damaging reports as to his honesty; but in every instance I found his statements borne out by other testimony or by general analogy. Making due allowance for the mythologic features, which rather serve to establish its traditional character, his account is probably as full and accurate as could be expected at this late day, and briefly is as follows:—

"'The practice of building mounds originated with the Anintsi, and was kept up by the Ani-Kituhwagi. They were built as sites for town-houses (see Bartram's account of Cowe mound and town-house); and some were low, while others were as high as small trees. In building the mound, a fire was first kindled on the level surface. Around the fire was placed a circle of stones, outside of which were deposited the bodies of seven prominent men, one from each gens, these bodies being exhumed for the purpose from previous interments.'

"Swimmer said that his statement was obtained from a man who died in 1865, aged about seventy. Some time later, while talking with an intelligent woman in regard to local points of interest, she mentioned the large mound near Franklin, in Macon County, and remarked, 'There's fire at the bottom of that mound.' Without giving her any idea of what Swimmer had said, I inquired of her how the fire got there, when she told substantially the same story as she had obtained it from an old woman now dead. She was of the opinion that this fire existed only in the larger mounds; but I found on investigation that the belief was general that the fires still existed, and occasionally sent up columns of smoke above the tops of the mounds." CYRUS THOMAS.

[To be continued.]

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

Dr. Hann's Studies on Cyclones and Anticyclones.

ON April 17, Dr. Julius Hann, director of the meteorological observatory at Vienna, presented to the Vienna Academy an essay on "The High Pressure Area of November, 1889, in Central Europe, with Notes on High-Pressure Areas in General." The particular value of the essay lies in the comparison of records from lofty Alpine stations with those from the surrounding low country; the highest station being on the Sonnblick, over 3,100 metres above sea-level. The anticyclone of November, 1889, was chosen because it lay over the Alpine region from the 12th to the 24th of the month, giving ample time for the full determination of its persistent features. The results of the study are thus summarized:—

1. The barometer maximum of November, 1889, extended to a great height in the atmosphere, and was as pronounced at a height of 3,000 metres as at sea-level. At a height of 2,500 metres, the centre of high pressure lay over that at the earth's surface.

2. The body of air in the anticyclone had a high temperature. At 3,000 as well as at 1,000 metres, the temperature stood 8° C. above the mean. The usual depression of temperature, characteristic of winter anticyclones, was limited to the lower layers of air, next to the earth's surface, and was only a few hundred metres thick. The mean excess of temperature over the normal at successive heights up to 3,100 metres, for the period from the 19th to the 23d of November, can be estimated as at least 6° C. An excess of temperature must, at the most moderate determination, have extended up to a height of 5,000 metres.

3. In the upper air, above 1.000 metres altitude, a great dryness prevailed. The mean relative humidity from the 19th to the 23d of November on the Sonnblick (3,100 metres) was only 43 per cent, and on the Säntis (2,500 metres) 34 per cent, according to carefully reduced psychrometer records. Hair hygrometers gave a still lower percentage.

Dr. Hann sees in these facts a strong proof of the descending movement of the air in anticyclones, such as is generally accepted. He then goes further in saying that the motion of the air is not a product of the temperature, but is in spite of it: the temperature is a product of the motion.

A study is then made, for purposes of comparison, of an area of low pressure that passed nearly centrally over the eastern Alps on Oct. 1, 1889. Here the temperature of the air-column averaged 4.3° C. *below* the thirty-year normal for the time and place. Although earlier in the season, the air in this cyclone was absolutely colder than that in the later anticyclone. Even while a warm foehn was blowing down the northern valleys of the eastern Alps, the temperature on the Sonnblick was distinctly below the normal. In reviewing this, Dr. Hann says that it is the high mountain stations, recently founded, that have freed us from the prejudices into which we have been led by observations at low levels. It has been thought that the temperature of cyclones and anticyclones was the chief condition of their motion; but it appears certain from the foregoing, that the theory of cyclones must take account of the fact, that, up to the height of at least four or five kilometres, the central air-column of an anticyclone may be, and probably always is, warmer than that of a cyclone.

It is manifest that this contradicts the prevailing theory of the convectional origin of cyclones and anticyclones, while it confirms the views of those who, like Dr. Hann, regard cyclones and anticyclones as merely subordinate members of the general circulation of the atmosphere, their energy coming from the fundamental and persistent difference of temperature between the equator and the poles. According to this view, as Dr. Hann says, the temperature of the air-masses in cyclones and anticyclones is the product of their motions, and not *vice versa*. In the stationary cyclonic circulation of the far northern Atlantic, and in the winter anticyclones of the continents, differences of temperature are probably operative. Hence the author agrees with Teisserenc de Bort in distinguishing between thermic and dynamic cyclones and anticyclones. Moreover, in dynamic cyclones, the evolution of latent heat will maintain the air-mass at a higher temperature than that to which it would otherwise be reduced; but even then, the descending air in the adjacent anticyclone will be warmer as a whole than that which ascends in the cyclone.

This most interesting conclusion as to the origin of cyclones is a surprise to me; and therefore, having frequently advocated the sufficiency of the convectional theory of cyclones, I now make haste to place Dr. Hann's observations before the readers of *Science*, that they may see how clearly a revision of opinion is called for. The apparently convectional circulation in cyclonic storms is not doubted. There is unquestionably an ascending component of motion in cyclonic areas, and a descending component in anticyclones. It also appears to be generally true that at the earth's surface, temperatures above the normal are noted in cyclones, and below the normal in anticyclones. It cannot be doubted that the evolution of latent heat from condensing vapor in the rainy cyclonic area would favor any convectional movement that had originated from other causes. For all these reasons, the convectional theory came into favor, and other possible explanations were little considered. The convectional theory is merely a local application of a theory that is universally accepted to account for the general circulation of the atmosphere between equator and poles; but the tests now furnished by high-level observations seem to show that the local application of the theory is incorrect.

This is as if an observer who was familiar with stationary steam-engines should see a train of cars for the first time: he would rather naturally say that the locomotive was the motor of the train; he would hardly suggest the possibility that the motor was concealed in the rear car, and that the driving-wheels of the

locomotive made the piston-rod move in the cylinder; in fact, that the engine was a dummy. And yet this curious conclusion appears to be analogous to the one now presented by Dr. Hann. The cyclonic machine does not drive itself by its own store of energy: it is driven by an external motor, the general circulation of the winds. Some of the warm tropical cyclones may at first depend on their own energy; these would be true motors: but, if the definite records quoted by Dr. Hann prove to be of wide application, cyclones generally may come to be considered dummies. The cyclonic air does not rise because it is warm, but, according to Dr. Hann, it is lifted in spite of becoming cool. The anticyclonic air does not sink because it is cold, but is pushed down in spite of becoming warm. The ascending air is cooler than the normal because its adiabatic rate of cooling by expansion in ascent is, on the whole, greater than the mean vertical temperature gradient of the atmosphere; the descending air is warmer than the normal because its adiabatic rate of being warmed by compression in descent is greater than the mean vertical temperature gradient. Cyclones do not work themselves: they are worked by the general winds.

Redfield advocated a theory analogous to this in his early essays. He suggested that cyclones are not generated at places of rarefaction, but are only eddies in the general winds. Other early observers made similar suggestions; but it was not then possible to deduce tests by which this eddy theory could be confirmed or excluded. Faye's modification of Redfield's theory involves so many contradictions to well-established physical facts and laws, that it receives little acceptance. Espy was the first to call attention to the general occurrence of convectional movements in the atmosphere, and to the importance of liberated latent heat in promoting these movements. Reye, in later years, gave precision to Espy's ideas, and advanced the convectional theory greatly in the estimation of many meteorologists. I do not see that his deductions are in any way inaccurate. His calculation of the available horse-power supplied by the latent heat in a tropical cyclone appears to be pertinent, even under Hann's new interpretation of the cause of cyclonic movements. But through all the statements of the convectional theory, it has been tacitly assumed that the warmed air of the cyclone would be cooled by radiation in the anticyclonic area; and this does not seem to be the fact. The anticyclonic air is not much cooled till it approaches the ground; and in this we find confirmation of Searle's theory concerning the atmospheric economy of solar radiation.

The warmth of the body of air in anticyclones has been recognized for some time. Dr. Hann was among the first to give proper emphasis to the fact; but its relation to the convectional theory of cyclones has been slowly perceived. In this country, Hazen has drawn attention to the absence of indication of the "neutral plane," called for deductively; and for this and other reasons he has discarded pretty much all parts of the cyclonic theory, following Faye more closely than any other. The reason why Dr. Hann's objection to the convectional theory of cyclones appears to me so cogent and convincing is that it is presented, not as a contradiction, but as a corollary to the principles of modern physical meteorology, with which this eminent meteorologist is so thoroughly familiar, and to which he has himself contributed so much of value. The theory of the foehn, for example, was known in a general deductive way from the suggestions made independently by Espy, Dove Tyndall, Helmholtz, and others; but it was demonstrated by Hann. So in the present case: Redfield and many others have thought that the general circulation of the atmosphere might produce cyclones and anticyclones, somewhat in the way that rivers form eddies when flowing in an uneven channel; but there is a long distance between suggestion and proof. General indefinite suggestion of what is afterwards shown to be the correct view is not much superior to the suggestion of what ultimately turns out to be the wrong view. Precise definition and demonstration are of much higher value, and these qualities are truly characteristic of Hann's work. If further observation prove the general applicability of these newer views as to cyclones and anticyclones, the credit of the demonstration will go primarily to Dr. Hann. W. M. D.

Harvard College, May, 1890.

An Hypothesis for the So-called Encroachments of the Sea upon the Land.

IT is assumed that there is no substance which is absolutely rigid. The earth is a plastic mass. Let a mountain-range disappear, the plain on which it once stood rises when relieved of its weight. Let a lake disappear, and its bed becomes contorted, and the contour of its shore-line is changed. The walls of the Grand Cañon of the Colorado are moving toward each other, and, should it become an arid chasm, they would some time meet.

Now, in the southern and eastern portions of the United States the "fall-line" is the boundary of the permanent continent. The "continental outline" is the one-hundred fathom hydrographic contour, as determined by the United States Coast Survey; and from its crest there is a drop of over three thousand feet, — a front equal to one side of the Grand Cañon. From the "fall-line" to this front there is a creeping-forward, which is comparable to the ice sheets of Greenland: therefore cannot we say that the land at present is crowding down into the sea, instead of the sea encroaching upon the land, which is only an apparent movement, not the real one? The distance from the backbone of the Appalachians to the historic-geologic cedar-stumps of the New Jersey coast has increased, and is increasing.

The deltas of the Mississippi valley and the Gulf coast are not only increasing by deposits of sediment, but are moving forward as well. Therefore may it not be expected in geodetic work on the North American continent that there will be encountered discrepancies between successive determinations of positions which can only be thus accounted for and understood?

GILBERT THOMPSON.

Washington, D.C., May 23.

The Winnebago County (Iowa) Meteorites.

As the Iowa meteor of May 2 has received notice in your paper, it may be desirable to make the record still more complete. I therefore offer the following notes from the south-western corner of that State. The writer was not in position to see it, nor did the sounds appear to him enough unlike thunder to attract particular attention at the time. It was seen by perhaps a dozen citizens of this place, one of them being at the time less than ten miles from the south-west corner of the State. Most agree in thinking that it passed from the south-west toward the north-east. Some who saw it felt so sure that it struck fields close by to the north north-east, that they searched diligently for it. Many heard it, and thought it thundered. Some compared it to an earthquake shock, the jarring of the ground was so evident. Four distinct explosions were observed by one. A local paper of Malvern, nine miles away, stated that three pieces had fallen in that vicinity, but the statement was based on observations similar to those already given. J. E. TODD.

Tabor, Io., May 20.

Tornadoes.

SOME years since, I visited the scene of a small tornado shortly after its occurrence, and found the arrangement of tree trunks and other *débris* in its track very similar to what is represented in the article by Professor Hazen at p. 318 of *Science* for May 23. It seemed to me, however, that the peculiar arrangement found might be due to the combined effect of a whirling motion of the tornado, together with its motion of translation as a whole. In such a case there must be a compounding of forces, and the direction of the fall of a tree or other object can only be determined theoretically by a somewhat elaborate computation. Practically, and as a matter of fact, I have noticed, however, that when a small whirlwind is passing over a corn-field, the stalks incline inward toward its centre with a twisting motion, and likewise bend forward in the direction toward which it is advancing. I have never seen stalks actually uprooted and left prostrate in this way; but it looks very much as if they would be left with their tops inward and forward if this should happen, thus corresponding precisely to what is found after tornadoes. M. A. VEEDER.

Lyons, N.Y., May 23.