of fact, the sun does not heat up a limited portion of the earth. Its rays shine with equal intensity over 1,000 miles from east to west. It has also been shown that this heating of the surface does not ascend more than a few inches in the air. One of the strongholds of the theorists is unstable equilibrium; but right here we find two seemingly contradictory statements. On p. 51 of Professor Ferrel's book, 'Recent Advances in Meteorology,' there is a suggestion that this state (unstable equilibrium) is brought about whenever there is a less diminution of temperature with height in an ascending column than in neighboring portions of air. On p. 328 of the same volume, however, the idea is given that this same state may be produced if there is an abnormally great diminution of temperature with height. It would seem as if in both these instances, even if there were a tendency to this state, air would flow in at all times from surrounding regions, and instantly relieve the condition. This relief would be afforded the more rapidly, the less the friction. However, the error here is farther back. We cannot suppose that the atmosphere is either quiescent or flowing in a current having a uniform velocity in all its layers, to the height, say, of 15,000 feet. The fact is admitted that there is a uniform acceleration in the different strata as we arise; so that, even if an upward movement should begin, a few hundred feet would destroy all vertical tendency. As a matter of fact, when we consider the actual conditions under which solar radiation acts at a storm-centre, we see that this unstable state could not be formed. At a storm-centre clouds cover the earth's surface, and prevent all abnormal conditions from great heat. Balloon-ascents have shown uniform temperatures up to the top of the clouds.

The theoretical computations of the velocity of the upper air strata do not correspond with the actual movements recorded. On p. 259 Professor Ferrel gives the velocity of the current at the height of 16,000 feet as 26 miles per hour in the middle latitudes of the United States.

On Mount Washington, 6,300 feet in height, the velocity when a low area passes is 53 miles per hour, and when a high area passes it is 21. The velocity of the low areas near Mount Washington is 34 miles per hour. This would indicate that the 'power' of the storm must be below 6,300 feet, since it is admitted that its progressive motion is due to the movement of the strata where it exists. It may be safely said that a height of less than 6,000 feet for the centre of disturbance would be fatal to a great many of the present theories of storm-generation.

Formerly it was said, that, owing to friction with the earth's surface, the upper part of the storm must be in advance of the lower; but it is certain that such a state of things could continue only a few minutes, for the upper portion of the storm would be rapidly separated from the lower. Professor Ferrel, on p. 260 of the present volume, explains this difficulty by suggesting that the upper part of the storm is continually re-forming itself, and that there is no actual transferrence of air. I hardly think that this suggestion will be accepted. It seems to me our storms would behave differently if it were true, and certainly our synoptic charts do not give any clew to such re-formations of the upper part of the storm. It seems to me this later theory destroys the continuity of the ascending current and the essential features of unstable equilibrium. One of the most difficult phenomena to explain is the fall of rain at a distance of 300 and more miles from the storm-centre. If we suppose the ascending currents are at the centre of the storm, then rain should fall at that point. Professor Ferrel, at p. 266, advances the novel idea that the rain is formed in or carried to the upper currents, and, as these are more rapid than the storm, it must fall in advance of the storm. I do not think this theory takes sufficient account of the facts. Let us suppose the raindrop carried to a height of 7,200 feet: observations in balloons show that rain very rarely occurs above that height, and that the 'power' of the storm is at 5,000 feet. We may consider the velocity of the current at 7,200 feet 15 miles per hour greater than at 5,000 feet: the drop would fall at about 10 feet per second, or would reach the earth in 12 minutes; and hence, if it had been carried in the upper current during this time, it would have fallen 3 miles in front of the centre, instead of 300 or more. As a matter of fact, since the currents below 5,000 feet are very much slower than above that height, any acceleration would be entirely overcome, and from these principles the drop would actually fall back of the centre. On the continent of Europe the bulk of the rain falls at the rear of the storm.

To my mind, however, theoretical meteorology most signally fails in its attempts to explain our more violent storms and tornadoes. That the sun's heat could start a vertical current which, with the condensation of moisture in the upper air, would give rise to winds of 200 or 300 miles per hour, seems incredible. The attempt to meet the difficulties by suggesting 'great contrasts of temperature,' 'meeting of warm southerly with cold northerly winds,' 'cool air overrunning warm,' 'warm air overrunning cool,' etc., does not seem at all satisfactory. As long as it was supposed that tornadoes occurred at the centre of a low area where it was thought there was an ascending current, the theory seemed plausible; but when it was clearly shown, in March, 1884, that tornadoes do not occur at a low centre, but 400 or 500 miles to the south-east, it became necessary to explain this. It seems to me that all attempts to elucidate this subject have merely served to lighten the darkness without removing it.

There is no space left for minutely examining the great superstructure built on what seem weak foundations. It seems as though the first and most important step is to remove the slur cast upon this science by those who are qualified to know its weakness. Let our theorists bend every energy to establish some fundamental proposition, either by actual experiment in the laboratory or by investigation in nature's laboratory at the spot where the 'power' of the storm manifests itself. It seems to me the recent attempts of Weyher in France to demonstrate the existence of this 'power,' by means of a rapidly revolving fan at some distance above water or grain, show the great need of further proof. These experiments show what might be if only there were an enormous fan in the upper air, but where is the fan? Must we not conclude that the true explanation is now farther off than before, and certainly much farther from the present theories.

H. ALLEN HAZEN.

Washington, July 1.

Determination of the Depth of Earthquakes.

THE report of Captain Dutton and Everett Hayden on the Charleston earthquake (*Science*, ix. p. 489) is undoubtedly a very valuable addition to earthquake literature. There are two or three points, however, to which I wish to draw scientific attention, in the hope that investigation hereafter may clear them up.

Perhaps the most interesting and important point in the report is their method of determining the depth of earthquakes. The authors first review rapidly other methods. Mallet's method — by protracting the lines of emergence back to their meeting-point — they dismiss as too uncertain. Seebach's method — used in the earthquake of Central Germany in 1872, which depends on the law of decreasing velocity of the emergent wave — they also dismiss, because the times of arrival at different points cannot be determined with sufficient accuracy, on account of the different velocities of the two different kinds of waves, normal and transverse. In place of these methods they propose what they claim to be a wholly new one, founded on the law of decrease of intensity; i.e., of decrease of the shock-motion or motion of the earth-particle, or, in other words, the wave-height or amplitude.[1] They show by mathematical discussion that the place of the maximum rate of decrease of intensity bears a fixed relation to the depth of the focus; viz., as 1 to $\sqrt{3}$. Upon this basis they estimate the depth of the focus to be about twelve miles. In Fig. 1, which we reproduce from their report, the fall of the double-curved line represents the decreasing intensity. The place of most rapid fall, i.e., where the curve changes from convexity to concavity, is the place of most rapid decrease of intensity. This place was quite distinctly marked. It was about seven miles from the epicentrum.

We wish now to draw attention to the fact that this method does not differ very greatly from, and perhaps is not an improvement upon, another method suggested by Mallet in his 'Report to the British Association, 1858,' p. 102, though not used in his discussion of the Neapolitan earthquake of 1857; viz., by means of what may be called 'the circle of principal disturbance.' This method is mentioned and explained in my 'Elements of Geology,' p. 117. The authors seem to have overlooked it.

[1] With constant wave-length, intensity $\propto$ amplitude.

The destruction about the epicentrum of an earthquake depends mainly, perhaps, upon the amount of motion, but partly also upon the direction of motion; horizontal motion being far more destructive than vertical. Now, the whole amount of motion is assumed to decrease as the square of the radius of the agitated sphere increases $(i \propto \frac{1}{r^2})$; but the horizontal element of the motion increases as the cosine of the angle of emergence. Under these two conditions, there will be a certain distance all about the epicentrum, bearing a fixed relation to the depth of the focus, where the horizontal element will be a maximum. This is at *dd'* (Fig. 2), where the angle of emergence is 54° 44'. In other words, the 'circle of principal disturbance' is the base of a cone whose apex is at the focus, and whose apical angle is 70° 32'. The distance *ad* of this circle from the epicentrum is to the depth of focus *ax* as 1 to $\sqrt{2}$.

Now, it is evident that in violent earthquakes the destruction over the whole area of this circle might be nearly the same; for in the central parts the whole motion would be greater, and on the margins the sideways motion would be greater. But beyond this circle the destructiveness would very rapidly decrease, because the whole motion and the sideways element are both decreasing: in other words, if we used the graphic method, the curve of destructiveness would be like the curve of intensity (Fig. 1), except that it would be flatter on the top, and the descent more abrupt at a certain distance from the epicentrum. The decrease of destructiveness is more rapid at a certain point than is the decrease of intensity.

Now, since the intensity is estimated largely, if not wholly, by destructiveness, and since destructiveness depends largely upon the sideways motion, is it not possible, is it not even probable, that the supposed place of maximum decrease of intensity is really the place of maximum decrease of destructiveness; i.e., the circle of principal disturbance? If so, then the depth of the focus would be about ten miles instead of twelve miles.

We have assumed all along that the intensity or excursion of the earth-particle, or the height or amplitude of the wave, varies inversely as the square of the radius of the agitated sphere $(I \propto \frac{1}{r^2})$. The authors as well as all other writers assume this law. But is there not good reason to doubt its accuracy? The law is probably true so long as the wave is spherical; i.e., until it emerges on the surface. But when it emerges, what becomes of the energy which would have continued the wave if it had not been cut off by emergence? Some of it is doubtless consumed in more violent motion, and perhaps rupture, at the surface; but is not much of it reflected back into the earth to combine with the advancing waves? All other elastic waves, whether light-waves or sound-waves, coming out of a denser medium into a rarer (or *vice versa*), are largely reflected from the surface: why not earthquake-waves also? Suppose the spherical wave were cut off, not on one side only, but on both sides; in other words, suppose a shock generating normal circular elastic waves of compression to occur in the centre of a thin plate: is it not evident that the intensity of these would vary simply inversely as the radius $(II \propto \frac{1}{r})$? Or, if the plane be reduced to a bar, such waves would be substantially constant in intensity.

But we are not left to general reasonings on the subject. If the intensity or wave-height follow the law of inverse squares, it is impossible to understand how the waves should carry so far as we actually find. In the Charleston earthquake the motion at the distance of six hundred miles was still sufficient to create alarm and to produce seasickness. Now, the amount of motion at the epicentrum was not more than ten or twelve inches. Let us take twelve inches as the greatest motion, and the epicentrum as ten miles from the focus. At the distance of six hundred miles, according to the usually assumed law of decrease, the amount of motion or wave-height would be only a three-hundredth of an inch; but if the spherical wave is reflected back from the surface, and combines with the advancing wave, it is probable that its decrease is only as the increase of the radius. In that case, at six hundred miles the motion would still be a fifth of an inch, which is a very sensible motion.

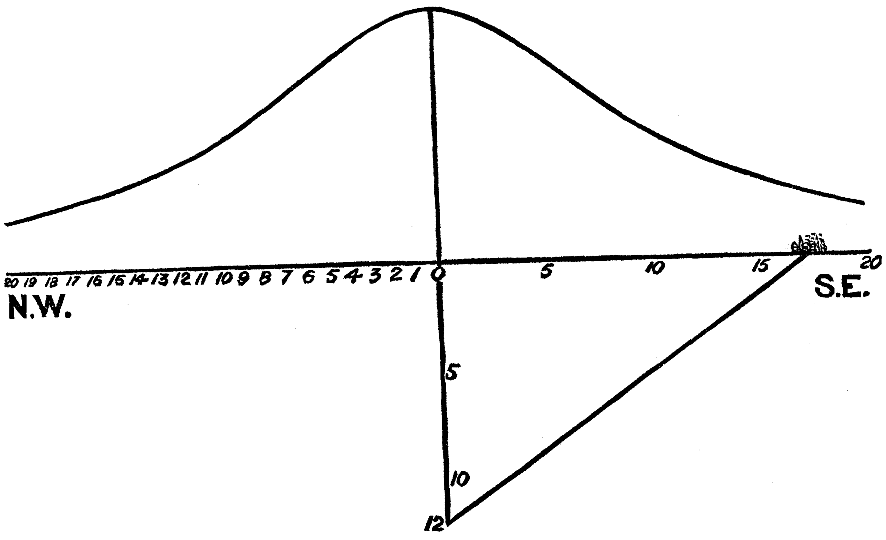

FIG. 1.

It is very important that investigations should be undertaken to determine the law of decrease of wave-motion of earthquakes. This, however, cannot be done without seismographs.

While on this subject, it may be well to say something about Seebach's method of determining the depth of the focus. The method by the circle of principal disturbance, and that by maximum decrease of intensity, are based on the law of inverse squares, and therefore fail if this law be untrue. Seebach's method, on the contrary, is based on the law of decrease of velocity of the surface wave; supposing, of course, a constant velocity of the spherical wave. I have been in the habit of representing Seebach's method as follows: on the co-ordinate axes *A*, *B*, *C*, *D* (*cd* being the earth surface), of the earth, let equal times be taken on *AB*, and spaces passed over in equal times on *CD*. The one represents the

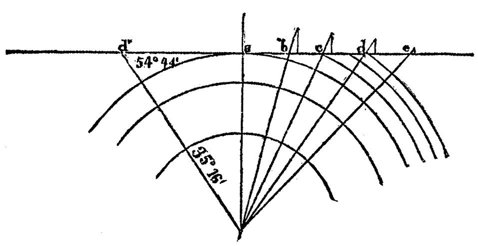

FIG. 2.

constant velocity of the spherical wave, and the other the decreasing velocity of the surface or emergent wave. By connecting these points by rectangular co-ordinates, an equilateral hyperbola is developed, the centre of which is the focus x, and the character of which depends on the depth of the focus. The hyperbola becomes more and more triangular in form as the depth becomes less (as in taking the surface at $c'd'$, $c''d''$), until, if the focus is at the surface, the hyperbola becomes a right-angled triangle; i.e., the surface wave passes over equal spaces in equal times. If, therefore, we plot accurately the times on AB, and the corresponding places on CD, we may develop the hyperbola and calculate its centre; or else by accurate trial we may find a point which shall be the centre of circles passing through corresponding times and places. That point will be the focus. Such is a very general account of the method, given in my own way. For more accurate detail, Seebach's work must be consulted.

We believe that this method, in a thickly settled country dotted over with observatories on railroad-stations where accurate time is kept, will prove the most accurate. Dutton and Hayden object to this method that it is important to have the accurate time of arrival

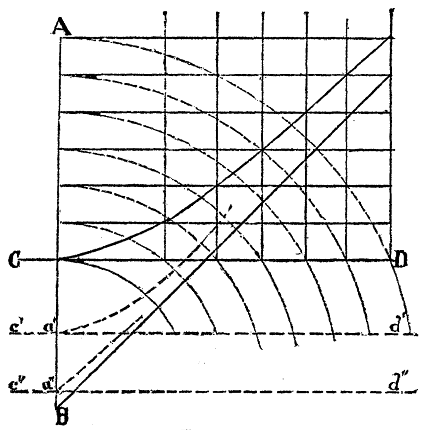

Fig. 3.

of the wave, because there are two kinds of waves, — the normal and transverse, — which run with different velocities. The answer to this is, that it is only over a comparatively small area that, on any method, observation can be relied on for estimating depth. Inspection of Fig. 3 shows that the arm of the hyberbola very soon becomes almost straight; the velocity of emergence at any considerable distance becomes sensibly the same as that of the spherical wave, and therefore can no longer indicate depth. But over the small area where the curve of the hyperbola, or change of velocity, is rapid, the time of arrival of the different waves would not greatly differ. At any rate, the use of seismographs which decompose the complex earth-motions will record these different waves separately, and thus enable us to determine the law of decrease of one of them — the normal — with accuracy.

In conclusion we would insist that we cannot any longer afford to study earthquakes without seismographs. The Geological Survey ought to have these in different parts of the country. The University of California has recently gotten three of these of the best character (Ewing's and Gray-Milne's), which will soon be set up in different parts of the State. JOSEPH LECONTE.

Berkeley, Cal., June 21.

The Corresponding Volumes, etc., of Ice and Sea-Water.

THESE determinations were made in Hudson Strait (latitude 62° 33′ 45″ north, longitude 70° 41′ 15″ west), in an inlet having a width of a little more than half a mile. I am thus particular in giving the width, because in a very narrow tidal harbor, with the ice fast on either shore, the line of flotation of the ice would sensibly alter with a rising or falling tide. In this instance I was particular in watching for such a difference, under these opposite conditions; but, if present, it was insensible.

The determination was made on Feb. 3, 1885, when the temperature of the air was — 3° F.; for the water, 26°.7 F.

A hole having been cut through the ice, of such a size as to prevent any sensible error owing to capillarity, its thickness was found to be 2 feet 9.6 inches from surface to surface; on top of the ice was an estimated average depth of snow of 3 inches, of such a density that by weight it was equal to 1.1 inches of the ice: the total thickness of ice, or its equivalent, would therefore be 2 feet 10.7 inches. Of this amount, 32.5 inches were submerged; leaving, therefore, 2.2 inches of ice, or its equivalent, above the water-line.

Therefore sea-water-ice floats with one part above the water-line and fourteen and eight-tenths below. Expressing the volume of a given quantity of sea-water by unity, its volume, when converted into ice, would be 1.0634; and their densities as 0.922 to 1.000.

W. A. ASHE.

The Observatory, Quebec, June 24.

Death of W. O. Ayres, M.D.

THE death of Dr. W. O. Ayres, one of the early members of the California Academy of Sciences, has recently been made known.

He was specially interested in the study of ichthyology, and for many years after his arrival in California, in the intervals of an extensive medical practice, contributed to this department of natural history by his investigations of and publications upon the fishes of California. At the first meeting of the academy of which there is any published record, Sept. 4, 1854, he presented descriptions of two new species, *Labrus pulcher* and *Hemitripterus marmoratus*, which still stand, though the generic status has been modified, — now *Harpe pulchra* and *Scorœnichthys marmoratus*. His contributions to the ichthyological knowledge of the Pacific coast were frequent for many years, especially from 1854, as above, to the year 1863. His scientific inquiries sometimes extended, though rarely, towards other forms of animal life. He returned to his native State, Connecticut, about 1872 or 1873. His services to the cause of science on the Pacific coast in those early days entitle him to grateful remembrance. R. E. C. S.

U. S. Nat. Mus., June 28.

Cause of Consumption.

THE experimental together with the clinical study of tuberculosis has established the view that there are three factors in its causation: —

First, The presence of the parasite, the tubercle-bacillus, as a pathogenic element. This factor is necessary for the production of the disease.

Second, Heredity figures as a prominent element in about thirty per cent of the cases ordinarily met with.

Third, Mal-hygienic and debilitating agents, such as foul air, sedentary occupations, violations of the laws of health, and diseases, have a powerful effect, by impairing the nutrition, in developing the disease.

Heredity and lowered vitality cannot of themselves produce tuberculosis, but clinically they play an important *rôle* as factors by rendering the individual more vulnerable to bacillary influence.

FRANK DONALDSON, M.D.

Baltimore, Md., June 29.

Volapuk.

YOUR correspondent, 'H. T. P.,' in your issue of the 24th of June, asks for information about Volapük. I can refer him to a most interesting article upon this subject, which appeared in the *Bulletins de la Société d'Anthropologie de Paris*, 1885, pp. 317–321. The article is by M. Kerckhoffs, who has published the following work, 'Cours Complet de Volapük,' par A. Kerckhoffs (Paris, 1886), and contains a sketch of the structure of the language and some interesting information about its prospects, progress, etc. A. F. CHAMBERLAIN.

Toronto, June 28.

Queries.

7. DEATHS AND THE TIDE. — A physician living near the sea states that during the past five years he has noted the hour and minute of death in ninety-three patients, and every one has gone out with the tide, save four who died suddenly by accident. Is there any other evidence to sustain this statement? — D.