the plate, both upon the air which is retarded and stopped in front of the plate, and also upon that behind the plate.

It is doubtful whether a descending current in the open air of more than two metres per second could be found anywhere in the whole atmosphere. This, we have seen, would increase the barometric pressure 0.0194 of a millimetre, a quantity which could not be detected by the most delicate and accurate barometer. It is seen, therefore, how very improbable is Dr. Hann's theory of the cause of high-pressure areas.

Dr. Hann lays great stress upon the efficiency of the steep gradients of the upper part of the atmosphere, in the middle and higher latitudes, in producing both cyclones and high pressure areas. But the forces arising from these gradients are almost completely counteracted by the deflecting forces of the earth's rotation in connection with the eastwardly moving currents in these latitudes, the velocities of which increase with increase of altitude very nearly in the same proportion as the steepness of the gradients Although the steepness of these gradients at high altitudes, especially in the southern hemisphere, is considerable when considered with reference to gravity simply, yet, if all the forces are taken into account, there is no part of the atmosphere in the middle latitudes where the gradients are smaller, the velocity of the easterly motion being such as to not quite counteract the force from the gradients, and to leave a residual force simply which is sufficient to counteract the frictional resistance in these high altitudes, which is very small. It would be just as reasonable to maintain that there is a strong tendency in the water of the ocean to rush toward the poles, because there are steep gradients, considered with reference to the earth's attraction only, and leaving out of consideration that the centrifugal force arising from the earth's rotation counteracts this tendency, as to maintain that the air in these high altitudes has a strong tendency to rush toward the poles. WM. FERREL.

Martinsburg, W.Va., Dec. 22.

Recent Investigation on the Causes of Cyclones and Anticyclones.

IF I were required to name the man who impressed me as the most profound meteorological writer whom I had read, I should without hesitation say Professor Ferrel.

The most of us are qualitative meteorologists: he may be called a quantitative meteorologist. Not content with mere general statements of causes and forces, he attempts to determine the exact value of each one, and by rigid mathematical formulæ to determine if they are sufficient to account for the given results.

This represents a high, if not the highest, development of a scientific mind. For this reason I would hesitate to dissent from Professor Ferrel's conclusions more than from any writer I know; but he has himself, in his recent letter to *Science*, severely criticised the supposed blind following of authority, and, if there were needed any excuse, I would give this as the reason for presenting the views opposed to those of Ferrel.

There are two methods of arriving at results. The one is by deduction, in which the thinker, starting from axioms, well determined constants, or general laws, works out the results which must follow. The other is by induction, in which the thinker starts from observation, or separate individual facts, and arrives at general laws. Both methods are necessary; and most thinkers of to-day will admit that no theory of natural phenomena is complete until the results of deductive reasoning correspond to the results of inductive reasoning, or *vice versa*.

Now, Ferrel is essentially a deductive reasoner. It is necessary in such reasoning that the fundamentals, or physical constants from which one starts, should be correctly determined. In Ferrel's and Marvin's replies to Hazen in *Science* and in the *American Meteorological Journal*, I believe it has been shown that the constants forming the basis of the calculation in Ferrel's condensation theory of cyclones were satisfactorily determined. Starting with these, and following Espy, he has shown, that, given a warmer body of air, or a rapid vertical decrease of temperature over a considerable area, the causes are adequate to initiate and maintain a cyclone.

The question now is, do the investigations of inductive meteorologists sustain these views?

In order to study the results which follow rapid vertical decrease of temperature in the atmosphere, Loomis "selected from the volumes of the published observations of the Signal Service (November, 1873, to January, 1875, and from January, 1877, to May, 1877) all of the cases in which the temperature at Pike's Peak was 40° lower than at Denver." With this difference between them, the air would theoretically be in unstable equilibrium. "The number of these cases in twenty months of observation was 343. Only 39 of these cases occurred during the seven winter months of observation, and they occurred most frequently during the months of May. . . . The facts appear to show that at the dates given there were seldom any extraordinary disturbances on Pike's Peak. In two cases hail was reported, in four cases sleet and in fifteen cases either rain or snow. These facts seem to indicate an occasional uprising, but it is remarkable that so few such cases occurred; and it will be noticed that a difference of temperature of at least 45° between Pike's Peak and Denver often continued from day to day for long periods. . . . I think we may hence infer that dry air, even when greatly heated, has but little ascensional force" (Loomis's "Contributions to Meteorology," 13th paper, in *American Journal of Arts and Sciences*).

Loomis also found that heavy rainfall was not necessarily productive of cyclones. In his sixth paper, after examining a large number of cases, he says, "We conclude, therefore, that great rainfalls do not generally continue over eight hours, and very rarely do they continue for twenty-four hours, either as experienced at one station, or in succession at different places." He arrives at the same conclusion in his seventh and seventeenth papers, and adds, "The forces which impart that movement to the air which is requisite to an abundant precipitation of vapor, instead of deriving increased force from a great fall of rain, rapidly expend themselves, and become exhausted."

Furthermore, after examining a large number of areas of low barometric pressure with which there was little or no rain, he says, "There seems to be no room for doubt that barometric minima sometimes form with little or no rain, and continue without any considerable rain for eight hours, and sometimes for twenty-four hours or longer; . . . so that it seems safe to conclude that rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion."

"In order to determine the circumstances under which storms originate and ultimately acquire their full intensity," Loomis selected thirty-six cases from the Signal Service weather-maps in which the storm appeared to develop in the United States, and, as a result of a study of these, says, "The first stage in the development of each of these storms was an area several hundred miles in diameter, over which the height of the barometer differed but little from thirty inches, with an area of high barometer both on the east and west sides, and at a distance of about 1,000 miles. In the few cases in which a high barometer is not reported on both sides of the origin, it is because the area of observation is not sufficiently extended. The mean value of the barometer on the east side was 30.42 inches, and the mean distance 1,033 miles; on the west side the values were 30.31 inches and 977 miles. . . . On Hoffmeyer's storm-charts we frequently find three areas of high barometer surrounding an area of low barometer. These areas of high barometer are regarded as one of the causes, and generally the most important cause, of the storm which succeeds. . . . Since the air presses in on all sides towards this area of low barometer, the area tends to assume an oval form, which may become sensibly circular if the winds are very violent, and the centrifugal force resulting from this revolving motion causes a still further reduction of the barometer. . . . Rain is one of the circumstances which increases the force of a storm, and it invariably attends storms when they have attained considerable violence. . . . Some rain was invariably reported whenever the barometer fell below 29.4 inches, and generally there was some rain reported whenever the barometer fell below 29.5 inches. I have found no storm of great violence which was

not accompanied by a considerable fall of rain" (Loomis's eighth paper).

As early as 1876 Hann found, from the observations on the alpine peaks, that the highest temperature in the upper air occurred with the highest pressure, and explained it as due to the dynamic heating of descending air.

In 1886 Dechevrens showed that on the European peaks Pic du Midi and Puy de Dome, and on Pike's Peak in the United States, the lowest temperature occurred with the lowest pressure, which was exactly the opposite of observations at sea level. He also gave an example of simultaneous observations at the base and summit of the Puy de Dome during a low and during a high pressure, as shown by the barometer at both stations. At the base the temperature was highest with the low pressure, but at the summit the lowest pressure and temperature occurred together (*American Meteorological Journal*, August, 1886).

In the *American Meteorological Journal* for May, 1886, Mr. Dewey stated that from thirty-four pairs of observations during the winter months of 1872 and 1873 he found the average difference of temperature between Burlington, Vt., and the top of Mount Washington to be 6.6° F. when the latter was within a hundred miles of the centre of an anticyclone. The normal difference between the two stations is 19°. In the different quadrants of the anticyclone he found the following differences: north, 9°; east, .0°; south, 4.5°; west, 12.2°; average, 9°. He found the average difference two degrees greater in cyclones. Hazen's results for Mount Washington and Burlington, however, differ from these (*American Meteorological Journal*, October, 1887), so that further comparisons are needed.

In a footnote to an article on the origin and development of storms in the *American Meteorological Journal*, September, 1886, I cited the following reasons for thinking that warmer air is not the essential condition of storm-formation: "Storms sometimes originate along the eastern Rocky Mountain slope when the temperature of the air is lower there than in any part of the United States (for an example see the Signal Service charts of Jan. 19 and 20, 1886), and storms appear to orginate in this region as often in the night as in the day."

Very recently Hann has investigated the temperature observations at numerous stations in the Alps during the passage of several cyclones (*Meteorologische Zeitschrift*, September, 1890), and has concluded that the temperature of the air-column as a whole is lower in cyclones than that of the surrounding air. Hann's investigations may not be conclusive for reasons stated by Ferrel, but they certainly add a link to the chain of evidence.

As a result of their investigations, Loomis and Hann both decided that cyclones were largely the result of mechanical causes. Loomis concluded that they were originated by the conflicting winds between two or more anticyclones, and Hann suggests that they are whirls originating in the upper air.

Now, I think Ferrel, in his recent letter to *Science*, unintentionally did Davis an injustice by suggesting that Davis had suddenly altered his opinion merely because Hann advanced these views. Davis has for years been the leading exponent in this country of the dynamical heating of the air in anticyclones, and during recent years I have several times spoken with him about the mechanical origin of cyclones; and, if he is now inclined to give these views more weight, it is because this last link in the chain of evidence has convinced him of the necessity of reconsidering the condensation theory.

I have for several years been convinced that mechanical action had much to do with the origin and development of cyclones, and as working hypotheses in making weather-predictions have carefully watched the following conditions as favorable for the production of cyclones: 1. The central region between approaching anticyclones. 2. The region where lower air-currents set in nearly opposed in direction to upper air-currents, so as to favor the production of a whirl. This latter condition is most frequently brought about in the United States when colder winds, moving from the north-west near the earth's surface, set in to the south or south-west of an area of high temperature or very high pressure, which give rise to upper currents moving from the south. This was the condition preceding the origin of the very violent storm of March 12, 1888. 3. The deflection of air-currents by a long, tall range of mountains, such as the Rockies. I have several times predicted the origin of cyclones under these conditions. One of these was on April 19, 1883.

I have found the following conditions favorable to the increase of energy in cyclones: 1. The meeting of cyclones moving from nearly opposite directions; 2. The closing-up of a long trough of low pressure by the pressure increasing at both ends; 3. Cyclones, being mainly controlled in their movements by upper air-currents, are sometimes carried by these toward areas of denser air near the earth's surface, and under these conditions tend to increase in energy. Examples of violent storms, developed, as I think, by these mechanical methods, will be found on the following dates: Oct. 14, 1886; Jan. 9, 1889; and Jan. 9, 1886.

The immense gain that would come from being able to anticipate this class of storms may be inferred from the fact that not one of those I have mentioned in this paper was heralded by our Weather Service in time to be of any use, though the amount of damage done was enormous.

The views I hold are, that differences of pressure result from differences of temperature over immense areas, as between equator and pole, ocean and continent. This distribution of pressure is modified by the effect of the earth's rotation, and is continuously varying with the changes in temperature of the air.

The smaller cyclones and anticyclones of our weather-maps are partly or chiefly brought about by the mechanical action of counter-currents in the manner previously explained, though greatly modified by local differences of temperature and density within the cyclone: in other words, they are caused by forces originating outside their field of origin instead of within it, as supposed by Ferrel.

General rains are chiefly the result, and not the cause, of ascending currents of air. Differences of pressure in the upper air have a very important bearing on the origin and development of cyclones. Well-defined areas of low pressure, accompanied by precipitation and an inward tendency of the upper wind, occasionally exist in the upper atmosphere without being indicated by the barometric pressure at the earth's surface.

I have held most of these views for several years, as will be found by my review of Loomis in the *American Meteorological Journal*, and by two articles in *Nature* on the origin of anticyclones, and the cause of precipitation (*Nature*, vol. xxxvi. 1887, and vol. xxxviii. July, 1888), and have hoped to make some quantitative estimates of the forces and supposed causes; but I have not had the time, and fear I have not the ability to do so.

I trust Professor Ferrel will not dismiss these as vague hypotheses unworthy of notice, but will tell us (1) whether the method suggested by Loomis is insufficient to generate a cyclonic whirl according to mechanical principles; (2) whether conflicting air-currents can be supposed to have sufficient inertia to aid in producing a whirl, as, for instance, when denser air sets rapidly inward from both ends of a long trough of low pressure; and (3) whether such cyclones as that of Jan. 20, 1886, which originated near the longitude of Denver, where the temperature was lower than in any other part of the United States, when the observations on Pike's Peak showed no vertical decrease at all between the summit and base of the mountain, and when there was no appreciable precipitation within a thousand miles of the place of origin, could be explained by any reasonable assumption of a higher mean temperature of the air-column within the field of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Dec. 29.

BOOK-REVIEWS.

Handbook of Problems in Direct Fire. By JAMES M. INGALLS. New York, Wiley. 8°. $4.

THIS book, which is believed to be the first of its kind ever published, shows the close attention now given to what may be called the scientific side of modern warfare, or, rather, of preparation for war. It is devoted wholly to problems in gunnery involving the use of ordinary service charges of powder and angles of elevation for the guns not exceeding 15°, which is the definition of