SECTION I.—AEROLOGY.

REMARKS ON THE NATURE OF CYCLONES AND ANTI-CYCLONES.

[*Communicated to the International Meteorological Congress at Chicago, Ill., August, 1893.*][1]

By Prof. Dr. JULIUS HANN, Director.

[Dated: K.k. Zentralanstalt für Meteorologie und Erdmagnetismus, Vienna, May, 1893.]

[Those not already familiar with the "convection theory" of the origin and maintenance of cyclones and other whirling storms as set up by Espy and Ferrel, will find it expounded in the MONTHLY WEATHER REVIEW, April, 1906, 34: 164–165.—C. A.]

A distinguished American meteorologist, *William Ferrel*, has devised and developed the foundation of a theory of cyclones. It gives me pleasure to know that it was my privilege (1) to be the first to make Ferrel's theory generally known in Europe in the German language. This theory deals principally with the laws of the movement of the atmosphere in the complete well-developed cyclone; the origin of cyclones, i. e., the source of the energy needed for the movement of the air in the cyclone is treated but incidentally or not at all in Ferrel's first memoirs. Subsequently, Ferrel introduces Espy's views to explain the origin of the cyclone and in the last years of his life he almost passionately defended the so-called "convection theory" of cyclones although, so far as I can judge, it lay far from his original ideas which were based upon the general circulation of the atmosphere for the theory of which we also have to thank him.

The "convectional theory" of cyclones, especially as presented by Reye (2) made on me also at first a great impression that one can indeed hardly escape from. By it a large group of phenomena is referred back to simple physical laws so clearly that one experiences a lively sense of satisfaction well expressed by Ferrel when he calls the "convection theory" a very beautiful and satisfactory theory.

But after the first incisive impression of this theory has given place to a more quiet consideration, and as soon as one turns to the facts and inquires how they harmonize with the theory, he soon finds that the totality of the phenomena of our whirlwind storms is not to be brought into conformity with the "convection theory." The great cyclones of the tropical oceans seem in most cases to find a satisfactory explanation in the "convection theory," as indeed a thorough study of the cyclones of the Bay of Bengal has led the most accurate of the new students of these storms, Blanford and Eliot, to be its enthusiastic advocates. As regards the origin of these cyclones, however, I can not share in the views of these prominent investigators, as I have expressly stated in the Zeitschrift für Meteorologie (3) where I have expounded certain views as to the origin of the cyclones of the Bay of Bengal based directly upon the admirable works of these Indian meteorologists.

The meteorologists of India, as is well known, have arrived at the opinion that agrees with the principle first developed by Espy and subsequently adopted by Loomis, Buchan, Mohn, etc., that the cyclones of the Bay of Bengal owe both their origin and their continuance to the process of condensation and the precipitations that accompany them.

As opposed to the idea that precipitation can produce barometric minima I have, immediately on the appearance of Reye's book, set forth the most serious and, as it seemed to me, most important objections (4). In fact all phenomena when they are examined impartially, as well as theory itself, speak against the origination of a barometric depression by precipitation. Even so active a defender of the so-called "condensation theory" as Mr. Eliot could not refrain from the remark that the immense summer precipitation at Cherrapunjee seems to have no influence on the pressure, that indeed the effect of the heaviest rainfalls is in general rather an increase than a decrease of the atmospheric pressure (5), exactly as I have already demonstrated (6) for Batavia, Java. In regions near the Equator where, on account of the absence of the deflection of air currents by the earth's rotation, no great atmospheric whirl can arise, the heaviest and most extensive precipitation remains without any influence on the barometric pressure. If no great atmospheric whirls exist then there is no local deep barometric depression. In one of his first pioneer works Ferrel pointed out that the mean value of the nonperiodic barometric fluctuations increases with the geographic latitude and in fact nearly in proportion to the square of the latitude, while the deflecting force of the earth's rotation is proportional to the sine of the latitude. Ferrel (7) showed that the observations agreed very well with this theory. The deep depressions in the storm areas are a consequence of the whirling movement of the mass of air and *not* of the precipitation that occurs as a part of the train of phenomena of the whirl. No one has demonstrated this more clearly than Ferrel himself. The fact that the whirl and the barometric depression caused by it, are almost invariably connected with more or less abundant precipitation, is the natural consequence of the ascending movement of the air in the whirl. Wherever air ascends it cools and the associated aqueous vapor is partially condensed, this is one of the simplest results of generally known physical laws. Therefore, where large whirls arise a tendency to precipitation must occur. To conclude that cyclones, especially those of the tropics, arise from the accompanying heavy precipitation, that the latter is the cause of the barometric depression and of the whirl itself, is therefore an evident confusion of the effect with the cause.

In order to avoid misconceptions and to protect myself against the suspicion of adopting a narrow standpoint, I would right here remark that in my opinion, also, the precipitations that are introduced [into the depression] by the ascending whirls of air favor and further the maintenance and partly also the increase of the ascending movement of the atmosphere in the whirl. Since in consequence of the condensation of aqueous vapor and the latent heat thereby set free in ascending masses of moist air, the dynamic cooling is diminished, therefore the ascensional force of the masses of air in the interior of the whirl is assisted and the eventual lateral overhead

[1] This paper was written in May, 1893. It was translated by the Editor and prepared for publication in December, 1901, but has been delayed for the reasons stated in this REVIEW, February, 1914, 42: 93.

outflow of the ascended air is made easier. This favors the continuation of the whirling movement when it is once established.

The "convection theory" of cyclones places the fundamental conditions of their existence in the lower strata of air, by whose relatively high temperature and large content of aqueous vapor the ascending movement is started. Then the latent heat of condensed aqueous vapor and the deflecting force of the earth's rotation come into play and carry the work further. The "convection theory" seeks the cause of the origin as well as the direction of progress of cyclones principally in local conditions, in the characteristics of the lower strata of air at or near the earth's surface.

If we transfer the fundamental originating conditions of vortices to the upper strata of the air—as frequently has been done of late by defenders of this theory because it is no longer possible to ignore the convincing force of the facts and considerations against the origination of whirls in the lower strata—then in fact the "convection theory" of storms is fundamentally abandoned.

If, with Ferrel in his latest publications, we locate the seat of the cyclone at an altitude of about two miles or 3,200 meters (8) then [we remark that] all the physical bases of the "convection theory" fail at this altitude. At a temperature of about −10° or −20° C., such as prevails at this altitude, especially in winter when the cyclones are most frequent and intense, and in middle latitudes (say 40° in the United States and 48° in Europe), even air saturated with aqueous vapor contains but 1 or 2 grams of aqueous vapor per cubic meter, and the rate of diminution of temperature with altitude in this mass when it is ascending can scarcely be distinguished from that of dry air. The vertical temperature gradient even at the beginning of the ascent amounts to 0.8°C. or 0.9°C. per hundred meters, whereas, even in perfectly dry air, as is well known, it amounts to about 1°C. per 100 meters. If now the air has ascended but 2,000 meters in the whirl then it must have cooled to −36° or −40° C. and its aqueous vapor has practically all condensed. Therefore a whirlwind corresponding to the "convection theory" formed even under the most favorable conditions at such altitudes as 3 kilometers or more, can attain no considerable intensity and in no way have the power that Ferrel assumes, to pump up the lower cold air strata 3,000 meters deep, by its suction or to set them in rotation by means of friction. Even in the moist, warm, marine climate of England's summer, according to Glaisher's balloon observations, the quantity of aqueous vapor in the air at altitudes of 6 or 7 kilometers is infinitesimal.

Therefore the "convection theory" of cyclones is forced to locate the place of origin and development of the great atmospheric whirl in the lowest strata of the atmosphere, because these alone hold so great a quantity of aqueous vapor that by its condensation the dynamic cooling of the ascending air in the whirl is so far diminished that one may suspect that the whirling mass of air has a higher mean temperature than that of the surrounding atmosphere. For this latter condition is a necessary assumption of the "convection theory." The mass of air in the cyclone must to a certain extent have a higher temperature than its surroundings; it must experience an uplift [due to buoyancy]. This assumption is certainly best fulfilled by the great cyclones of the Tropics, for example, by those of the Bay of Bengal that originate during the transition period from one monsoon to the other. In the cyclones of middle and higher latitudes this assumption seems to be inappropriate; at least it does not appear so regularly and to such an extent as that a theory of cyclones can be based upon it. The cyclones of the Bay of Bengal, that occur about the time of the change of monsoons (May and October) may originate in the lower atmospheric strata. The two following circumstances favor this view. These cyclones form above the Indian Ocean, generally in the southeastern part of the Bay of Bengal and dissolve rapidly as soon as they reach the land. This characterizes them as formations that are located principally in the lower strata of the atmosphere. In support of Faye's theory of waterspouts and tornados, Hirn very properly calls attention to the difficulty of assuming that powerful whirls arise in the lower layers where the movement of air once initiated toward a center is greatly enfeebled and will certainly soon be destroyed by friction even before an intensive whirl can be formed. For it is only when large masses of air are drawn from a distance into the whirling movement that a powerful whirl can develop in a previously quiet atmosphere. But this is not properly conceivable in the lower strata of the atmosphere above land surfaces, although one may without serious contradiction assume it for the cyclones of the Bay of Bengal. The whirls of the "convection theory" must break up and dissolve when they encounter high mountains, both for mechanical reasons and because they derive their source of energy from the lower strata of the atmosphere, but lose their vital principle in the higher, cold, vaporless strata. Precisely this does occur with the great cyclones of the Bay of Bengal; they generally come to an end as soon as they reach the land, and notwithstanding their extraordinary intensity are unable to pass over even the relatively low mountain chains of the Ghats, the Tipperary Hills, etc. In this respect these cyclones in fact show properties that we must assume to characterize the whirls of the "convection theory."

The cyclones of the season of the southwest monsoons, on the contrary, are of much feebler intensity, but pass entirely across southern India and show all the characteristics of the whirlwinds of higher latitudes, as clearly follows from the thorough investigations of John Eliot (9). In the upper strata there occur whirls that move with the general currents of the air and that certainly can not be explained as to their origin and nature by the "convection theory" alone. These are, like the cyclones of our latitudes, whirls in a general great air current from which they derive the principal part of their energy and which in general determines their progressive movement. Of course the "convection theory" has a part to play in this process, as indeed could not be otherwise in consideration of the high temperature and the abundance of vapor in the atmosphere in these latitudes and the season of the year (June and July).

The fact that the cyclonic storms of middle and higher latitudes frequently pass unhindered over mountain ranges several thousand meters high proves that, in these cases at least, the true seat of the atmospheric whirl is to be sought at far greater altitudes. At such altitudes, however, the "convection theory," as before explained, loses its applicability, since the physical forces themselves on which it is based can no more be effective.

In general, the method of progression of the large atmospheric whirls is in contradiction to the "convection theory," which demands a much greater dependence on the local meteorological conditions than is actually the case. The direction of progress of the whirl shows a much greater dependence on the general air currents in the upper strata of the atmosphere than on the temperature and moisture conditions of the lower strata of air. The "convection theory" must needs attach great weight to the rapid vertical temperature decrease in the regions frequented by the paths of cyclone centers. A more or less unstable vertical equilibrium in the atmosphere extraordinarily favors the continuance and intensity of the whirls of the "convection theory." But in those regions over which they move these whirls strive to restore the stability of the vertical equilibrium in the atmosphere; the lower air strata are cooled, the higher strata warmed, the differences of temperature between the upper and lower are diminished. At the same time the aqueous vapor of the lower strata is condensed, the potential energy accumulated in the heat of the higher strata and in the greater vapor content of the lower strata is used up, and a new precipitation can not be at once initiated simply by ascending convection currents. Therefore cyclones of the "convection theory" must either avoid advancing over those portions of the earth's surface where a cyclone was active shortly before, or they must dissolve there.

Now, in fact, the smaller whirls that give rise to the so-called heat thunderstorms of the European summer do show this peculiarity and thereby they show themselves to be phenomena to which the pure "convection theory" may find full application. On the other hand, the greater atmospheric storms have the peculiarity of readily following at short intervals along the same path. They not only do not avoid the path of the preceding whirl but they favor it. This fact has already frequently been demonstrated. Even Doberck in his "Law of Storms in the Eastern Seas" says: "It is a well-known fact that barometric depressions are drawn toward those regions over which another depression has just passed." Now, this latter peculiarity of the greater atmospheric whirls is in complete contradiction to the "convection theory." Rather does this fact show that this theory plays only a subordinate role in the mechanism of these whirls and that the forces upon which the greater atmospheric whirls depend primarily for their origin and progress are not to be sought for *within* but *outside* of these whirls. This peculiarity undoubtedly indicates that it is the general relation of the distribution of atmospheric pressure and the disturbances in the general atmospheric circulation, to which the origin and the progression of our storm whirls must be attributed.

An additional fact that stands in notable contradiction to the "convection theory" explanation of our storms is the annual periodicity of the frequency and intensity of the extratropical cyclones. If the "convection theory" were really applicable to the majority of extratropical cyclones, then they should attain their maximum frequency and intensity in the summer season—but the facts are directly opposite to this. How can the "convection theory" explain that these storms attain their greatest intensity and frequency in the winter half of the year, in that season when the conditions of both origin and continuance are of all others most unfavorable? In winter the vapor-content of the air is very small and the vertical thermal equilibrium of the atmosphere extremely stable. Over the continents the lower strata of air are, at that season, not infrequently colder than the strata above—the temperature increases upward. The vertical temperature decrease is slower than in ascending air currents, even when these become saturated with aqueous vapor by the lowering of the temperature. Ascending masses of air, therefore, experience no uplift by reason of colder surroundings. How can one assume that under such conditions the atmospheric whirls of the "convection theory" can penetrate to the interior of Siberia where the aqueous vapor in the air is reduced to almost absolutely nothing at atmospheric temperatures of −30° to −40° C.?

And why are the great atmospheric whirls relatively rare and much less intensive in summer, when every condition for their origin and continuance is so much more favorable, if they really can be explained by the "convection theory"? The fact that in the course of their annual periodicity cyclones attain their maximum of frequency and intensity in the winter season, does therefore stand in very decided contradiction to the "convection theory" of storms.

On the other hand, this fact is in complete agreement with the view that the great atmospheric whirls derive their existence and their energy from the upper general currents of air which control the circulation of the atmosphere between the equatorial and the polar regions. The energy of these upper air currents is greatest in the winter season when both the temperature gradient and the barometric gradient of the upper and uppermost atmospheric strata between the Equator and the poles are greatest. Ferrel has shown that in the Northern Hemisphere the temperature gradient between the pole and the Equator is twice as great in January as in July; and that the corresponding upper west-to-east current must theoretically have from 2 to 4 times greater velocity in winter than in summer.

Therefore, when we assume that the great atmospheric whirls are to be considered as disturbances, so to speak, in the great currents circulating between the Equator and the poles or are dissolved by them, we at once explain the annual period of their frequency and intensity. The upper-air poleward pressure gradient is much steeper in winter and causes a much stronger atmospheric circulation. Therefore, if disturbances of the dynamic equilibrium occur, the forces thereby respectively annulled or roused to action must be much more powerful in winter than in summer. Möller has shown most thoroughly (10) that the rapidly moving upper layers of air must frequently produce a condition of unstable dynamic equilibrium that then leads to the formation of cyclones and anticyclones.

The movement of fluids in streams never goes on as "steady motion" but always partially resolves itself into whirls. *It is certainly not to be imagined that the powerful and rapid upper currents of air should flow from the Equator to the pole without the formation of whirls.* Helmholtz, also, has suggested that the reason why extraordinarily great velocities, such as the masses of air flowing from the Equator to the pole must attain in the higher latitudes according to the law of conservation of areas, actually do not appear, is to be sought in the fact that vortices must arise which absorb or dissipate a great part of the energy. Therefore the source of the energy of our storms need not be the first thing to be sought after.

If, now, many other facts also show that the atmospheric whirlwinds of the middle and higher latitudes must originate in the upper layers of the atmosphere, and if we note how simply the phenomena observed in the storms of our latitudes can be explained when we refer them back to their origin—then it is certainly not evident why we do not drop the useless effort to explain them by the "convection theory" and apply the latter to those phenomena only that it actually can most naturally explain.

In most recent times [i. e. about 1893] another class of observations has come to join the previous array of facts that testify most decidedly against the explanation of the storms of our latitude by the "convection theory." I mention this class in the last place intentionally, in order to show that the "convection theory" of the cyclones of middle and higher latitudes can not be saved even by the attempt to deny the importance of these observations.

Moreover these facts are of higher interest because they show the most beautiful agreement with the physical theory of cyclones.

It is well known that all observations agree in showing that in cyclones the air is ascending while in anticyclones, on the contrary, a descending current prevails. Cyclones and anticyclones are the two members of the vertical circulation of the atmosphere. In the ascending current of the cyclones there occurs dynamic cooling. In this process the aqueous vapor is condensed at a certain altitude, and this diminishes the rate of cooling in the ascending masses of air. The rate of the cooling, in the adiabatic expansion of *moist* air, lies between the limits 0.5° and 0.9°C. per 100 meters in our latitude. On the other hand, in the descending branch of the vertical circulation the air warms up and therefore can not form any precipitation; on the contrary, the air becomes relatively very dry. The rate of increase of temperature in descending air amounts to about 1°C. per 100 meters. If, therefore, we assume a closed vertical circulation of air in cyclones and anticyclones, such that the air ascending in the cyclone again sinks to the earth in the surrounding region, then theory shows that in the ring of descending air with high pressure at the earth's surface, the temperature of the air is higher than in the ascending current of the cyclone with a barometric depression at the earth's surface. Such formations can, of course, only be produced dynamically, in consequence of the general circulatory currents between the pole and the Equator, or generally in the general currents of air of greater extent and energy, as for example within the monsoon winds.

The "convection theory," on the contrary, assumes that the mass of air constituting a cyclone has a higher average temperature than the air in its neighborhood. It is precisely the buoyancy experienced by the specifically lighter warmer air in the ascending whirl that alone explains the origin and continuance of the cyclone according to the "convection theory."

Therefore the observations of temperature up to great altitudes in the region of cyclones and anticyclones should decide whether the cyclone and the anticyclone are at least in part purely dynamic formations or can be explained by the "convection theory."

The temperatures registered at the higher mountain stations in the Alps, on the Pic du Midi, etc., have recently made it highly probable that the great mass of air in the cyclone, at least up to altitudes of 3 or 4 kilometers, actually has a lower temperature than the air mass of the anticyclone (11). This fact speaks decidedly against the "convection theory," but stands in complete accord with the theory that ascending currents of air in general must have a lower temperature than the descending masses as in the anticyclone.

Although these observations hold good directly only up to 3 or 4 kilometers in altitude, still the absence of observations from greater altitudes does not favor the defenders of the "convection theory." The whirls of this theory must draw their energy from the lower layers of air that are richer in moisture; the theory has no longer any application at altitudes of 4 kilometers or more, because the whirls of the "convection theory" are well nigh impossible in this region of little vapor.

As to the temperature that prevails at very great heights (above 3 or 4 kilometers) in the descending masses of air of the anticyclones, this can only be decided by observations in balloon ascensions. Theoretically, this descending air warms at the rapid rate of 1°C. per 100 meters, but the actual temperature of the air depends upon how rapidly the process of descent takes place and how great is the corresponding loss of heat by radiation. Cleveland Abbe (12) has given very interesting computations and conclusions on this point, but the temperatures determined by direct observations in anticyclones up to altitudes of 4 kilometers are more decisive in reference to the theory of cyclones.

Moreover, the ascending air within the cyclones experiences a very considerable cooling in contrast with the computed theoretical rate of vertical temperature decrease. This is the cooling due to the falling products of condensation, namely, the rain, hail, and snow. This lowering of the temperature of the body of air of cyclones by the falling rain and snow is very important. In the barometric Low of July 12, 1890, that moved from the Gulf of Genoa northeastward and partly over the Alps, the air was so greatly cooled that thick snowfall prevailed down to an altitude of 600 meters above sea level and the temperature at altitudes of 400 to 500 meters fell to 4° or 5° C., and this in the middle of July. At an altitude of 3,100 meters the temperature was −5.3°, or about 6° below the average. It appears to me very probable that the air ascending in the region of cyclonic precipitation is cooled by the falling products of condensation more than the air descending in the anticyclones is cooled by the radiation of heat.

I may finally refer to (13) my "Remarks on the Temperature in Cyclones and Anticyclones;" also to my "Studies on the Temperature and Pressure of the Air on the Summit of the Sonnblick."

REFERENCES.

(1) **Hann, J.,** *in* Meteorol. Ztschr., Wien, 1875, **10:** 81–88, 97–106.

(2) **Reye, Theo.** Die Wirbelstürme, Tornados, und Wettersäulen in der Erdatmosphäre. Hannover. 1872. xvi, 248 p. 8°.

(3) **Hann, J.,** *in* Meteorol. Ztschr., Wien, 1877, **12:** 308; 1880, **15:** 313.

(4) **Hann, J.** Bemerkungen über die barometer Minima in den Sturmcentren. Meteorol. Ztschr., 1873, **8:** 102.

Der Einfluss des Regens auf den Barometerstand. Meteorol. Ztschr., Wien, 1874, **9:** 289.

Luftdruck und Regen. Meteorol. Ztschr., Wien, 1875, **10:** 11.

(5) **Eliot, John.** Quarterly jour., Royal met'l. soc., London, No. 41, January, 1882, **8.**

(6) **Hann, J.** *in* Meteorol. Ztschr., Wien, 1874, **9:** 291. The American reader will find some of these papers *translated in*

Abbe, C. Short memoirs on meteorological subjects. Ann. rpt., Smithson. Instit., 1878. Washington.

(7) **Ferrel, W.** *in* Amer. jour. sci., New Haven, Conn., Nov., 1874, (5) 8; and Jan. 1861, (2) **31.**

(8) Möller, in order to more surely overcome certain difficulties, thinks this altitude might be increased to 5 or 10 kilometers.

(9) **Eliot, John.** Account of the southwest monsoon storms generated in the Bay of Bengal during the years 1877 to 1881. Indian met'l. mem., Calcutta, 1885, **2,** pt. 6. See particularly p. 437, ffg.

(10) **Möller,** *in* Meteorol. Ztsch., Wien, Feb., 1891, **8:** 72. Compare also the presentation by Köppen in "Humboldt," December, 1888.

(11) **Hann, J.** Das Barometermaximum vom November 1889. Denkschr., Wiener Akad. d. Wissensch., 1880, **57.**

Hann, J. Resultate des ersten Jahrganges der meteorologischen Beobachtungen auf dem Sonnblickgipfel. Studien über die Luftdruck- und Temperaturverhältnisse auf dem Sonnblickgipfel nebst Bemerkungen über derer Bedeutung für die Theorie der Cyclonen und Anticyclonen. Sitzungsber., Wiener Akad. d. Wissensch., Jänner, 1888, **97A;** April, 1891, **102A.**

I first demonstrated the relatively high temperature of the air in the region of the anticyclones in my memoir "Ueber das Luftdruck Maximum vom Januar, 1876." Met. Ztschr., Wien, 1876, **11:** 129, ffg.

(12) **Abbe, Cleveland.** American journal of science, New Haven, 1892, (3) **43:** 364.

See also **Hann** in Meteorol. Ztschr., Wien, 1892, **27.** Jhrg.: 259.

(13) **Hann, J.** Bemerkungen über die Temperatur in den Cyclonen und Anticyclonen. Meteorol. Ztschr., Wien, 1890, **25.** Jhrg.: 328–344, 457.

Hann, J. Studien über die Temperatur- und Luftdruckverhältnisse auf dem Sonnblickgipfel. Sitzungsb., Wiener Ak. d. Wissensch., April, 1891, **100, IIA:** 40–51.