Dr. Hann's Studies on Cyclones and Anticyclones.

PROFESSOR FERREL'S letter in *Science* of Dec. 19, commenting on mine of May 30, closes with the suggestion that I should make further statement of the matter of Dr. Hann's studies, which I do with pleasure.

The best reasoned general account of the convectional theory of cyclones and anticyclones (by the latter term I mean areas of high pressure) that I know of is given in Professor Ferrel's "Popular Treatise on the Winds." Of various statements in regard to cyclones, the following may be quoted from the concluding paragraph on their vertical circulation: "The greater temperature of the interior [of cyclones] causes an upward expansion of the air and greater vertical distances between the isobaric surfaces here than in the exterior part where the temperature is less" (p. 241). In regard to anticyclones or areas of high pressure, of the kind that Dr. Hann has investigated, the following explanation may be quoted: "The principal cause of the large areas of very high barometer which frequently occur in the higher latitudes in winter is undoubtedly found in the clearness of the atmosphere over these areas and the intense coldness produced by the radiation of heat at a time when little is received from solar radiation. The density and pressure of the air are much increased from this cause, and the areas are too large and irregular for this disturbance to give rise to a cyclone with a cold centre" (p. 345). The inversion of temperature accompanying such areas of high pressure is referred to on the next page, but still with the implication that the mass of air in the anticyclone is cooled below the temperature of the surrounding atmosphere, and therefore that it descends and flows out at the base by gravitative convection.

These quotations might be further extended, but they suffice to show that the essential of the generally accepted theory of the areas of low and of high pressure which appear so frequently on our weather-maps is that the first are relatively warm, and the second are relatively cold, when compared with their surroundings. Cyclonic and anticyclonic areas are both of common occurrence, and therefore as a rule their temperatures should be respectively above and below the normal temperatures of their time and place.

Records of temperature made on high mountain-peaks furnish the best means of testing the convectional theory of cyclones; for, even if all other tests were successfully borne, failure under this test would be fatal to the theory. Dr. Hann's essay on the anticyclone of November and the cyclone of October, 1889, as observed in the Alps, furnishes the best means of applying this test that has come to my knowledge. It is true that one example of each of these phenomena is not sufficient for final determinations, and it is very apparent that the results would be far more convincing if they included records from mountain stations scattered over a much larger area than that of the Alps. Surely no one will be more careful to supplement these deficiencies, whenever possible, than Dr. Hann himself.

I do not see any reason for believing that the anticyclone that stood over the Alps in November, 1889, was exceptional in its nature or in its relation to the surrounding atmosphere. All of its features except its mean temperature warrant the belief that it was a typical example of the phenomena referred to under the heading of "Areas of High Pressure" in Professor Ferrel's treatise. Unless it can be shown to have been of exceptional nature, the abnormally high temperature of its air mass is a direct contradiction of the fundamental idea of the convectional theory of areas of high pressure. It has not been claimed that the conditions of a cyclone exist in this high-pressure area; but the explanation of high-pressure areas as quoted above is a direct corollary of the cyclonic theory. If the corollary is contradicted by facts, the theory needs revision. The burden of proof in this case lies with those who would maintain that the anticyclone in question was of so exceptional a nature that it cannot be regarded as a representative of its class. Its long duration does not show it to be a thing of another kind from other areas of high pressure: the long duration merely gave good opportunity for repeated observation of its prevailingly high temperature.

As to the cyclone of October that was examined by Dr. Hann, it was certainly of moderate development; but it was as good an example, according to Dr. Hann, as he could find. The observations that he quotes show that its general central temperatures were below the normal of its time and place. The fact that the temperatures were not determined in the free air, but at stations on the surface of the ground, does not seem to me to invalidate their use here; for on the peaks where the critical observations were made the air is generally in motion, and the mass of the mountain is small; and for both these reasons the control of the temperature of the air by the ground is not great enough to explain the reported low temperatures. Over a broad surface of a lowland, where the wind is weaker and the opportunity for contact of air and ground is greater, the case is different. The low temperature of the central part of this cyclone may fairly be regarded as contradictory to the convectional theory of cyclones, unless it can be shown that the example in question was surrounded by air more abnormally cooled than its own, or unless it is shown to have been an expiring cyclone,—one whose long circulation had so thoroughly exhausted its supply of warm, moist air, and so successfully warmed the surrounding air, that it had no further support, as Professor Ferrel has shown might sometimes be the case. It is true that Europe might offer more examples of self-exhausted cyclones than occur in this country, for they are there advancing from moister into dryer regions; but it is difficult to believe that so considerable a deficiency of temperature as probably occurred in the case under consideration should be produced before the cyclonic motions had stopped, if they depended entirely on a convectional origin. It is not likely that so exceptional a case as this must be, if it is to be explained by convection, would have been the very case that Dr. Hann happened to choose for his studies. It is still more unlikely that both the cyclone and the anticyclone here referred to should have been exceptional members of their classes, both departing from the normal in a way that would contradict the convectional theory. As these are the first examples of their kind to be carefully examined by means of regular observations at stations at so high a level, the probability is strongly in favor of their being ordinary, and not extraordinary, phenomena; and as such they did not possess the peculiar temperatures that the convectional theory would lead us to expect. Although mere probability of this kind does not close a case, it seems to me that it may be fairly said to open it.

I do not see that there is any necessary contradiction in this discussion. The theories under consideration are not mutually exclusive. Both may be true. The liberation of latent heat from condensed vapor is an aid to the circulation in both cases. Certainly there is nothing in Dr. Hann's essay to make one think that thunder-storms, tornadoes, and desert whirls are not convectional phenomena. It is entirely possible that true convectional cyclones might prevail in the tropics, while driven cyclones might characterize the temperate zones. A cyclone begun chiefly by one process might be continued chiefly by the other. Of course, this is hypothetical: it was not my intention last May to regard it in any other light. For that reason my letter closed with an "if." Others besides Professor Ferrel, however, understood me to hav abandoned the older theory and taken up with the newer. tried to state Dr. Hann's point of view, and I do not regret hav ing stated it so fairly that it was taken for my own. That I ha not adopted it as fully as Professor Ferrel implies, may be in ferred from the close of my eighth paragraph and from th middle of the ninth, as well as from the ending of the letter a ready referred to. But in making this explanation, I do not wis to be understood as not welcoming the new theory. The abno mal warmth of anticyclones had been in my mind as a difficul in the way of convection, yet I had expected that cyclones wou be found to be still warmer; and it was not until reading D Hann's forcible statement that I perceived I had become t strongly settled in favor of the prevailing theory. On recognizi this partiality, I made all the more effort to give full and f consideration to the new one. It seemed to me nothing less th a duty to announce the facts and Dr. Hann's interpretation them in the same journal that had published my outline rend ing of the other theory some years before; and, in spite of Professor Ferrel's letter, it still seems to me that I was right in saying that the convectional theory needs revision in the light of Dr. Hann's results, but by revision I do not mean abandonment.

The incompleteness of the new theory is not a reason for being silent about it. It should be welcomed, if only for the reason that it will cause a healthful revision of previous views. The value of multiple working hypotheses has been so well set before our scientific readers, that nothing more need be said on that point. I will not venture to speak for Professor Ferrel, but I am sure that practically every meteorologist in the country will profit from a serious re-examination of his knowledge of the theory of cyclones in the light of Dr. Hann's researches.

As to the process by which the general circulation of the atmosphere shall produce cyclones and anticyclones, it is not to my mind necessary that this should be worked out completely before the suggestion of it may be profitably made. But it does not seem impossible that the general winds might here and there crowd together, owing to irregularity of flow; that, where crowded together, anticyclones would appear; and that, between the anticyclones, cyclonic whirls might be formed. It would be indeed a satisfaction if I could here answer all the pertinent questions, and give all necessary explanations, about such a problem; but, if we may judge by the treatment that dynamical meteorology has received thus far in this country, there is only one American who can do that. I wish that he might consider the possibilities of some such process arising from the general circulation of the atmosphere as is outlined above, and, after working them out rigorously, state them as clearly as he has explained the general circulation of the atmosphere itself. Whatever truth there is in the convectional theory of cyclones would not be harmed by such an investigation, while whatever truth there may be in the hypothesis of driven cyclones would pretty surely be discovered by it.

There is a corollary to the suggestion made by Dr. Hann, that may be of interest to those who seek for an explanation of our past glacial climates. It is generally recognized, that, if there were an increase in the activity of our winter cyclones, there would be an increase of snowfall as well; and, if this were carried far enough, the accumulation of snow might last over the summer. The increase of cyclonic activity would presumably accompany an increase in the general circulation of the atmosphere, if cyclones in our latitudes are driven by the general winds; and this would appear in that hemisphere whose equatorial and polar contrasts of temperature were strengthened. Such strengthened contrasts might be expected in the hemisphere having its winter in aphelion, and particularly at times of maximum orbital eccentricity. I do not mean to imply that a glacial period might depend on this condition alone; yet it may be one of many whose varying combinations at times produce a glacial climate, as Croll and J. Geikie and many others have shown; but this particular element of the combination does not appear to have been recognized.

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Harvard College, Cambridge, Mass., Dec. 27.

Moisture in Storms.

NEXT to the action of heat in storms, the part that moisture takes in them has been greatly emphasized. The so-called "condensation theory" of storms has had wider acceptance than any other. We may imagine a limited portion of the earth's surface heated up by the sun, and this more or less of a circular shape. There will be induced a tendency to an uprising current of heated air, which will continue so long as the central portion is warmer than the air surrounding it at the same level. This tendency, however, would be quickly brought to rest were it not for the fact that the uprising column has its moisture condensed, which liberates latent heat and causes the column to rise still faster. Here is a most remarkable fact, notwithstanding that the release of this moisture diminishes the total amount in the air, and the latent heat warms up the air, both of which causes would stop precipitation at once; yet we are taught that the force of the storm is increased by this process. There is another serious objection among many. If rain occurred at the centre of the storm, this theory might be plausible; but since the bulk of the rain in this country occurs three hundred miles to the eastward of the centre, and over only about one-fiftieth part of the area covered by the storm, it requires an enormous stretch of the imagination to grasp the causation of our wide-extended storms through this condensation effect. We may add still another consideration. It is fairly well ascertained that the upper limit of our storms, as shown by pressure and temperature observations at Pike's Peak (14,134 feet), is far above four or five miles, and may extend to the limits of the atmosphere. Now, the bulk of our precipitation is formed within 6,000 feet of the earth's surface: hence it is plain that the condensation of moisture plays a very subordinate part in our wide-extended storms, and has nothing to do with their generation or maintenance.

I do not propose to discuss at this time all the objections to this "condensation theory," which have been repeatedly advanced both in this and other journals, and which have not been answered, but I wish to present a recent most extraordinary abandonment of this theory by Dr. Hann, who stands at the head of the old school on the continent. I quote from a translation, by Professor Blanford of London, of a recent statement by Dr. Hann. Speaking against the condensation theory, he says (*Nature*, Nov. 6, 1890), "These views are such as I have always enunciated (for a long time, indeed, without any apparent result) in opposition to the then prevalent theories of the local origin of barometric minima through the agency of condensing water-vapor (as contended by Mohn, Reye, Loomis, and Blanford). They now begin to make way and prevail. Most clearly is this seen in the case of Loomis, who, in the course of his own persistent study of the behavior of barometric minima and maxima, has been compelled by degrees to give up the 'condensation theory' to which he formerly adhered so strongly, and to ascribe the origin as well as the progressive movement of cyclones to the general circulation of the atmosphere."

The importance of this utterance from such an authority cannot be exaggerated. While I have shown that Dr. Hann has been misled by his study of mountain observations, yet it seems to me this avowal on his part reaches out far beyond that. As I have just shown, the very life and existence of the old theory depend upon condensation of moisture. Now, if Dr. Hann, who must understand this fact most thoroughly, has deliberately set it aside, must we not conclude that it has an inherent weakness in itself to his mind. Those who are familiar with Loomis's work will be surprised to learn that he ever abandoned the condensation theory of storms.

It would seem that this controversy over the condensation theory is rapidly culminating, and the indications point to a speedy downfall of that theory. It is a remarkable fact that all the objections urged against this theory, now these many years, have been studiously ignored; but a few words from a recognized authority, even though based upon a wrong interpretation of facts, seem to make headway very rapidly. Surely Hann, Davis, and Blanford form a most formidable front against this theory, and it is high time its defenders should come to its assistance ere it be too late.

H. A. HAZEN.

Washington, Dec. 13.

["Letters to the Editor" continued on p. 8.]

NOTES AND NEWS.

AT a meeting of the Royal Botanic Society on Dec. 13, as we learn from *Nature* of Dec. 18, the secretary answered various questions as to the destructive action of fogs on plants. He said it was most felt by those tropical plants in the society's houses of which the natural habitat was one exposed to sunshine. Plants growing in forests or under tree shade did not so directly feel the want of light; but then, again, a London or town fog not only shaded the plants, but contained smoke, sulphur, and other deleterious agents, which were perhaps as deadly to vegetable vitality as absence of light. Soft, tender-leaved plants, and aquatics, such as the *Victoria regia*, suffered more from fogs than any class of plants he knew.