They also exert a powerful effect upon the body in all skin-diseases, but are probably less useful than the sulphurous waters in such cases. At present no waters of this character are utilized for baths, but could be readily led into suitable bath-houses at the Norris Basin. This locality is indeed the best suited for a sanitarium of any of the geyser basins of the park, as all the varieties of waters occur here, save the calcareous.

Sulphurous waters are very familiar, though those of the Yellowstone are particularly strong. The Mammoth Hot Spring waters, though smelling strongly of sulphur at the vent, possess little, if any, of that important constituent when led into baths, for it is all deposited about the vents and upon the algæ growing in the waters; but excellent examples of this type are found at the Norris Basin, as well as elsewhere in the park.

Now that the roads and hotel accommodations in the park are so good, and the region so easily reached in Pullman coaches and with dining-cars, it is to be hoped that the waters of these springs may bring relief to many sufferers.

WALTER HARVEY WEED.

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.

Time-Measuring among Savage Peoples.

THE question has arisen in the National Museum whether the American aborigines or any other savage peoples have any mechanical devices for measuring the time of day or portions of the day. I do not now allude to calendars, of which there are many, nor to observation of dawn, sunrise, a little after sunrise, near noon, noon, etc., based on the diurnal movement of the heavenly bodies, but to primitive dials and the like. I have heard of the Montaguai's practice of setting a staff in the snow and marking the shadow, and of the Pueblo habit of marking the path of a sun-ray across the floor, but my information is not first-rate. My familiarity with the African and Insular peoples is limited ; but it is designed to set up in the National Museum an elaborative series to illustrate time-keeping, and we are anxious to know what manner of invention should stand at the beginning of the series.

OTIS T. MASON.

Washington, Jan. 10.

Professor Ferrel and American Meteorologists.

IT would seem to be high time that some one having authority should read the riot act to a number of American meteorologists. The views lately advanced by Dr. Hann, that cyclones (excepting those of tropical regions) have their origin rather in the great general movements of the upper atmosphere than in the ascensional movement of relatively warm and moist air and the consequent vapor condensation, may or may not stand the test of a more extensive and critical series of temperature studies than those made in 1889, but it is none the less incumbent upon American meteorologists to treat with proper courtesy the conscientious and life-long labors of a fellow-countryman; and it is but scant courtesy to exhibit to the world an eagerness to drag into prominence and accept seriously a new theory of cyclonic genesis, when such a theory lacks in every way extensive and careful study, and is really but little more than a mere possibility suggested by an eminent foreign meteorologist, when he found in certain temperature observations a somewhat marked difference from those which the accepted theory seemed to him to require.

There may be "thermic," and there may be dynamic, cyclones; but the observations should be numerous and trustworthy before it is claimed that such a distinction exists, and before we seriously accept the very radical view that temperatures in cyclones are determined by the motions of the air. A thorough series of temperature determinations at different parts of the storm, as a mechanism, is needed, and should be offered. Especially is this demanded when the acceptance of the new view implies a partial remodelling, at least, of a theory that is of long standing, and has the sanction of one of the best equipped minds of the many that have tackled meteorological problems. Should occasion require, Professor Ferrel can doubtless successfully defend the views he holds; but, for the benefit of some who may not be aware of his methods of work, it may be not out of place to say here that nothing from his hand is the result of haste, but, on the contrary, the result of mature thought, and patient, careful, deliberate study of the best scientific information at his command.

With all possible deference to Dr. Hann's eminence in matters meteorological, it is to be questioned whether a series of temperature observations at some fourteen stations, seven of which have an altitude of over two thousand metres, for only two storms (the barometric maximum of Nov. 12–24, and the minimum of Oct. 1), prove any thing, after all, but that it is quite possible to find temperatures higher than the normal when lower ones might be expected. But this abnormality is but a slim support for a new theory, nor does it disprove the old. The air in the high area late in November was apparently warmer than the air in the "low" at the beginning of October; but that does not prove that the mean temperature of the air in any and every maxima is always higher than the mean temperature of any and every extra-tropical minima (it is conceded that the new theory will not hold for tropical storms). Dr. Hann claims that seven of these alpine stations have an elevation over two kilometres above sea-level. Yet it may be an open question if these heights give the conditions which he sought, more particularly if we remember that certain of the cirri clouds certainly have an elevation of not less than eighty kilometres, and a two-kilometre temperature observation may give but an uncertain indication. We can even find at surface stations abnormalities, that, if misinterpreted, might lead us to doubt a great many of our accepted views in the matter of atmospheric temperature. Mr. Kingston,[1] director of the Toronto Observatory in 1868, called attention to the fact that the twelve-year normals (1841–52) were not applicable to observations of later years, and, according to five year normals, it was easy to show that January was warmer than February, etc.; and Schott shows in a table how, from 1841 to 1850, February was colder than January at New Haven, Toronto, Philadelphia, Charleston, and Savannah, while from 1851 to 1860 the reverse holds true.

It is therefore, it seems to me, only fair to insist that American meteorologists demand full and most thorough evidence before seriously considering the question of modifying present theories; more particularly, too, when an unintentional but none the less real disposition exists in certain quarters to speak carelessly of Professor Ferrel and his work, and to deny him his proper place.

Not a bad example of this carelessness appears in a translation by E. F. Bamber, in the *Philosophical Magazine* for December, 1890, of Werner von Siemens's views on a general system of winds of the earth. The eminent physicist, in refuting the statement of Dr. Sprung in a recent paper in the *Meteorologische Zeitschrift*, that he attempted, like Ferrel, to found on theoretical calculations a theory of the general system of winds on the earth, disclaims in all modesty a sufficient proficiency in the higher mathematics to do this, but then immediately adds, it appears to us somewhat illogically, that he "considers this method altogether inappropriate." He therefore repudiates the charge that "he sought, like Ferrel, to demonstrate by means of calculation an original state of atmospheric motion in order to afterwards base his further speculations thereon."[2] There is no intentional intimation here, we take it, that Ferrel's views are based on a supposition more or less hasty and uncertain, and there is therefore little occasion for the rejoinder that any such intimation indicates a lack of familiarity with Ferrel's work; but it ought to be felt and recognized, especially by American meteorologists, that experimental fact rests at the bottom of every natural law

[1] See Schott's Tables, p. 199.

[2] Sitzungsberichte d. K. Preuss. Akad. d. Wiss. zu Berlin, 1890.

discussed by Ferrel, that in every case the latest and most accurately determined physical constants are used, and that the theoretical deductions, while simply offered as such to be tested, are strictly the results of mathematical analyses. If in time these appear inadequate, the measure of praise for the man and his work may be diminished, but only in proportion as it is remembered that meteorological data and laws were in a condition more or less chaotic when he took up his labor of developing these into a consistent harmonious science.

ALEXANDER MCADIE.

Washington, D.C., Jan. 9.

Cyclones and Areas of High Pressure.

I HAD supposed that Professor Davis would give some explanation of the argument against the condensation theory of cyclones deduced from the comparisons of the temperatures in cyclones with those in high-pressure areas. He commences with a citation from my book, in which I state that the high pressures in the north-west sides of cyclones in the higher latitudes, in winter are caused mostly by their lower temperatures, and consequently greater densities. He thinks the high pressure over the Alps in November, 1889, is a typical case of all such high-pressure areas. While I do not so regard it, yet, for the sake of brevity, I will here concede it, and consider merely this supposed typical case. Over the Alps, during the last five of the fourteen days of the existence of this high pressure, the temperature on the summits of the Alps was found to be several degrees warmer than the normal temperature of the season. There are no observations to show how high this abnormal temperature extended, but I am willing to admit that it may have extended up to a considerable altitude. Professor Davis, because this temperature is found to be above the normal a few degrees, maintains that the descent of the air is not due to its being heavier than the surrounding air, thus assuming that the surrounding temperatures at a distance at the time are the same as the normal temperature, notwithstanding the well-known great and long-continued departures from the normals which frequently occur over large areas of the country. But it is not necessary that this body of heated air in high-pressure areas should have a temperature lower than the surrounding temperatures even; for if the great vertical extent of air above it has a temperature only one or two degrees lower than the surrounding temperatures on the same levels, which gives rise to a descending current, the air below, if it even has a little higher temperature than the surroundings, cannot rise up through the descending current, but must be forced downward. But suppose it were clearly established that the air in a high-pressure area extending hundreds of miles had a lower temperature than the surroundings even, and not merely the normal of the season: how is the greater pressure and the descent of the air to be accounted for? Professor Davis has never hinted at a probable explanation merely. The deduction, therefore, from a few surface observations merely in a very limited region, that the air over a large area, and extending to the top of the atmosphere, is warmer than the surrounding air at a great distance in all directions, especially where these few observations are found to give a temperature above the normal merely, and not above the surrounding temperatures at the same levels, should be received with great caution; for, if there were even a well-established theory to account for the descent of the air under these circumstances, these observations could scarcely be regarded as having any weight in confirmation of such a theory.

In what precedes I have gone upon the assumption that a lower temperature is the only cause of the descent of the air in high-pressure areas. While I regard this as adequate to account for it, I have never said or thought that it is the only cause, but simply the principal cause. I think there are other causes, especially in the origin of these high-pressure areas, which, for our present purpose, it is not necessary to discuss here.

Professor Davis says, "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." By "convectional theory of cyclones" I understand him to mean the condensation theory, which requires the air in the ascending current to be warmer and lighter than that of the surroundings at the same levels. Now, this theory can neither be established nor overthrown by any such tests. Cyclones are usually several hundred, sometimes a thousand and more, miles in diameter; and to prove that the air over so large an area up to the top of the atmosphere, or at least up to high altitudes, has a higher or a lower temperature than its surroundings, would require numerous stations of observation at many different levels, not only over this large area, but also all around this area at great distances. The condensation theory requires that the temperature of the air in a cyclone must be greater, in a general way, than that of the surrounding air; but this does not mean that there are no places within the cyclone, especially on the earth's surface, with lower temperatures than those of many places outside. In the theoretical treatment of a cyclone we have necessarily to assume certain regular conditions of uniform temperature at the same distances in all directions; but I have always been careful to explain that such conditions are never found in nature, but generally only rough approximations. In a large cyclone there is a great difference between the north and south sides, due to difference of latitude, which is taken into account in the general motions of the atmosphere, and so must be excluded in the treatment of the cyclones, and the differences of temperature only with reference to corresponding temperatures outside of the cyclone on the same latitudes must be considered. Besides, the temperatures vary all around the cyclone, not only on account of difference of latitude, but likewise from various abnormal causes. It must be expected, therefore, in comparing inside temperatures with the surrounding ones, especially surface temperatures, that there would be numerous cases in which those within would be found lower than many of those in the surroundings. The theory only requires that there shall be a predominance of higher temperatures in the interior. Besides, the conditions of a cyclone need not extend down to the surface at all, and, in fact, mere surface conditions generally have little or nothing to do with a cyclone. If the necessary conditions exist at altitudes only considerably above the earth's surface, the air is thrown into a great whirl or gyration, which relieves the air below of a part of the pressure upon it, and increases the pressure round about; so that this air tends to rise up, just as the water does in a suction-pump, and the surrounding air flows in to take its place; and in flowing in it assumes a gyratory motion, not only from the deflecting force of the earth's rotation, but likewise from the action of the air above by means of friction, so that it is brought into the general vertical and gyratory circulation. But suppose that it could be shown that the air in a cyclone is mostly or entirely of a lower temperature than the surrounding air at all altitudes, and yet ascends, as it always does: how is this strange phenomenon to be accounted for when there is no force, either real or imaginary, to cause it to ascend?

Professor Davis thinks that the snow-fall on the Alps at the time of the cyclone of Oct. 1, 1889, had little effect in lowering the temperature, on account of the wind; but this is one of the causes which Dr. Hann gave, a few years ago, of the lower surface temperatures in cyclones. The air, in being forced up the mountains on the windward side, is expanded and cooled below the temperature of the air generally on the same level. Another reason which he assigned was, that as the lowest pressure above lags behind that below, as was shown by Loomis, and first explained, I think, by Dr. Hann, the cold north-westerly winds set in above rather before the lowest pressure-point is passed. The real centre of the cyclone above is not that of lowest pressure.

I admit that it is not strictly logical to assume that two theories, or two kinds of forces, may not be such as to give the same effects, especially where nothing is known of the nature or manner of application of the one kind; but still this is extremely improbable. As the general motions of the atmosphere, cyclones, and tornadoes, are all very much alike, consisting of gyrations around a centre, — and it is admitted that in the first and last the air rises where it is warmest and lightest and because this is so, and that this is even the case with cyclones in the lower latitudes,— we should hesitate in making an exception in the case