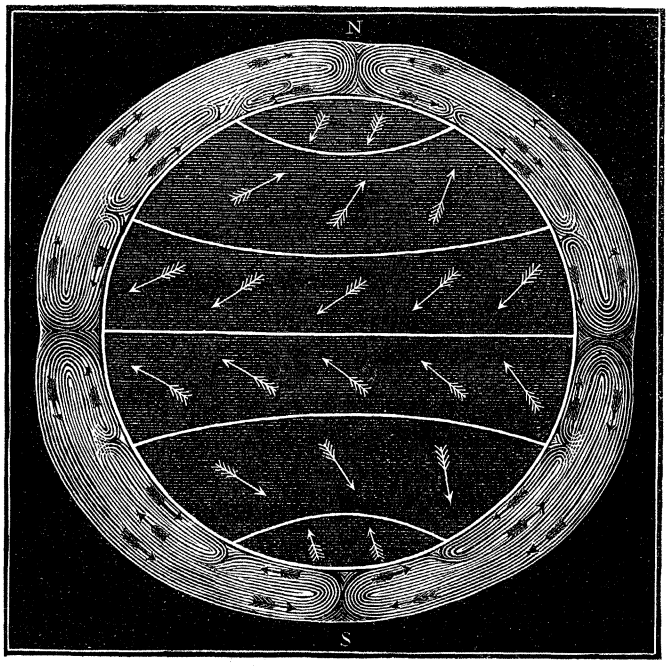
tendency in it as compared to the air above, is caused to reverse what would otherwise be its motion towards the Equator, and to take its course towards the Pole instead.

The next publication to which I have to advert is a second paper by Mr. FERREL. It is entitled "The Motions of Fluids and Solids, relative to the Earth's Surface; comprising Applications to the Winds and the Currents of the Ocean,"* and is dated at its close, "Cambridge, Mass., February, 1860," and is noted on its title-page as being "Taken from the First and Second Volumes of the 'Mathematical Monthly.'"

In that paper he offered a scheme of atmospheric circulation totally different from his previous one of 1856, and entailing a fundamentally altered theory. In fact, he

Fig. 5.

FERREL—1860.



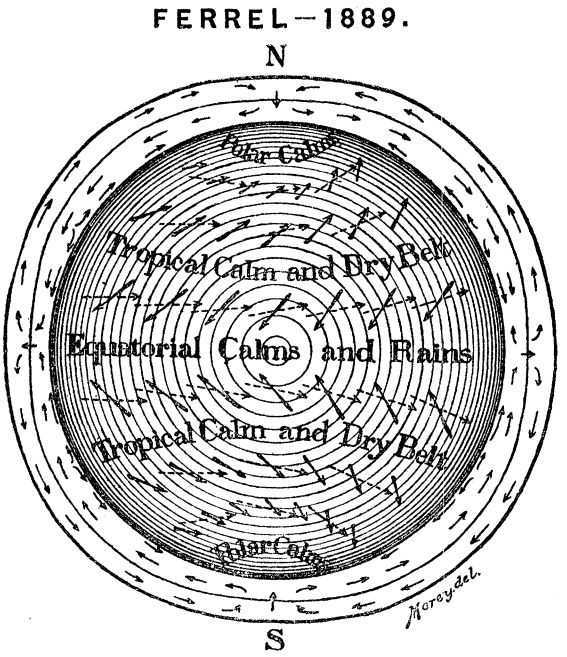
there abandoned his arrangement of six zonal vortex rings of circulation, three for the northern hemisphere and three for the southern, and, instead, he adopted really the scheme that had been put forward by me in 1857 with its two great currents of primary circulation, one flowing from equatorial to polar regions above, and the other flowing as a great return current from polar to equatorial regions below; together with the bottom subordinate current close on the surface of the Earth in middle latitudes or middle and higher latitudes, flowing pole-ward on account of the frictional retardation by the Earth's surface of its eastward relative motion and consequent diminution of centrifugal tendency.

*New York, IVISON, PHINNEY, and Company. London, TRÜBNER and Co., 1860. It appears that this paper was subsequently republished by the United States Signal Service in 'Professional Papers,' No. VIII., with extensive notes giving the mathematical processes in detail by Professor FRANK WALDO.

These currents are shown distinctly by arrows in his diagram, notwithstanding some puzzling confusion introduced by lines which present the appearance of being meant to indicate average current lines, but which, in some parts, would suggest impossible courses, and which show signs of their having been put in without deliberate care. Fig. 5 here is a copy of that diagram.*

The diagram retains two vestiges of his original scheme. Thus it exhibits distinctly a depression of the top of the atmosphere at the Equator making a place of minimum height for the atmosphere there; and it retains systems of arrows throughout the polar regions representing winds having, relatively to the Earth's surface, motion

Fig. 6.



towards the west together with motion towards the Equator; and so in the polar region of the northern hemisphere representing north-east winds. Both of these features I regard as having been introduced through mistaken apprehension. In a later work, indeed, by Mr. FERREL, entitled, 'A Popular Treatise on the Winds,' 1889,† both these features of his former scheme of circulation are completely eliminated from his scheme and theory as there presented. This is shown by his diagram‡ taken in connection with the printed explanations by which it is accompanied. Fig 6 is a copy of this diagram. The depression of the top of the atmosphere, or more strictly speaking, the depression of any isobaric interface in the very lofty

* The same diagram exhibiting his scheme of the winds is repeated in a subsequent paper by FERREL of date 1861, which he offered as being more popular and less mathematical. It is to be found reprinted in 'Professional Papers of the United States Signal Service,' No. XII.

† London, MACMILLAN and Co.

‡ FERREL'S 'Popular Treatise on the Winds,' 1889, § 105, p. 155.

regions to a minimum height over the Equator remains in the diagram, but it is expressly eliminated by words in the accompanying text. This diagram, when corrected according to Mr. FERREL's printed words is, as may readily be seen, essentially the same as my own.

In the closing passage of his second paper, 1860, FERREL made mention of the theory given by me at the British Association meeting in Dublin, 1857, but he did this with erroneous representation of the theory, and with inadequate recognition of its importance and of the fundamental changes he had made from his own previous theory in adopting the main features of mine and incorporating them with some remnants of his own previous views or modes of consideration.

I proceed next to offer some considerations which, I think, may be of intrinsic interest in themselves, besides helping towards the development and elucidation of true theory in regard to atmospheric motions and other conditions.

I have to mention, at this stage, that it may sometimes be convenient, as an aid towards brevity and clearness in expression, to characterize air which has no eastward or westward motion relative to the Earth's surface as having *par*, or being *at par* of revolutional velocity and, likewise, to use the designation *over par* of revolutional velocity to signify eastward relative motion, and *under par* to signify westward relative motion.

(*a*.) Recalling to notice the theory of MAURY and the first theory of FERREL given in his 1856 paper, and drawing attention to the confluence supposed, under both these theories, of two great upper currents of the atmosphere meeting aloft over the belt called the Calms of Cancer in the Northern Hemisphere, and of other two currents likewise meeting over the belt called the Calms of Capricorn in the Southern, I think it is well to remark that, if such a confluence were to take place of two currents, one coming from higher latitudes and the other from lower to a zonal belt of meeting, the current from the higher latitudes would have a rapid westward motion relatively to the Earth below, that is, a revolutional velocity greatly under par, and the current from lower latitudes would have a rapid relative motion eastward, or, in other words, a revolutional velocity greatly over par. They would meet one another obliquely with a velocity of each relative to the other very great because of its having had no frictional mitigating resistance such as the Earth's surface would afford to currents meeting in like manner at bottom of the atmosphere. Thus the belt of meeting aloft would be a place of extraordinary commotion, and this commotion would be propagated with the two descending currents down to the surface of the Earth below; and thus, instead of the Calms of Cancer or Capricorn we ought to expect to find there a belt of wild and varying storms. This very simple, and, I think, very obvious principle, is one of the numerous objections which might singly or conjointly have checked both MAURY and FERREL in the early inception of their theories, and might reasonably have prevented them from propagating views so fallacious.

(*b*.) Next we may raise questions, and proceed to solve them more or less completely,

as to what must be the general character of the motions of the air at various places in the Trade-Wind Zone, both in the lower great current approaching to the Equatorial or Doldrum Belt,* and in the upper great current departing from that Equatorial Belt and flowing aloft over the Trade-Wind Zone to pass over the Border Belt and thence into what, for want of a better name, we may for the present call the middle latitudes. In doing this, we shall have to consider and bring to light some features of the motions of the atmosphere in the middle latitudes more fully in detail than hitherto in the present paper.

Let us accompany in thought the progress of a point advancing with the current along an average stream line, or rather an average current course in the great under-current from Polar to Equatorial Regions. For simplicity, let us confine attention to the Northern Hemisphere. Let us begin the course somewhere within the middle latitudes. To help imagination we may fix on a point of commencement situated vertically over New York. The moving point may, if we please, be idealized as being a small balloon constrained by frictionless guidance to keep in an average current course while being propelled along that average course by the more or less varying motions of the surrounding air. Now, during the progress of the travelling point in its course making way both eastward and southward, so long as the bottom lamina close to the surface of the Earth directly beneath the travelling point is blowing eastward with over-par revolutional velocity, the air above the bottom lamina there must be going forward with still greater over-par velocity. The reason for this statement is, that the only cause for maintenance of eastward relative motion in the frictionally restrained bottom lamina is, that the air above in virtue of revolutional momentum brought from equatorial regions, and not yet exhausted, is blowing with over-par revolutional velocity, and driving forward the resisted lamina below. Also, as long as the over-par velocity is existing in the frictionally resisted bottom lamina under our travelling point, a flow pole-ward also must exist in that bottom lamina at the place, for the time being, directly below the travelling point. This is for reasons fully explained in the account already given of my own theory. When further, the travelling point, in making its way southward, arrives at a stage where that eastward bottom over-par motion no longer exists directly below, and the travelling point then goes on making progress further south, it comes to places where the bottom lamina at the place then below it is moving equator-ward because of indraught thither, and because all reason for that lamina's going northward has ceased. Thereafter in the

* This equatorial belt of rising air may also well be called the Medial Belt, while the Calms of Cancer and Capricorn may be referred to as the Border Belts, this last especially when it is wished to speak of either of these two indifferently, without distinction as to whether it be in the northern or southern hemisphere. Also, either of these border belts may very well be described, or may be named when desirable, as the Belt of Offturn Parting or briefly as the Offturn Parting. The reason for this name will be seen readily by inspection of the diagram, where the great return current towards the Equator is parted into two currents, one going on southwards, and the other turning off towards the north.

Trade-Wind region now entered upon the surface of the Earth is dragging the bottom air forward revolutionally, and so is helping it briskly towards the Equator through increasing its centrifugal tendency.

Then we have to notice that the air, during its course equatorwards and back again through the Trade-Wind Zone, receives forward revolutional momentum through the frictional forward drag applied to it by the Earth's surface, and it loses no revolutional momentum, as the vacuum above the atmosphere can take none from it. So in departing northwards, as the grand upper current, it must carry with it far more revolutional momentum than it had in entering, as the great under current from the north across the Border Belt; but that great under current in entering was either at par, or partly at par, and partly at over-par, of revolutional velocity; consequently the grand upper current must depart across the Border Belt with great over-par of revolutional velocity.

It follows from this, as a corollary, that the top of the atmosphere, or any isobaric interface near the top, must have a declivity in approaching the Border Belt from the top of the Equatorial Belt; and the Border Belt must not have a maximum height with declivity thence to a minimum at the Equator.

The foregoing demonstration seems also likely to give help towards the proper interpretation to be put on observational results recorded by the Krakatoa Committee, and to render highly improbable any suggestions such as seem to be conveyed in some parts of the report, to the effect that in the very lofty regions of the atmosphere—at such elevations as 13 miles above the sea level—a velocity such as 70 miles per hour from east to west has been indicated in the atmosphere, through the phenomena manifested after the great Krakatoa eruption.

(*c.*) In connection with the reasoning or demonstration I have just given, there is another element which I regard as forming part of the whole truth, and which must, I think, form an important element towards the development of the theory more completely. I have already indicated in the demonstration just now offered, that the bottom lamina of the atmosphere in the trade-wind region, is especially helped to advance towards the Equator by the increased absolute centrifugal tendency superimposed on it by the forward revolutional drag it there receives from the Earth's surface; and which communicates to it, throughout its course towards the Equator, new accessions of revolutional momentum, and prevents it from getting into under-par of revolutional velocity, so much as does the air above it in the great under-current towards the Equator. For ready apprehension of this, it is well to notice that the under-par and increased under-par of revolutional velocity imply westward relative motion in the bottom lamina, and quicker westward relative motion in the air next above within the great under current towards the Equator.

This greater abatement of absolute revolutional velocity below par, or increase of relative velocity westward, constitutes a condition opposing flow towards the Equator in the main body of the great under current, and we may reasonably suppose that

the principal flow towards the Equator takes place in the bottom lamina—the lamina whose motions constitute the winds noticeable by action on the sails of ships. So we may suppose that the main body of that great under-current blows nearly due westward with only a small component of motion equator-ward. I could not venture, through theoretical considerations alone, to form an opinion as to the velocity of westward relative motion which might thus be attained to in the main body of the great under-current, or the velocity of westward relative motion which might remain in some parts of the upper current proceeding from the Equator before it has made much advance in latitude to places importantly nearer the Earth's axis. The complications involved in the frictional conditions attendant on the flow of sheets of air with others below and with others above going at very different velocities render the question practically unsolvable by theory alone. But I have to point out emphatically that the Doldrum air, deadened as it is to the condition commonly spoken of as equatorial calm, is very approximately at par of revolutional velocity, and when it rises to the top, or to the very high regions, of the atmosphere, it will have scarcely any westward relative motion, and therefore will not be able to make its way thence as an upper current pole-ward except by flowing as we may say down hill, or as we may better say, among isobaric interfaces down-sloping forward. The lower part of this sheet of deadened air departing aloft pole-ward, and which lower part is much below the top of the atmosphere, and is in close contiguity with the current of westward relatively moving air (already just now mentioned) commencing to move pole-ward without ever having attained to par of revolutional velocity, will we may suppose, by buffeting and commingling between it and that westward relatively moving air, be dragged forward from the Equator, even among up-sloping isobaric interfaces, in a manner that may be likened to being dragged up hill.

I might at present extend the explanations and reasonings on this matter somewhat further, but I abstain from doing so in order not to prolong unduly the present paper. I prefer to leave the subject over for further consideration and exposition by myself, perhaps, and probably by others.

(*d.*) I propose next to offer some considerations in respect to the atmosphere of the polar regions. For simplicity of expression I shall speak, in particular, of the polar regions of the Northern Hemisphere; and I intend that in this, as indeed throughout nearly all I have said in the present paper, the complications introduced into atmospheric motions by local distinctions of the Earth's surface into land and sea are to be, primarily at least, disregarded.

I consider that we should take as one element of our theories the principle that we have to suppose a stagnation of impounded air around the Pole over a great extent of the Polar Regions, this impounded air being maintained by the influx along the surface of the Earth of air frictionally deprived of the over-par of revolutional velocity which is possessed by the great cap of air higher up above the surface of the Earth. This impounded air lags, I affirm, in the Polar Regions, being unable, for want

of revolutional momentum, and accompanying want of centrifugal inertial tendency, to take part readily in the great circulation between polar and equatorial regions. In fact, it cannot get out from its imprisonment there except by being dragged away through gradual entanglement with the comparatively rapidly revolving air arriving by the great upper current from regions having more rapid revolutional motion and passing away in the great middle return current towards the Equator.

(*e.*) Further, I may now offer some considerations as to whether, according to theory, we should expect very clear skies to prevail in the Polar Region of impounded deadened air. I think we must suppose the great upper atmospheric current converging towards the Pole and having over-par of revolutional velocity must be already very dry, owing to its greatly reduced pressure and cold temperature. So, when its air descends in level to return towards the Equator, that air must, I think, be greatly under its saturation point with water-substance; or, in other words, must be far from ready to form clouds, or to precipitate rain or snow. We have to recollect that descending air is generally very rainless.

On the other hand the bottom flow along the surface of the land and sea converging towards the Pole I affirm to be moist. It will be from lower latitudes and generally warmer climates, and will carry moisture with it from sea and land. This bottom current will supply water-substance for cloud and snow in the impounded deadened polar air. The cold of radiation out to interstellar space, coupled with expansion in ascending before it can join the great middle current of return towards the Equator, will cause clouds and snow.

I will now conclude this paper by offering a sketch of a contemplated experimental apparatus for affording practical illustration of the theory of Atmospheric Circulation which I have propounded.

The apparatus would consist mainly of a horizontal circular tray kept revolving round a vertical axis through its centre. The tray would be filled to some suitable depth with water. Heat would be applied round its circumference at bottom, and cold would be applied or cooling would be allowed to proceed in and around the central part at or near the surface. Under these circumstances I would expect that motions would institute themselves, which would be closely allied to those of the great general currents supposed under the theory to exist in either hemisphere of the Earth's atmosphere. The motions of the water, I would propose, should be rendered perceptible to the eye by dropping in small particles of aniline dye, and perhaps by other contrivances. Great variations would be available in respect to the velocity of rotation given to the tray, and in respect to the depth of water used, and the intensity of the heating and cooling influences applied. By various trials with variations in these respects I think it likely that the phenomena expected could be made manifest.