troublesome problem of how to treat pamphlets in professional libraries." EDWARD S. HOLDEN.
Washburn observatory, Oct. 19.

An attempt to photograph the corona.

By a slip of the pen in my communication on this subject in the last number of *Science*, I gave the references to two previous letters as April 29 and April 13. These should read May 29 and May 15. By a typographical error I am made to refer the observations on the light of the corona to Prof. S. P. Langley. The observations were made by his brother, Prof. John W. Langley.

According to his observations, as we have already seen (*Science*, August 14), the light of the corona within 1′ of the sun's disk is six times that of the full moon; which, according to my observations, would be one-fiftieth that of our atmosphere in this vicinity. Professor Bonney states, (The sun, p. 229.) that a brilliancy of only one sixty-fourth would be sufficient to render the planets visible. Therefore, even if the atmospheres of Mercury and Venus produced no visible effect at all, the facts would still sustain Professor Langley's observations.

WM. H. PICKERING.

Recent Proceedings of Societies.

Philosophical society, Washington.

Oct. 24.—Mr. H. A. Hazen read a paper on condensing hygrometers and sling psychrometers. As preliminary to the paper proper, Mr. Hazen gave the results of some interesting experiments which he had made with a view of determining the most desirable distance between the lines upon the stem of a thermometer, in order that tenths of degrees may be estimated with the greatest accuracy. He had made a considerable number of trials, in which he had first estimated the fractions, and afterwards measured them by the use of a vernier. The results seemed to indicate that there was a length of division on which the estimation of tenths might be made with greater precision than on one either longer or shorter. Mr. Hazen did not consider, however, that his experiments were sufficiently numerous to enable him to determine this with certainty. On directly addressing himself to the subject of his paper, he called attention to the various forms of Regnault's condensing hygrometer, which had appeared from time to time, briefly discussing the advantages and disadvantages of each. He spoke of the numerous methods which have been devised for ventilating the psychrometer, and expressed his belief that the form known as the 'sling' was the best of all. Experiments made by using both of these instruments for the purpose of determining the value of the constant A in the common psychrometric formula were described, and the effect of elevation was considered. ——Mr. Mendenhall exhibited one of Sir Wm. Thomson's long-range voltmeters, which had been recently imported by the chief signal officer. A small cylinder of soft iron hangs upon the short arm of an index lever, which is so balanced as to be practically in indifferent equilibrium. The iron is surrounded, without contact, by a coil which is so wound that the strength of the field produced by the passage of a current increases from the lower to the upper end of the coil. As the pull on the soft iron is proportional to rate of the change of the square of the strength of field, and as this rate diminishes from the lower to the upper end of the coil, the force exerted on the cylinder will depend at once on the current strength and on its position in the coil. By hanging a small non-magnetizable weight to the iron by means of a hook projecting from the lower end, this force is made to be constant when equilibrium exists, so that when the current strength varies, the position of the iron cylinder changes, and this position is read off on a scale at the extremity of the long arm of the lever. The great merits of the instrument are its constancy and the ease with which it may be adapted to the measurement of potentials differing greatly in magnitude.—Mr. Mendenhall also made some remarks upon instruments and measurements of the so-called re-action time, originating in the exhibition of these instruments by Dr. Matthews at a previous meeting of the society. He referred to a paper upon the subject, which he had published in the *American journal of science*, in 1871, in which instruments and methods are described identical in many respects with those recently exhibited. Results were given, showing the time occupied in responding to a signal, which was a flash of light, the appearance of a card, a sound, or a blow upon the hand or head, and also the time consumed in the simplest processes of reasoning. These times were, in general, shorter than those recently obtained by Dr. Matthews, but differences in the manner of conducting the experiment will doubtless account for this.——Mr. Harkness discussed the flexure of transit instruments. He pointed out its dependence on the form of the instrument, and also that its amount might be expressed as the sum of two different functions of the zenith distance. The nature of one of these functions can be readily ascertained; but unfortunately that of the other is unknown, and, in a general sense, impossible to determine. For certain classes of instruments it might be ascertained by the assumption of accuracy in tables of star positions, but Mr. Harkness declared that he knew of no way by means of which the problem could be completely solved for the astronomical observatory. Discussion of this paper was prevented by the operation of a rule of the society, in obedience to which it closes its session promptly at ten o'clock. In a general way, it cannot be denied that a strict compliance with this rule has many advantages. A visitor to the society may be certain that he will not be obliged to wait for more than a minute or two after eight for its beginning, and, however uninteresting to him the dissertations to which he listens may be, he may console himself by the reflection that a limit is set to their duration.

Academy of natural sciences, Philadelphia.

Oct. 20.—A communication was read from Mr. W. N. Lockington on the causes of elevation and depression of the earth's surface, with special regard to the setting of loosely compacted sediments by pressure of superincumbent beds. Some have supposed that the great beds of ice which encumber the poles bear down the surface rocks of the region by their weight. It is, of course, possible that a downward movement of the earth's crust may be caused by strata piled upon it; but as the earth's contraction is a sufficient cause for all such movements, it is useless to postulate other causes. The extreme of possible compactness, however, is reached in the sediments themselves by the

pressure of their own bulk. When leaves, stumps of trees, etc., are found in a delta several feet below the sea level, subsidence by a downward movement of the earth's crust is usually invoked as a cause. It is forgotten that the weight of the upper strata of the delta has consolidated the lower, and gradually pressed them down. We know that a dirt bed in the older strata is but a line, while existing dirt beds are many inches or even feet in thickness. Sand becomes sandstone by pressure, but a hundred feet of sand will by no means form a hundred feet of sandstone. The alternations between fresh water and marine strata, the changes from land to shallow lake or sea, which marked the age of the coal-measures and caused the production of coal, need not have been, and probably were not, caused by the alternate rising and sinking of the actual crust of the earth. It is far more likely that they were produced by the settlement or consolidation of the strata themselves. Regularity of settlement would depend on regularity of deposition, which was impossible from the nature of the case. The deposition of vegetable material and the formation of coal were dwelt upon.——Prof. Heilprin remarked that geologists regarded the alternation of coal and dirt beds as indicating recurrent periods of stability, rather than alternating depression and elevation. The stability of the ocean bed and the formation of deltas were considered.——Mr. Jos. Willcox described the effects of glacial action as observed by him north of the St. Lawrence. The farther north we proceed, the less soil would be found on the rocks, depending on the lessened amount of moraine deposited by the glacier as it retreated toward the pole. The scorings on the rocks in the region visited by him were from the northeast to the southwest. He believed that the oldest or Laurentian rocks were at one time covered with sedimentary deposits which had been worn away by glacial action. In some places patches of such deposits containing fossils were yet to be found.——Prof. Heilprin believed that the truth was yet to be ascertained regarding the direction of glacial movement in Labrador. He was inclined to believe that glacial action started below the great circumpolar ice-sheet, and proceeded both south and north from the point of departure. He referred to his communications made some years ago, in which he objected to the belief that glacial action, as we know it, was owing to movements of a large body of ice from the north pole. The results of the most recent arctic investigations on the condensation of moisture and the existence of a cloud limit were given.

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