## THE GLACIAL THEORY OF CORAL REEFS.[1]

SUESS'S demonstration that many of the relative changes of land and sea may be due to variations in the height of the sea, while the land remained stationary, and his suggestion that Darwin's theory of coral reefs was as consistent with a rise of the sea surface as with a subsidence of the sea floor, were followed by various attempts thus to explain the phenomena of coral islands. This explanation has now received its strongest support in a valuable memoir by Prof. R. A. Daly, who brings to the problem his usual thoroughness and ingenuity. His interest in the question was roused by the coral reefs of the Hawaiian Islands, which are so small that they are clearly young, and were probably all formed after the disappearance of the glaciers that once existed around the summit of Mauna Kea.

After some years of careful study, Prof. Daly concludes that the coral reefs of the world consist of a thin veneer of coral limestone resting on a great submarine bank; and he holds that the fundamental problem is the origin of these banks, and the recent establishment of the coral reefs upon them. His theory is that coral growth was checked or stopped by the chilling of the tropical seas during Glacial times; that as the temperature rose the coral polyps started active growth, while the sea surface was being gradually raised by the melting of the polar ice-sheets. Prof. Daly assumes that the ice-sheets of Europe, America, and the Antarctic all reached their maxima at the same time; and he calculates that the retention of this water on land would lower sea-level by from 27 to 33 fathoms, while the movement of sea water into the polar regions by the lateral attraction of the ice caps lowered the tropical seas another five fathoms. When the sea was thus lowered wave action planed down the great tropical banks and shelves which now support the coral reefs. One of the longest sections of the memoir discusses the depths of coral lagoons, and claims (p. 194) that "neither maximum nor general depths in atoll and barrier-reef lagoons of larger size should so nearly agree if subsidence has been the essential control in forming coral reefs."

The evenness of the lagoon floors may be due to the distribution of sediment by wave action; for the evidence collected by many authorities, such as Nansen and Stanley Gardiner, has shown that the influence of waves extends far deeper than the limit formerly accepted. The fact that no such great thickness of coral limestone as is assumed by Darwin's theory has ever been conclusively established cannot be lightly set aside; and Prof. Daly makes the novel suggestion that the formation of coral reefs may have been stopped by excessive heat as well as by cold. He remarks that when Grinnell Land had a January temperature 50° warmer than it has now, the growth of corals in the tropics was probably inhibited owing to the lowering of their vitality by excessive heat.

Prof. Daly has, therefore, adopted the bank theory of coral reefs, which, as he remarks, was advocated by Tyerman and Bennett in 1832, and in later times by Wharton and Agassiz. The part of Sir John Murray's theory which explained the depth of lagoons by solution is summarily dismissed. That Prof. Daly's explanation is correct for some coral islands may be at once admitted. Thus the evidence from the Maldives and Laccadives, which Prof. Daly clearly states, long ago led supporters of the Darwinian theory to regard those reefs as a coral crust upon a submerged ridge parallel to the Western Ghats. Sir William Wharton originally proposed that one of these islands should be selected for the boring test, but he withdrew this recommendation when it was pointed out to him at the British Association Committee on the subject that these islands would not be regarded as a satisfactory test; so he withdrew his proposal, and at the next meeting recommended Funafuti, which was afterwards selected for the famous boring. Its evidence, however, Prof. Daly rejects on the ground that the bore passed into coral talus, and that "the actual site of the borings was unwisely chosen" (p. 247); but taking all the circumstances into account, the site on Funafuti was probably the best available.

Glaciation has been summoned to relieve geologists from many difficulties, and in spite of the ingenuity of Prof. Daly's arguments, the Darwinian theory may still survive this appeal to Glacial influences. The fundamental assumption that all the Glacial ice-sheets reached their greatest size simultaneously seems opposed to the current trend of opinion. The Glacial period was obviously one of widespread earth movement; the subsidence of Scandinavia, the British Isles, and northern America during their glaciation would have tended to lower the sea-level; but these movements and the amount of water used in the formation of land ice might easily have been masked by uplifts under the tropical oceans.

One objection to the view that the coral reefs have grown upward to keep pace with a rise of sea-level has generally been regarded as fatal; for any such movements should have affected the whole of the tropical seas and should have been uniform throughout them. But vast lengths of coast show no sign of any such rise of sea-level. In the coral seas themselves some districts have raised reefs, while elsewhere the coasts present the features characteristic of subsidence. This fact was shown by Darwin, and has been confirmed by the detailed work of Alexander Agassiz. The grouping of coral reefs according to size and form is also evidence that the coral seas have been affected by differential movements of the sea floor. Dana showed that the coral islands are so grouped as to indicate rapid subsidence along certain lines, while adjacent areas remained stationary. Such facts of distribution appear irreconcilable with the Glacial control theory.

J. W. G.

[1] "The Glacial-Control Theory of Coral Reefs." By R. A. Daly. Proc. Amer. Acad. Arts Sci., Vol. li, No. 4, 1915, pp. 157–251.

## ILLUSIONS OF THE UPPER AIR.[1]

### A REVIEW OF PROGRESS IN METEOROLOGICAL THEORY IN ENGLAND SINCE 1866.

*The Study of Cyclones and Anticyclones.*

IN 1866, a year after Admiral FitzRoy's death, the Royal Society undertook, by means of the new Meteorological Office, to establish seven other observatories in various parts of the country, equipped just like the Kew Observatory at Richmond, and to use the automatic records in explanation of the weather as set out in the daily maps. The explanation of the winds and the interest of the sailor were the justification of the public expenditure.

Meteorologists knew about cyclones from Piddington in 1848 and about anticyclones from Galton in 1863; from that time onwards until the end of the century the study of cyclones and anticyclones was the dominant idea of dynamical meteorology.

It was mainly conducted by observations at the earth's surface; and necessarily so. In 1852 Welsh, the superintendent of Kew Observatory, had made four sets of excellent observations of the upper air in

[1] From a discourse delivered at the Royal Institution on Friday, March 10, by Sir Napier Shaw, F.R.S.



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balloons, and Glaisher had followed them up by a large number of ascents for the British Association, which reached their climax in the famous ascent with Coxwell in 1862. They added a good deal to our knowledge but very little to our ideas. They told us that the atmosphere showed continual decrease of temperature with height, and that surprised nobody; it was a natural incident in the gradual transition from the temperature of the surface of the earth to the absolute zero of space. "The nicely calculated less or more" was not of vital importance. Cyclones and anticyclones obviously belonged to the upper air, the regions where clouds are formed and dissipated, where rain and snow and hail are produced, but balloon ascents told us little about them beyond confirming the surmise that there are great ascending currents associated with certain forms of cloud.

The only real information to be got about the atmosphere in upper regions was that contained in observations of pressure at the surface, which is the cumulative result of the whole thickness of the atmosphere, and the amount of rain, hail, or snow which falls from above. There were also observations of the forms of cloud and their motion, and, if we please, of their position. The rest is necessarily speculation, so that out of these observations meteorologists were obliged to imagine for themselves what cyclones and anticyclones are, how far up they extend, how they are produced and maintained, what kind of air they are made of, and so on.

*Observations of the Upper Air.*

Speculation can do a great deal with the atmosphere. It goes beyond the reach of our balloons, and tells us of the substitution of hydrogen and the rarer gases for oxygen and nitrogen in the region of the meteor and the solar electron But from the year 1896 onwards there has been a systematic collection of facts about the upper air by using kites to carry instruments up to heights of 3 kilometres, or occasionally more; ballons-sondes which carry instruments up to heights of 35 kilometres (20 miles or more); and pilot balloons which give the direction and velocity of the wind at various levels up to 10 kilometres, sometimes more.

*Comparison of Fact with Speculation.*

This investigation has given us a wealth of information about the upper air. The principal result is the division of the atmosphere into two layers: a lower layer about 10 kilometres thick, the troposphere, the region of convection; and an upper layer, the stratosphere, in which there is no convection. We can use the information to test some of the generally accepted ideas about cyclones and anticyclones by comparing the results of speculation with the new facts. Many of the pictures which we imagined now appear to have been illusions. Those of us, for example, who thought that because the air was warmed from the bottom, the upper part would be free from sudden changes of temperature such as we get at the surface were rapidly and rudely disappointed. Simplicity is not apparently the characteristic of the upper air.

*The Convection Theory of Cyclones and Anticyclones.*

Before giving you other examples, let me quote the description by which Galton introduced the name "anticyclone," because the mental picture of the structure of cyclones and anticyclones which has guided the thoughts of the majority of meteorologists has been formed by the gradual elaboration of the ideas contained in that description:—

"Most meteorologists are agreed that a circumscribed area of barometric depression is usually a locus of light ascending currents, and therefore of an in-draught of surface winds which create a retrograde whirl (in our hemisphere).'

"Conversely, we ought to admit that a similar area of barometric elevation is usually a locus of dense descending currents, and therefore of a dispersion of a cold, dry atmosphere, plunging from the higher regions upon the surface of the earth, which, flowing away radially on all sides, becomes at length imbued with a lateral motion due to the above-mentioned cause, though acting in a different manner and in opposite directions" (Proc. Roy. Soc., vol. xii., 1862–1863, p. 385).

Out of that there gradually grew the conception, on the one hand, of the central area of a cyclone on the map as a centre of centripetal motion, a focus of attraction for the surrounding air, and of the general area of the cyclone as a region of ascending warm air producing rain or snow; round the central region the air moves inward with a counter-clockwise motion in spiral curves. On the other hand, the conception of the central area of an anticyclone is of a centre of centrifugal motion, a region of repulsion; the general area of an anticyclone is a region of descending cold air moving with a clockwise motion spirally outwards. The fundamental dynamical idea is that of air driven like gas along a pipe from high pressure to low pressure, retarded by the friction of the surface, and diverted from its direct object by the rotation of the earth.

For future reference, let us separate the three elements of this picture and keep them distinct. First, the *circulation*, counter-clockwise in a cyclone, clockwise in an anticyclone. Second, the *convergence* across the circulation from high to low. Third, the *convection*, or vertical motion, which appears as ascending air in the cyclone and descending air in the anticyclone.

According to the conception which developed on the lines of Galton's description, and found ready acceptance, the circulation is incidental to the convergence; the convergence is universal, the convection genera'

It is another example of the *facilis descensus Averni*. The very simple piecing together of the three parts makes it almost obvious that the third element, the convection, is the effective cause of the whole dynamical process; it is natural to regard convection as the ascent of warm air in a relatively cold environment, causing low pressure on account of the relatively high temperature of the ascending air; and high pressure as the natural corollary of cold descending air. The convergence, or motion across the isobars, is the primary result of the distribution of pressure, and the circulation is merely the deviation from the straight path caused by the rotation of the earth. The theory is quite simple and quite self-contained, and it has this great advantage: that the cause which it assigns for the cyclone, namely, the convection of warmed air, has always been regarded as the cause of winds; it has been accepted as explaining land- and sea-breezes, the trade winds and the monsoons; and if it is also accepted as explaining the cyclone and anticyclone, which are the modern meteorological names for the diverse winds of the temperate latitudes, we can see in the idea a beautiful unity in meteorological theory. The origin of all the winds is thereby assigned directly to what we know must be their ultimate cause, namely, the warming of the lowest layers of the air by the warmed surface of sea or land. If we doubt its efficiency in one case, there seems no good reason for holding to it in the others.

It seems a pity that an illusion which apparently does such good service should be shattered; but it cannot face the facts of the upper air.

You will notice that the whole matter depends upon



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the idea of the low pressure in the warm ascending air of the cyclone as the driving force, whatever be the area covered by the circulation. The observations of the upper air have made us familiar with certain facts about the height of the atmosphere that make such an idea too improbable. The convective atmosphere is only about 10 kilometres thick. The region in which convection can operate is therefore a thin skin represented by a centimetre in the case of a map on the millionth scale, on which 1000 miles is about 6 ft. in length. A cyclone is often regarded as a towering structure which may produce curious effects by tilting its axis, but that is clearly illusory; the idea that descending air over northern France is operating in conjunction with rising air over Iceland to produce a flow of air along the line joining them is an unproductive way of representing the facts.

The idea of the ordinary cyclones and anticyclones in our latitudes as foci of centripetal and centrifugal motion is an illusion. In all ordinary cases of cyclone the convergence of the paths of air towards the centre is itself an illusion, because the motion of the cyclone makes it miss its apparent aim, and we get in actual fact paradoxical cases of air which, always seeking a place of lower pressure, yet makes its way to a place of higher pressure, because the pressure has been raised over its path; and though it always seeks the centre, in reality it goes further away from it. If it wanted to reach it, it was a mistake to aim at it; if it wanted to get near, it should have aimed to get away. There certainly is convergence and convection, but it is local and not general over the cyclone. The idea which is conveyed by convergence in spiral paths to the centre of a moving cyclone is an illusion. It did not even require observation of the upper air to tell us that.[2]

Take the time required for the operating forces to produce any such wind velocities as we find in actual experience. In one hour an ordinary pressure-difference would produce a velocity of 1000 metres per second if it were free to act. The time required to generate a velocity of, say, 10 metres per second is infinitesimal compared with the time during which we see the forces in operation; these last for hours, or even days, while a minute would suffice for the production of all the velocities exhibited; the motion of the air which we register on anemometers is not accelerating motion but uniform motion, except for the effect of turbulence and local convection; so we must picture to ourselves the air of cyclones as being under the operation of balanced forces, not unbalanced forces. I wish to suggest that the idea of air being accelerated by the forces we see on the map is another illusion so far as the upper air is concerned.

The ostensible reason for supposing that the distribution of pressure created by convection is pushing air from high to low is due to the fact that the charted winds show the air at the surface crossing the isobars from high to low; the observations with kites and pilot balloons suggest that the effect is peculiar to the surface. If the driving force from high to low were the operative force which produces the wind of a cyclonic depression, we should expect to find its operation more strongly marked as we get higher up, because the friction of the surface would not interfere with it; but the fact is quite otherwise. The movement across isobars becomes less and less marked as we ascend. It is much less at Pendennis Castle than it is at Falmouth Observatory, a mile away. We cannot be sure that it exists at all at 1500 ft., because we cannot draw the isobars at that level with the necessary accuracy; the consensus of our observations goes to show that there is no real evidence of convergence at that level. There the centrifugal force of the air travelling over the moving earth, combined with the centrifugal force due to the curvature of the air's path, is sufficient to balance the force due to pressure, and there is no component of motion towards the centre.[3]

What happens nearer the surface is that the friction of the surface converts part of the energy of the motion of the wind into eddy motion and the air does not move fast enough on the right path to keep up the balance. Consequently, it drifts inwards as a pendulum does when its motion is retarded, but the lower air cannot hold back the air far above it; the effect of viscosity in that direction was shown by Helmholtz to be negligible. The effect of the eddy motion is very limited in height.

*Observations in the Upper Air in Relation to the Convection Theory.*

But the greatest blow to the illusion that I have portrayed comes directly from the observations of the upper air; the convection theory requires that the air of the cyclone should be warmer than that of the anticyclone, but, as a matter of fact, the new observations show that the opposite is the case.

In a paper published by the Royal Society, Mr. W. H. Dines[4] gave the mean values of the observations of temperature in the upper air of this country arranged according to the pressure at the ground. From his results the following table has been compiled:—

*Table of Average Values of the Pressure, Temperature, and Density of Air in High and Low Pressure.*

| Height | | High pressure | | | Low pressure | | |
|---|---|---|---|---|---|---|---|
| | | Pressure | Temp | Density | Density | Temp. | Pressure |
| 1000-ft. | k. | mb. | A | $g/m^3$ | $g/m^3$ | A | mb. |
| 32·809 | 10 | 273 | 226 | 421 | 382 | 225 | 247 |
| 29·528 | 9 | 317 | 233 | 474 | 444 | 226 | 288 |
| 26·247 | 8 | 366 | 240 | 531 | 514 | 227 | 335 |
| 22 966 | 7 | 422 | 247 | 595 | 583 | 232 | 388 |
| 19·685 | 6 | 483 | 254 | 662 | 652 | 240 | 449 |
| 16·406 | 5 | 552 | 261 | 736 | 724 | 248 | 516 |
| 13·124 | 4 | 628 | 267 | 818 | 807 | 255 | 591 |
| 9·843 | 3 | 713 | 272 | 911 | 893 | 263 | 675 |
| 6·562 | 2 | 807 | 277 | 1012 | 992 | 269 | 767 |
| 3·281 | 1 | 913 | 279 | 1137 | 1100 | 275 | 870 |
| 0 | 0 | 1031 | 282 | 1270 | 1226 | 279 | 984 |

The figures show that a pressure-difference of 26mb. exists at the level of 10 kilometres where convection has ceased to exist. The difference is accentuated to the extent of 21mb. as the surface is reached by the existence of the high pressure transmitted from above, in spite of the relative coldness of the air at the lower pressure. The diagram included in Mr. Dines's paper showed that there is a remarkable change at the top of the troposphere. Above the level for which values are given in the table, *the high is colder than the low, reversing the state of things in the troposphere.*

We cannot resist the conclusion that the pressure-differences of cyclone and anticyclone are not local surface effects at all; we must seek their origin in the upper air where there is no convection. They are little affected by the lower stratum of 9 kilometres, which, roughly, marks the range of the effect of heating at the surface.

The idea of warm air in the lower layers causing the low pressures which are recorded on our barometers is therefore an illusion.

Thus it will be seen that the observations of the

[2] See "Life-history of Surface Air-currents." By W. N. Shaw and R. G. K. Lempfert. M.O. publication No. 174.

[3] See the four reports on wind structure to the Advisory Committee for Aeronautics by W. N. Shaw and J. S. Dines, also "Barometric Gradient and Wind Force," by Ernest Gold. M.O. Publication, No. 190.

[4] See M.O. Publication No. 210b. Geophysical Memoirs No. 2.



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upper air have proved that all the vital parts of the facile description which was the accepted theory of cyclones and anticyclones are quite illusory. What it took for guidance in forming a picture of the structure was the accidental character of motion near the ground. We now feel that the motion of air in the lowest kilometre had better be disregarded, or, better still, be handed over to students of turbulent motion, while we as meteorologists consider the normal state of the atmosphere as motion under balanced forces. Instead of a natural flow from high pressure to low pressure, we have a natural flow without any change of pressure; the motion of a heavenly body round its sun is taken as the type for the air instead of the motion of a falling stone.

While we are considering illusions, let me add another example depending upon what was at one time, and possibly is still, a commonplace of physical teaching in regard to the relation of barometric changes to weather.

It is this: moist air is lighter, bulk for bulk, than dry air, and consequently pressure is low where the air is moist. That is why a low barometer is indicative of rain; the moist air causes the low pressure. This is not true to fact. Mr. Dines has recently examined the correlation between the humidity of the troposphere and the pressure at the surface. The coefficient is quite insignificant; there is no relation between moist air and low pressure on the map.

(*To be continued.*)

## *UNIVERSITY AND EDUCATIONAL INTELLIGENCE.*

IT is announced in the issue of *Science* for March 31 that the wills of the late Edith and Walter Scull, niece and nephew of Mr. David Scull, for many years a manager of Haverford College, give 20,000*l.* to the college.

A MEETING convened by the Committee on the Neglect of Science will be held on Wednesday, May 3, at 3 p.m., in the rooms of the Linnean Society, Burlington House. Lord Rayleigh, O.M., will take the chair. A series of resolutions will be submitted to the meeting. Among those who have written in support of the objects of the meeting (many of whom will speak) are:—The Duke of Bedford, Lord Montagu of Beaulieu, the Lord Chief Justice, the Right Hon. Arthur Acland, Mr. Stanley Leathes (Civil Service Commissioner), the master of University College, Oxford, the rector of Exeter College, the master of Christ's, the headmaster of Westminster, the dean of Christ Church, Sir Harry Johnston, Sir Edward Schäfer, Sir William Crookes, Sir William Osler, Sir Ronald Ross, Sir Ray Lankester, Sir William Tilden, Sir Hugh Bell, Sir Robert Hadfield, Dr. Martin Forster, the headmaster of Sherborne, Mr. H. G. Wells, Sir Owen Seaman, and the Poet Laureate, as well as many other leaders in science, education, and industry. Those desiring invitations to the meeting should apply to the Committee on Neglect of Science, 28 Victoria Street, S.W.

WE learn from the issue of *Science* for March 24 that Mr. J. D. Rockefeller, junior, has been re-elected president, and Mr. J. G. Greene secretary, of the Rockefeller Foundation. The capital fund of the Foundation on January 1, 1915, was 20,009,600*l.* Grants amounting to 240,000*l.* not hitherto announced have recently been made by the Foundation. To the Rockefeller Institute for Medical Research 200,000*l.* is given for additional endowment needed in connection with the Department of Animal Pathology; and among other grants, the China Medical Board receives 25,000*l.* for the promotion of medical teaching in China. From the same source interesting particulars are forthcoming of the work of the General Education Board founded by Mr. J. D. Rockefeller to promote education within the United States. Since its inauguration and up to June 30 last the Board had made grants amounting to 3,372,400*l.* The value of the Board's resources is 6,791,800*l.*, and the gross income for 1915 was 446,000*l.* approximately. Among the grants made up to the date mentioned, we notice: for the endowment of universities and colleges, 2,334,500*l.*; for the current expenses of colleges and schools, 31,200*l.*; for salaries of professors of secondary education, 55,100*l.*; and for farmers' co-operative demonstration work, 157,200*l.*

THE approaching retirement of Dr. Lyttelton, the headmaster of Eton, has led to the suggestion that the governors of the college should appoint as his successor a representative of modern scientific learning instead of a classical divine. The usual objections have been raised to such a course, and the usual unenlightened opinions have been expressed as to the association of scientific education with German barbarity. It would be just as illogical to suggest that the war and its instruments of destruction were due to Christian doctrine as it is to assert that science is responsible for them. Science is concerned with the discovery of new phenomena, new forces, new relationships; and men may use them for good or ill—to ease pain and suffering, or to maim and destroy. It produces chloroform as well as chlorine, and enables a wireless call to be sent from a sinking ship as well as makes the explosive for the torpedo or mine which destroyed her. The popular conception of a man of science as a being without human compassion may do for the stage or a penny novelette, but it ought not to be too much to expect people who write to the leading newspapers to know better. We are glad to see, therefore, that the *Daily Mail*, in a leading article on April 22, gives strong support to the claims of science in public-school education. It points out that "clever talking has come to be regarded as almost or quite as important as sound and vigorous action. Precisely the same defect appeared in the later Roman Empire when its education degenerated into a mere study of rhetoric and declamation." Whatever defects we possess as a nation—and they have been unmercifully exposed in the present war—are due, not to science, but to its neglect. It is satisfactory to know that this is at last being realised by the public; and we hope it may be taken for a sign that, whether through a new type of headmasters or otherwise, the education of our future politicians, administrators, and manufacturers shall include general scientific knowledge and scientific method as essential constituents.

## *SOCIETIES AND ACADEMIES.*

### LONDON.

**Zoological Society**, April 4.—Dr. A. Smith Woodward, vice-president, in the chair.—G. A. **Boulenger**: The lizards allied to *Lacerta muralis*, with an account of *Lacerta agilis* and *L. Parva*. This paper is the third and last instalment of a revision of the wall-lizards, of which the first two parts were published in the Transactions in 1905 and 1913. The author has endeavoured to depart from the empirical method usually followed in the arrangement of species, by tracing back the various forms of this difficult group to a hypothetical ancestor of which *Lacerta agilis* appears to be the nearest living representative. The characters of lepidosis and coloration on which his views are based are discussed, and detailed descriptions are given of *L. agilis* and its ally, *L. parva*, the latter being



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