altogether, and in others it would render useless for nomenclatural purposes much original investigation through which genera have been definitely established. Unless rigidly restricted in such a way as to avoid these defects it would be impracticable and undesirable.

N. L. BRITTON.

CLAYTON'S ECLIPSE CYCLONE AND THE DIURNAL CYCLONES.

MR. H. H. CLAYTON, of the Blue Hill Meteorological Observatory, Mass., has published an account of his discussion of certain meteorological observations made during the eclipse of May 28, 1900, in the *Proceedings of the American Academy of Arts and Sciences*, Vol. XXXVI., No. 16, January, 1901; and the full report in Vol. XLIII., Part 1, of the *Ann. Har. Coll. Obs'y*, 1901. Mr. R. DeC. Ward reviews these papers in SCIENCE of March 1, 1901, and says, "Clayton has gone far ahead of all previous investigations of the phenomena of eclipse meteorology. The low temperature, the circulation of winds and the form of the pressure curve all proclaim the development by the eclipse of a *cold-air* cyclone, as described by Ferrel; * * * The fall of temperature due to the occurrence of night must also produce, or tend to produce, a cold-air cyclone. Since the heat of day produces, or tends to produce, a warm-air cyclone; * * * These causes must, in the opinion of the author, produce entirely, or in part, the well-known double diurnal period in air pressure; * * * His explanation of the diurnal variation of the barometer seems to have in it many evidences of being the best yet offered to account for this puzzling phenomenon."

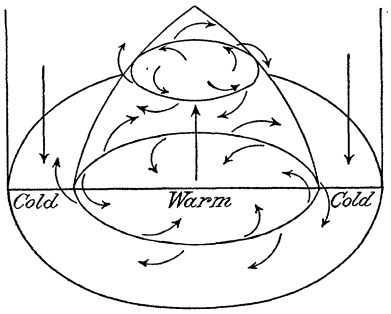
I suspect that Mr. Clayton and Mr. Ward have an incorrect conception of Ferrel's cold-center cyclone, or else they could hardly have written about it the remarks contained in these papers. The subject is rather complex, but I hope, very briefly, to indicate the leading discrepancies for the benefit of others who think that the problem of the diurnal variation of the barometer can be solved along these lines.

1. *Some Minor Errors.*—Clayton's Formula, page 8, $\tan\theta = \frac{\Sigma(\sin o)v}{\Sigma(\cos o)v}$ should be written $\tan\theta = \frac{\Sigma v.\sin o}{\Sigma v.\cos o}$, where o is the azimuth of the observed wind, and θ that of the mean or prevailing wind; but this function between θ and o is incomplete and it often gives an incorrect result. In the diagram the wrong diagonal is drawn, and this introduces confusion into the exposition of the formulæ for determining C, used in $\theta^1 = \theta \pm 180 \pm C$, where θ^1 is the azimuth of the eclipse wind which is required. As the eclipse wind for Washington, Ga., Wadesboro, N. C., and Blue Hill were computed by these formulæ, the results must be imperfect. The observations were themselves not very satisfactory, as is inferred from the account of them.

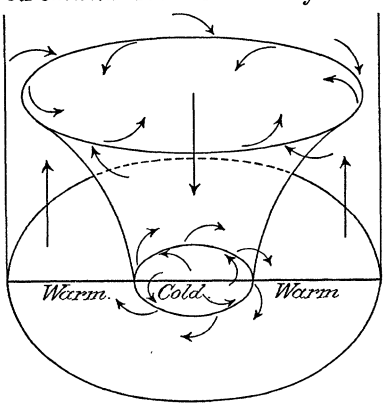
2. *The Cold-Center Cyclone.*—Ferrel's account of the cold-center cyclone is found in the Report of the U. S. Coast Survey, 1877, Appendix No. 20, page 187; in the Report of the Chief Signal Officer, 1885, Appendix 71, page 257; and in other places. Also there are some general remarks on the subject in the International Cloud Report, 1898–99, page 615. The accompanying diagrams show the circulation in the warm-center and cold-center cyclones, respectively, and the distributions of pressure characteristic of them; these must serve for further explanations in this place. In order that there may be no doubt about Ferrel's idea of the cold-center cyclone, I quote from the Report of the Chief Signal Officer, page 257, "The gyrations at the earth's surface must be in the same direction as in the case of ordinary cyclones; * * * The interchanging motion is from the center below and toward it above." Report of Coast Survey, page 188, "The maximum barometric pressure is where the gyrations are reversed; * * * The pressure is a minimum at the center and a maximum at the edge" of the cold-center cyclone, at all altitudes, meaning by edge the locus where the gyrations reverse direction.

Ferrel illustrates the cold-center cyclone in these two reports, also in his popular treatise on the winds, pages 246–247, 337–342, by comparing it with the general circulation over a hemisphere of the earth, where the poles are cold and the tropics warm, and states that the circulation is the same in each. That is, the air descends at the pole, flows south and *east*

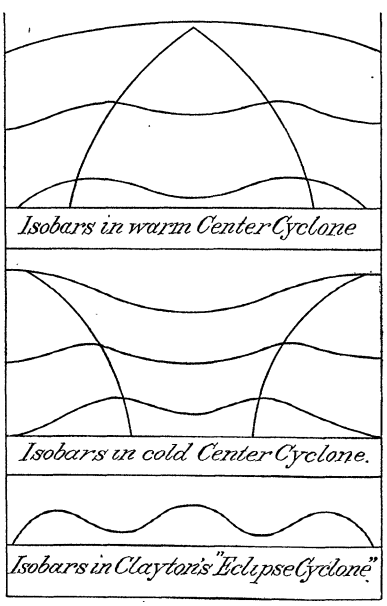
(not west) to latitude 30°, thence south and *west* (not east) to the equator, in which region it ascends, etc. Thus, page 337, "This vertical circulation, as in the case of the ordinary cyclone, gives rise to a cyclonic motion in the interior (that is, to the place where the gyration reverses from $+v$ to $-v$) and an anti-cyclonic in the exterior part." Cyclonic rotation is against the hands of a watch, looking down upon it, and anticyclonic is with the hands, so that in warm and in cold-center cyclones the rotation is cyclonic near the center, and anticyclonic beyond a certain circle of reversal. In the warm-center cyclone the motion is ascending and inward near the surface in the interior and descending in the exterior; in the cold-center cyclone these motions are reversed, but the *direction of rotation about the pole is not changed.* Ferrel's formulæ, diagrams, and discussions agree in this throughout his works; indeed, there could be no other interpretation of them on mechanical principles. He says, C. S. O., page 203, "Such areas of high barometer (referring to the high pressure area produced by the overlapping of two adjacent cyclones) are usually called anticyclones, and the air does in some manner move around them in a direction contrary to that of a cyclone, but this does not arise from a central area of greater cold, for it has been shown that such a condition *would give rise to a cyclone and not to an anticyclone*, and that the latter would be entirely at variance with fundamental and well established principles of mechanics."



Circulation in warm Center Cyclone.



Circulation in cold Center Cyclone



When the temperature gradients are very steep and there is much friction on the surface, a small secondary maximum pressure may develop in the center of the cold-center cyclone. In the case of the eclipse the temperature gradients were feeble, and the wind velocity v small, upon which the friction-force $= k.v$ depends, so that it could not happen under the circumstances of the eclipse phenomenon.

Now by comparing Clayton's diagram, Fig. 5, Plate II., for the wind direction, we find a *true anticyclonic* configuration at the center, and this rotates in the opposite direction to that required in the cold-center cyclone; if wind stream lines were to be drawn from the center outward at *right angles* to those contained in the Clayton diagram, we should then obtain a cold-center cyclonic circulation,

but this is just the opposite of the result of the eclipse observations. Furthermore, Plate III., 'Pressure in Eclipses,' shows us the variations in pressure as observed, and these are reproduced in the lowest section of the adjacent diagram. It is seen that the pressure in the eclipse is *reversed throughout its length to that required in the cold-center cyclone.* The Weather Bureau observations at sixty-five stations confirm the Clayton distribution of pressure, but the conclusion is also unavoidable that we are not dealing with a cold-center cyclonic circulation. My further remarks on this subject will be found in the forthcoming report by the Weather Bureau.

3. *The Semidiurnal Cyclones.*—Even if the analogue between the eclipse circulation and that assumed to exist in the nocturnal circulation were not hopelessly in error, there are yet other difficulties to be considered, even supposing the cooled hemisphere of the atmosphere were to produce something like a cold-center cyclone and the heated hemisphere a warm-center cyclone. (*a*) The vertical circulation in the cold-center and in the warm-center cyclones are in *opposite directions* to one another at the belt of highest pressure, so that they would tend to destroy each other, rather than build up any high pressure belt such as the diurnal pressures require. (*b*) If there is a cyclonic circulation of any kind, the conditions demand that the pressure be distributed quite symmetrically on all sides of the center in order to maintain a true gyratory motion; but in the diurnal barometric pressure there are found to be simply two peculiar waves extending from pole to pole, which do not in the least form such a symmetrically distributed pressure about a center. Compare Int'l Cloud Report, Chart 44. (*c*) According to Clayton, Plate IV., the high pressures at the surface shift about 90° in the higher strata. But so far as I know, the only data bearing upon this point are contained in the Blue Hill hourly cloud observations, and these show that the *same diurnal circulation* exists in the atmosphere from the surface to the cirrus level. However, this may be a doubtful point and I will not press its accuracy. Compare Int'l Cloud Report, Chart 45.

I have generally found myself in accord with Mr. Clayton's published results, and I am sorry to be obliged to dissent from his interpretation of the eclipse observations in this case. The variations of pressure in the eclipse are not larger than 0.01 inch of mercury, and the velocity of the eclipse wind is only 2 or 3 miles; but it would seem hardly credible in the face of these facts that this small atmospheric disturbance should set up any true cyclonic circulation over an area 5,000 miles in extent, as is claimed in the report under consideration.

FRANK H. BIGELOW.

WASHINGTON, D. C., March 20, 1901.

THE REVERSAL OF THE PHOTOGRAPHIC IMAGE BY CONTINUED ACTION OF LIGHT.

THE remarkable results described by Professor Francis E. Nipher, in developing photographic plates in daylight, bring to mind some of the earlier experiments upon the reversal of the photographic image. It has long been known that under particular conditions of over-exposure in the camera a positive, instead of a negative, is produced by ordinary development. This result has been repeatedly observed by amateurs, much to their astonishment and mystification.

The present writer has several times attempted to bring about the effect by prolonged exposure in the camera, but without success. The necessary conditions not being known, the result is accidental and uncertain. The idea of giving a supplementary exposure of the plate in broad daylight did not suggest itself; indeed, it is not one that would spontaneously commend itself to a photographer. All his previous training and experience is opposed to it on general principles.

Nevertheless, it is not entirely new. Herschell, in the year 1839 or 1840, did very much the same thing. He observed reversals of photographic action, and so did Draper on strips of sensitized paper with which he was studying the chemical action of the sun's light in Virginia, and photographing the spectrum in ephemeral colors. This subject was referred to quite recently in an article by the present writer, entitled, 'Tithonic Rays and Early