SCIENCE:

A WEEKLY NEWSPAPER OF ALL THE ARTS AND SCIENCES.

PUBLISHED BY

N. D. C. HODGES,

47 LAFAYETTE PLACE, NEW YORK.

SUBSCRIPTIONS.—United States and Canada.....$3.50 a year.
Great Britain and Europe..... 4.50 a year.

Communications will be welcomed from any quarter. Abstracts of scientific papers are solicited, and twenty copies of the issue containing such will be mailed the author on request in advance. Rejected manuscripts will be returned to the authors only when the requisite amount of postage accompanies the manuscript. Whatever is intended for insertion must be authenticated by the name and address of the writer; not necessarily for publication, but as a guaranty of good faith. We do not hold ourselves responsible for any view or opinions expressed in the communications of our correspondents

Attention is called to the "Wants" column. All are invited to use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

VOL. XVI. NEW YORK, OCTOBER 31, 1890. No. 404.

CONTENTS:

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.

Fluctuations of Air-Pressure.

IT is probable that no problem in meteorology has been so puzzling as the explanation of the diurnal range of the barometer. This phenomenon is one of the most constant in meteorology; and, in fact, it is so regular near the equator, that Humboldt once said he could tell the time of day in that region by reading his barometer. On examining a barograph trace made in the tropics, we find a most surprising regularity both in the maximum and minimum points of the curve day after day, and also in the amplitude of the range. These conditions, however, do not exist in the temperate regions or in those farther north. Here there is superposed upon the diurnal range a mixed fluctuation, due in the main, if not entirely, to the passage of areas of high and low pressure. These areas are continually passing, and in consequence the diurnal range is masked, or even entirely obliterated on some days. It is known, however, that in general the diurnal range is much increased on bright, sunshiny days. Gen. Greely found a trace of this range at Fort Conger, 81° 44′ north. It is known that the range has two maximum and two minimum points. The principal maximum occurs all over the globe between 9.30 and 11 A.M., and the principal minimum from 2.30 to 4 P.M. The two other points occur approximately at the hours of the same name at night. This remarkable fluctuation is observed at the tops of high mountains, showing that the cause is above the lower atmosphere. The voyages of the "Challenger" and of other vessels have shown that this range is the same over the ocean as on the land, though the water temperature changes very slightly over the former.

It would be impossible to give in a short space all the explanations that have been advanced for this phenomenon. Changes of temperature and moisture have been appealed to in vain. A potential effect has been suggested from the fact that there may be a re-action, as it were, from the air, owing to the increasing heat in the lower strata after sunrise. It is not too much to say that any and all explanations which ascribe this change in pressure to movements of any kind in the air, to a secondary effect from changes in temperature or moisture, and to any of the forces or agencies usually appealed to in atmospheric movements, have signally and utterly broken down. In 1881 J. Allan Brown (now deceased), after thoroughly examining this question, says (*Nature*, April 14, 1881), "If we suppose that the attraction of gravity is not the only attraction which affects the pressure of the atmosphere, but that this pressure varies through some other attracting force, such as an electric attraction of the sun depending upon the varying humidity of the air, and this again depending on its temperature, we should find another method of relating the two variations, which does not exist if gravitatien alone is employed." In 1882 the present writer was called upon to give a course of twenty lectures upon meteorology before a class at Fort Myer, Va. In the nineteenth lecture of this course the remarkable similarity between the curves of diurnal range in air-pressure and of the declination magnet was pointed out, and a connection between these phenomena, as well as a common origin, were distinctly suggested (see *Annual Report of the Chief Signal Officer*, 1882, p. 142).

There has just come to hand an interesting paper on "Diurnal Variation of Terrestrial Magnetism," by Professor A. Shuster (*Philosophical Transactions of the Royal Society, London*, vol. 180, p. 509). I quote from the latter part of the paper.

"The late Professor Balfour Stewart has suggested that the earth's magnetic force might induce electric currents in the convection currents which flow in the upper regions of the atmosphere. One difficulty of this hypothesis was removed by an experimental investigation, by means of which I proved that the air can be thrown into a sensitive state in which small electro-motive forces will produce sensible electric currents. To bring the air into that sensitive state, it is only necessary to send an electric current through it from some independent source of high potential. It is very likely that the air in the upper regions of our atmosphere is in such a sensitive state; and it is quite possible, therefore, that the induced electric currents suggested by Professor Stewart really exist. In order that electric currents should be induced which could account for the observed movement of the magnetic needle, it is only necessary to imagine convection currents in the upper regions from east to west during certain parts of the day, and from west to east at other times. As regards the effect of the sun, we have, indeed, a daily period of the barometer which is probably due to thermal effects. It is curious and suggestive, that the horizontal motion which must accompany the change in pressure is just such as would account for the daily variation of the magnetic needle. In the tropics the principal minimum of the barometer takes place about 3.40 P.M., and the principal maximum about 9 A.M. According to the theory of waves, there would be a horizontal movement from west to east in the afternoon, and from east to west in the forenoon. The direction of the induced electric currents would be away from the equator in both hemispheres in the afternoon, and towards the equator in the forenoon. This is exactly the system of currents we have been led to, starting from the observed magnetic variation. The only difficulty I feel in suggesting that the cause of the diurnal variation of the magnetic needle is the diurnal variation of the barometer, lies in the fact that it would oblige us to place the electric currents into the lower regions of the atmosphere, as these only will be much affected by the thermal radiation of the sun."

I give four of Professor Shuster's conclusions.

"1. The principal part of the diurnal variation [of the needle] is due to causes outside the earth's surface, and probably to electric currents in our atmosphere.

"3. As regards the currents induced by the diurnal variation, the earth does not behave as a uniformly conducting sphere, but the upper layers must conduct less than the inner layers.

"4. The horizontal movements in the atmosphere which must accompany a tidal action of the sun or moon, or any periodic variation of the barometer such as is actually observed, would produce electric currents in the atmosphere, having magnetic effects similar in character to the observed variation.

"5. If the variation is actually produced by the suggested cause, the atmosphere must be in that sensitive state in which, according to the author's experiments, there is no lower limit to the electromotive force producing a current."

Meteorologists are now becoming somewhat accustomed to seeing suggestions from generally recognized authorities, showing the possibility of some kind of electric action taking place in the atmosphere, though I do not claim that this view is accepted as yet in any except an extremely limited sense as an explanation of atmospheric phenomena. I do not fully understand some of Professor Shuster's views. The expression "convection currents which flow" does not convey a definite meaning. Just how electric currents are to be induced in the upper air strata by a wave of heat is also not entirely plain. The expression "the horizontal motion which must accompany the change in pressure," as applied to the diurnal range of pressure, I think, simply means the horizontal propagation of a wave of pressure, and not any actual motion of air. I can hardly see how the convection currents of a thermal wave with the minimum point in the lower air strata at sunrise, and the maximum point about 3 P.M., can possibly induce electric currents having critical points at 10.30 A.M. and 3 P.M. It is generally accepted that the diurnal range of temperature in the free air is practically nothing at heights above five thousand feet; so that the supposed convection currents from the direct thermal effect cannot induce electric currents in the upper strata.

It seems to me there is also an objection to Dr. Hann's view, that the diurnal range of air-pressure can be due to the direct heat-action (*Wärmewirkung*) of the sun upon the upper limits of the atmosphere.

I have very recently discovered a method of studying the diurnal range of air-pressure and allied phenomena which seems capable of great extension, and possibly of displaying many important facts. For many years I have searched for a simple method of separating out the accidental variations of pressure, temperature, moisture, etc., in our atmosphere, produced by the progress of high and low areas, from those which are more or less constant in their action. It is quite well established that these highs and lows (called by many "anticyclones" and "cyclones") have a common progression from west to east, especially in this country. The plan proposed is to carry back the conditions observed at any moment in the atmosphere to a point of time several hours before, say twelve hours, or especially twenty-four hours. This becomes a simple matter when we have a regular advance in a high or low; for we can place its centre at the point where it was twelve or twenty-four hours previously, and then at once read off the amount of change at each station for either of those intervals of time. During the month of August, 1890, I had occasion to try this method for determining the diurnal range of air-pressure from 8 A.M. to 8 P.M., and *vice versa;* and the results were of the highest interest. For example: on Aug. 6, at 8 P.M., there was a fall in pressure in twelve hours of from one tenth to two-tenths of an inch from the Atlantic to the 107th meridian. The next morning there was almost a complete recovery over this region. On the 8th in the morning there was a rise of from one-tenth to two-tenths of an inch over the whole, and at 8 P.M. of the same day there was a fall of one tenth of an inch over half, the above region. I found the general tendency for this diurnal range in air-pressure to be the same for both highs and lows, though the fall is generally slightly greater in the low, and *vice versa* in the high. It should be noted that this diurnal range was often obliterated, or even carried in an opposite direction, in some parts of the country when it was impossible to centre exactly the two maps. There was a law, however, showing a marked general rise in pressure in the forenoon, and a corresponding fall in the afternoon. This may be seen to be all the more remarkable, since the observations were made at 8 A.M. and 8 P.M, 75th meridian time, and not at 10.30 A.M. and 3 P.M., which are the maximum and minimum points in the diurnal range. We see that these changes in air-pressure are not due to the advance of any air-wave or of air-particles, for this would imply a velocity of over two hundred miles an hour for this region alone. The rise in pressure in the forenoon cannot be due to an inflow of air from surrounding regions, because the rise occurs over all the surrounding region. It seems impossible to consider that any direct thermal effect can do more than simply heat the air without changing its pressure. It seems to me we are driven to the hypothesis that these changes are brought about by some agency outside of the earth. Is it incredible to suppose that there is a thermo-electric action from the sun upon the atmosphere propagated like a wave one thousand miles per hour, which is the principal cause of the diurnal range of both the magnetic needle and of air-pressure, and upon which other effects may be superposed?

H. A. HAZEN.

Washington, Oct. 18.

Origin of Right or Left Handedness.

THE question of the nature and origin of right or left handedness has given rise to much discussion in late years, the conviction growing among investigators that it is due to some hidden difference in the structure or function of the two hemispheres of the brain. The best *résumé* and general discussion of the question is the learned monograph by Sir Daniel Wilson entitled "The Right Hand and Left-handedness," being a reprint from the "Transactions of the Royal Society of Canada," Section II., 1886.

In order to examine more particularly into the time at which the child begins to show signs of marked preference for either hand, I instituted a series of experiments upon my own child, extending them over the greater part of the first year. As I have no time at present to write up the results systematically, I wish simply to announce a point or two which may be of interest to students of the subject.

1. I found no trace of preference for either hand as long as there were no violent muscular exertions made (based on 2,187 systematic experiments in cases of free movement of hands near the body: i.e., right hand 585 cases, left hand 568 cases, a difference of 17 cases; both hands 1,034 cases; the difference of 17 cases being too slight to have meaning).

2. Under the same conditions the tendency to use both hands together was about double the tendency to use either (seen from the number of cases of the use of both hands in the statistics given above), the period covered being from the child's sixth to her tenth month inclusive.

3. A distinct preference for the right hand in violent efforts in reaching became noticeable in the seventh and eighth months. Experiments during the eighth month on this cue gave, in 80 cases, right hand 74 cases, left hand 5 cases, both hands 1 case. In many cases the left hand followed slowly upon the lead of the right. Under the stimulus of bright colors, from 86 cases, 84 were right-hand cases, and 2 left-hand. Right-handedness had accordingly developed under pressure of muscular effort.

4. Up to this time the child had not learned to stand or to creep: hence the development of one hand more than the other is not due to differences in weight between the two longitudinal halves of the body. As she had not learned to speak or to utter articulate sounds with much distinctness, we may say also that right or left handedness may develop while the motor speech centre is not yet functioning.

Other points resulting incidentally are of interest in general psychology: i.e.,—

5. At the end of the seventh month the child's visual estimation of distance was exact enough to lead her invariably to refuse to reach for an object more than fourteen inches distant, her reaching distance being from nine to ten inches (based on tabulated experiments). Moderate stimuli she refused beyond thirteen inches.