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.