Brooklyn Entomological Society has appointed a committee to welcome the members of the club, and to assist in making the meetings interesting, as well as to give such information regarding matters of special interest to entomologists as may be desired. The same society will arrange for one or more field-excursions in the vicinity of New York, and a reception will be arranged for. Members of the club intending to contribute papers will please communicate the same to the president, Prof. J. H. Comstock, Ithaca, N.Y., or to the secretary, Mr. E. Baynes Reed, London, Ontario.

— The Botanical Club of the American Association will hold its meetings, as usual, during the week of the association. For particulars address Mrs. E. L. Britton, secretary of the club, Columbia College, New York.

— The Society for the Promotion of Agricultural Science will hold its eighth annual meeting in New York, beginning on Monday evening, Aug. 8, at Columbia College, and continuing on Tuesday. For further information address Prof. W. R. Lazenby, secretary, Ohio State University, Columbus, O.

— The aggregate production of shad for distribution the present season by the United States Fish Commission has been enormous. The number produced has been increasing from season to season, owing to the perfection of the methods in use. A summary of the distribution for the present season, arranged by river-basins, is as follows: —

Penobscot River	1,169,000
Kennebec River	800,000
Tributaries of Narragansett Bay	1,275,000
Hudson River and tributaries	1,979,000
Tributaries of Delaware Bay	5,099,000
Tributaries of Chesapeake Bay	68,149,000
Tributaries of Albemarle Sound	5,322,000
Tributaries of South Atlantic coast	3,566,000
Tributaries of Gulf of Mexico	7,048,000
Inland waters	1,014,000
Total	95,421,000

It will thus be seen that over 68,000,000 young shad-fry have been returned to the waters of Chesapeake Bay. The entire production of the fisheries of the Chesapeake for the present season was about 2,000,000 young shad. It is therefore evident, that, for every mature shad taken from the waters of the Chesapeake, thirty-four young, healthy, and vigorous shad have been returned to those waters. Experiments already made by the commission indicate, that, up to the close of their river-life (the young shad migrating in October), twenty per cent of the fry placed in our rivers will survive, and attain a size of from two to three inches in length. Arrangements have been made by the commission to secure complete statistics of the shad-catch all along the entire coast for the present year, similar statistics having already been collected in 1885 and 1886. Information already in the hands of the commissioner makes it certain that the aggregate production of shad on the coast has been larger the present season than at any time in the last twenty years, but it will be impossible to give the measure of increase. For the Potomac River it is already assured that the increase of 1887 is fully 100,000 shad over that of 1886, and the increase of 1886 over that of 1885 exceeded 100,000. In the Potomac fisheries alone in the last two seasons the increase in shad has been over 250,000; the increase representing a much larger number than the entire catch of 1879, in which year the fisheries of the Potomac reached their lowest ebb.

— Professor Riley, the entomologist of the Department of Agriculture, has made public the result of an exhaustive personal investigation into the habits of the *Phorodon humili*, or hop-louse. His discoveries are expected to prove of great value to hop-growers, as he has succeeded in learning the habitation of this plant-pest during the winter months, and tracing it through the varying stages of insect-life. Before the investigation, it was not known how or where the insect survived the winter. As a result of his inquiries, Professor Riley has satisfied himself that the eggs laid by the female at the close of the summer are deposited in plum-trees, where the insect hatches in the spring, and resides until the third generation. This third brood, unlike its predecessors, is winged, and immediately after development abandons the plum-tree and attacks the hop-vine. In the autumn a counter-migration from the hop-vine to the plum-tree occurs, the winter eggs are deposited, and the cycle of life goes on in the same way. It is a notable fact that in regions where the cultivation of hop-vines is a new industry, the growers have had complete immunity for a while from the pest. In California to-day they are not troubled by it. Professor Riley believes that the *Phorodon humili* has been brought to this country from Europe on plum-stock; and there is reason to believe that the *Phylloxera*, the dreaded grape-pest, was carried from this country to Europe on grape-vine cuttings. Therefore California hop-growers are warned to beware of importing plum-stock from eastern hop-regions. These discoveries render it possible to check the ravages of the hop-louse either by the use of insecticides in the springtime, before the insect has reached the winged state, or by the destruction of the sheltering plum-trees. The experiments will be continued with a view to protecting the hop-vines after they have become infected with the hop-louse.

— The project of holding a summer school of physics at Harvard College this season has been abandoned; but on July 19 and 20 apparatus designed for use in the 'forty-experiment course,' preparatory for admission to Harvard College, will be shown to teachers or others at the Jefferson Physical Laboratory, and questions relating to the experiments will be answered. The same thing will be done for the 'sixty-experiment course' on the second day, July 20.

LETTERS TO THE EDITOR.

*** The attention of scientific men is called to the advantages of the correspondence columns of* SCIENCE *for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.*

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

Theoretical Meteorology.

A REVIEW of Professor Ferrel's recent work on this subject in *Science* for June 3 furnishes an opportunity to present a few points on this subject. Professor Laughton, ex-president of the Royal Meteorological Society, once said that there was hardly a theory in meteorology that was well established. If this be so, it seems to me there is great danger of putting too much reliance upon mere theory, which does not have a sufficient groundwork of facts. There is special danger of this in meteorology, where the mathematical discussions of gaseous movements and vortices are hedged about with so much difficulty and complication. I am well aware that the views here advanced are opposed to those of many most advanced thinkers in this field, and I only ask an unbiassed hearing.

To my mind there are at least two fundamental errors in this subject, but these are intimately interwoven throughout its warp and woof. These are, first, that there is friction only between the air and the earth, or at least that friction between contiguous air strata may be neglected; second, that conditions and changes of pressure, temperature, and moisture in the atmosphere, are the only causes acting in producing either its general motions or storms.

The objections to the first theory are briefly as follows. At a height of 100 feet, or at the most 200 feet, in a level country, there is no longer friction between the air and earth, but rather between air and air. This is especially the case on the ocean; and here, surely, we would have no waves, if it were not for the friction between air and air. If there were no friction, all storms would take place in a virtual vacuum, and into a vacuum air would tend to flow with about the velocity of sound. Professor Ferrel thinks, that, according to laws of gaseous motion, the earth's atmosphere would leave the poles and heap itself at the equator, but this is prevented by friction with the earth's surface; but, as we have just seen, we need consider only friction of air on air at 100 feet elevation.

The objections to the second theory cannot be set forth as easily as the above. When we are gravely told that the sun heats up a certain portion of the earth's surface, and that in consequence vertical currents are set up which finally bring about a wind of 100 miles an hour, we can but be credulous. As a matter

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.