SCIENCE.

FRIDAY, NOVEMBER 14, 1884.

COMMENT AND CRITICISM.

THE CUSTOM-HOUSE at Philadelphia and the Treasury department at Washington are wrestling over a difficulty growing out of the special tariff upon 'philosophical instruments.' Such instruments are, under the law of 1883, subject to a duty of thirty-five per cent, while instruments of glass or metal, not 'philosophical,' are subject to forty-five per cent duty. Where can the line be drawn? An astronomical telescope is evidently philosophical, as the word goes. But there are instruments of every grade, from the 26-inch equatorial to the little glass through which the opera-goer contemplates the movements of his favorite *prima donna*: shall they all be classed together? If not, who can define what telescopes, spyglasses, binoculars, lorgnettes, microscopes, and other instruments for aiding vision, are entitled to patents of nobility which shall distinguish them from the plebeian mass of 'manufactures not specially provided for in this act'? Of course the same question arises in the case of chemical and physical instruments of all sorts, which may be used either in a laboratory, private or public, in a factory or telegraph-office, or in a children's playroom. It is understood that the aid of the National academy of sciences will be invoked to furnish a solution of the problem, and the result will be looked for with great curiosity.

THE PROBLEM, how to make scientific assemblies more profitable to those who attend them, is constantly recurring. It is conceded that the more profound and special a paper may be, the fewer will be the number of those who take an interest in hearing it. On the other hand, if those who are special and profound are not to be encouraged to present their papers to scientific associations, who shall have the privilege? Certainly not the vague and the shallow. Papers must be presented, as elaborate and recondite as can be secured; but such papers repel the auditors. What shall be done in the dilemma? How can the mathematician presenting some new development of the theory of functions expect to interest the botanist? How can the petrographer discussing the microscopic aspects of rocks command the attention of the morphologist? Or, in a philological association, how can an elaborate paper on some point in the grammar of the Vedas command the attention of linguists who have never learned the Sanscrit alphabet? Has the advancement of knowledge reached such a point that there is no place left for the general society, the academy of science, and is specialization to be so special that each line of inquiry is to be considered only in a limited company of those who are devoted to it?

We venture to make a few suggestions which seem to us worth considering by those who are called upon to manage scientific meetings, especially the annual gatherings which bring from a great distance, at a great expense, those who are desirous of securing the utmost advantage from the meeting. *First*, Let the committee in charge make arrangements of a positive character for the conduct of the meeting, and require conformity to their regulations. Among these rules should be, (1) a strict adherence to the allotted time; (2) the presentation, in advance, of an abstract of what is to be read (and this should be printed, particularly if it contains any tabular statement, mathematical formulas, chemical formulas, or other rigidly technical statements); (3) the allowance of a definite time for discussion, questions, answers, and comments. *Second*, Let every speaker or reader form the habit of stating in general terms the purpose of his investigation, its relations to other work, and its results, refraining from going into minute details unless he is sure that a considerable part and, it may be said, the oviparity of the monotremes firmly established, the fact had been authoritatively proclaimed. Sir John Jamison, for instance, especially declared that 'the female is oviparous, and lives in burrows in the ground' (*Trans. Linn. soc. London*, xii. p. 585). The Rev. Dr. Fleming, in his 'Philosophy of zoölogy' (ii. 215), published in 1822, remarked, that, "if these animals are oviparous (and we can scarcely entertain a doubt on the subject, as *the eggs have been transmitted to London*), it would be interesting to know the manner of incubation." Further, Fleming refused to admit the monotremes among the mammals, dividing the Vertebrata 'with warm blood' into 'quadrupeds' and 'birds,' and the former into 'I. Mammalia' ('1. Placentaria' pedota and apoda, and '2. Marsupialia'), and 'II. Monotremata.'

But, notwithstanding all these facts, scepticism as to the truth of the representations and authenticity of the eggs, developed into positive disbelief; and Bonaparte himself recanted, and took that decidedly retrograde course, which others had entered upon, of associating the monotremes with the marsupials in the unnatural and artificial negative group of Ovovivipara, or Implacentalia. I, too, was so far influenced by the prevalent scepticism or disbelief, and by the similarity of the monotreme egg to that of a reptile, that I retained viviparity as a special attribute of the mammals in 1872, although I declined, on other evidence, to include a small size for the eggs in my diagnosis of the class. I then, also, adopting the subclasses Monodelphia, Didelphia, and Ornithodelphia, segregated them into the major groups, combining the first two under the name Eutheria, and contrasting the last as the Prototheria. These names have since been accepted by Professors Huxley, Flower, and others; and, inasmuch as Professor Huxley did not accredit their origin, they have been ascribed to him. I must add, however, that Professor Huxley has restricted the name Eutheria, although apparently with a hypothetical qualification, to the monodelphs, while he has coined a new name (Metatheria) for the marsupials. I fail to appreciate the need for such modifications, which virtually become exact synonymes of Monodelphia or Placentalia, and Didelphia.

Finally, the old data as to the oviparity of monotremes became almost lost to memory, so that no one has recalled them since the rediscovery. In view of such forgetfulness and scepticism, therefore, further information was necessary to insure the admission of the old evidence as valid. But Mr. Caldwell has further added the intelligence, quite new, that the eggs of Ornithorhynchus are meroblastic. This discovery will have an important bearing on the question of the origin of the mammals, and is antagonistic to the suggestion of Professor Huxley that the type was a direct derivative from the amphibians, while it increases the possibility that Professor Cope may be nearer the truth in affiliating the ancestors of the mammals to the theriomorphous reptiles of the Permian.

THEO. GILL.

Sun-spots.

The long-delayed maximum of solar spots, now undoubtedly passed, has attracted unusual attention to the spot-periodicity. To-day and yesterday the visible hemisphere of the sun was, for the first time in nearly fourteen months, observed to be entirely free from spots; the occasion next preceding this being 1883, Sept. 25. During the past two years, the only additional days on which the sun was observed to be without spots were, in 1882, Oct. 9 and Dec. 3, and, in 1883, Feb. 23, and May 25, 26, 27, and 28.

DAVID P. TODD.

Lawrence observatory, Amherst, Mass., Nov. 3.

The numerical measure of the success of predictions.

Suppose we have a method by which questions of a certain kind, presenting two alternatives, can in every case be answered, though not always rightly. Suppose, further, that a large number of such answers have been tabulated in comparison with the events, so that we have given the following four numbers:—

(aa), the number of questions for which the answers were the first way and the events the first way;

(ab), the number of questions for which the answers were the first way and the events the second way;

(ba), the number of questions for which the answers were the second way and the events the first way;

(bb), the number of questions for which the answers were the second way and the events the second way.

Then the problem is, from these data to assign a numerical measure to the success or science of the method by which the answers have been produced. Mr. G. K. Gilbert (*Amer. meteorological journal*, September, 1884) has recently proposed a formula for this purpose; and I desire to offer another.

I make use of two principles. The first is, that any two methods are to be regarded as equal approximations to complete knowledge, which, in the long-run, would give the same values for (aa), (ab), (ba), and (bb). The second principle is, that if the answers had been obtained by selecting a determinate proportion of the questions by chance, to be answered by an infallible witness, while the rest were answered by an utterly ignorant person at random (using *yes* and *no* with determinate relative frequencies), then the approximation to knowledge in the answers so obtained would be measured by the fraction expressing the proportion of questions put to the infallible witness. The second witness may know *how often* he ought to answer 'yes;' but I give him no credit for that, because he is ignorant *when* he ought to answer 'yes.'

Let i be the proportion of questions put to the infallible witness, and let j be the proportion of questions which the ignorant witness answers in the first way. Then we have the following simple equations:—

$$(aa) = i\{(aa) + (ba)\} + (1-i)j\{(aa) + (ba)\},$$
$$(ab) = (1-i)j\{(ab) + (bb)\},$$
$$(ba) = (1-i)(1-j)\{(aa) + (ba)\},$$
$$(bb) = i\{(ab) + (bb)\} + (1-i)(1-j)\{(ab) + (bb)\}.$$

Now, whatever the method of predicting, these equations can always be satisfied by possible values of i and j, unless the answers are worse than if they had been taken at random. Consequently, in virtue of the two principles just enunciated, the value of i obtained by solving these equations is the measure of the science of the method. This value is,

$$i = \frac{(aa)}{(aa)+(ba)} - \frac{(ab)}{(ab)+(bb)},$$
$$= \frac{(aa)}{(aa)+(ba)} + \frac{(bb)}{(ab)+(bb)} - 1,$$
$$= \frac{(aa)(bb) - (ab)(ba)}{\{(aa)+(ba)\}\{(ab)+(bb)\}}.$$

Mr. Gilbert's formula has the same numerator, but a different denominator. It is, in the present notation.

$$i = 2\frac{(aa)(bb) - (ab)(ba)}{\{(aa) + (ab) + (ba) + (bb)\}^2 - (aa)^2 + (ab)^2 + (ba)^2 - (bb)^2}.$$

For Sergeant Finley's tornado-predictions, $(aa)=28$, $(ab)=72$, $(ba)=23$, $(bb)=2,680$. From these data, Mr. Gilbert finds $i = 0.216$, while my formula gives $i = 0.523$.

If the questions should present more than two alternatives, it would be necessary to assign relative values or measures to the different kinds of mistakes that might be made. I have a solution for this case.

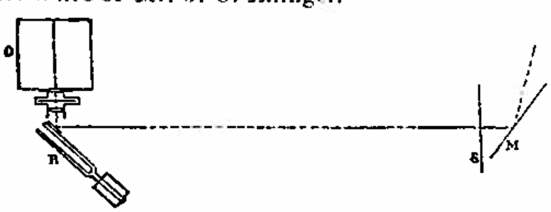
Another problem is to measure the utility of the method of prediction. For this purpose, let p be the profit, or saving, from predicting a tornado, and let l be the loss from every unfulfilled prediction of a tornado (outlay in preparing for it, etc.); then the average profit per prediction would be,

$$\frac{p\,.\,(aa) - l\,(ab)}{(aa) + (ab) + (ba) + (bb)}.$$

C. S. Peirce.

Measurement of the speed of photographic drop-shutters.

The usual method adopted for this purpose depends on photographing a white clock-hand revolving rapidly in front of a black face.[1] The chief difficulty in this case is to maintain a uniform rotation at high speed. To avoid this difficulty, and to determine the uniformity of exposure of any particular shutter under apparently like circumstances, the following method has been suggested. In carrying out the experiment in practice, I have had the assistance of Mr. J. O. Ellinger.



A tuning-fork, B, with a mirror attached to the side of one of the prongs, is placed in front of the camera-lens. This mirror is so arranged as to reflect into the camera, C, a horizontal beam of sunlight, which, before reaching the fork, has passed through a half-inch hole in a screen, S, placed about ten feet distant. This produces on the ground-glass a minute brilliant point of light. If the fork be set vibrating, the point will become a short, fine, horizontal line: if the fork be rotated about its longitudinal axis, the line will become a sinusoidal curve described on the circumference of a circle of long radius. A photographic plate is now inserted, and the drop-shutter attached. On releasing the latter, it will be found that a portion of the sinusoid has been photographed; and the precise exposure may be determined by counting the number of vibrations represented on the plate.

The mirror employed should be somewhat larger than the lens to be measured, so as to cover its edges during the whole exposure. The mirror may be glued directly to the prong of the fork with strong carpenter's glue, after first scraping off a little of the silvering at the edges of the glass. The rate of the fork is then determined, by comparison with a standard fork, by the method of beats. W. H. Pickering.

Photographic laboratory,
Mass. inst. of technology.

[1] For other methods, see *Brit. journ. photography*, Aug. 31, 1883, and May 23, 1884.

THE IMPORTANCE OF CHEMISTRY IN BIOLOGY AND MEDICINE.

The position of chemistry in the biologic sciences has long been, in English-speaking communities, a very indefinite one: in fact, it may be questioned whether the science has, even at the present day, any generally recognized position among biologists themselves. That this has been the case for many years, even in Europe, is evident from the fact that until recently the published results of investigation in the field of physiological chemistry have had to be sought for in widely diverse places. Many papers have been published in purely chemical journals, others in journals devoted to physiology, while still others have appeared in so-called 'natural-history' journals, — a fact which in itself plainly indicates the past status of this branch of science.

There can be no question that physiological chemistry should occupy a definite place among the biological sciences. Biology is confessedly a study of life, and, as such, has to do with the development, structure, and function of living organisms. The first two of these we suppose to be included under the heads of embryology and morphology: while the third, constituting, in the words of Herbert Spencer, "the second main division of biology, embracing the functional phenomena of organisms, is that which is in part signified by physiology." Further, "that part of physiology which is concerned with the molecular changes going on in organisms is known as organic chemistry," or, with equal propriety, as physiological chemistry; hence a study of the functions of the body, to be at all complete, must include a study of the chemical changes incident to life, and cannot be restricted to the purely physical phenomena of the organism. Yet it is very noticeable that wherever 'biology' is taught in this country, even in the most liberally conducted institutions, where the course of study embraces embryology, animal and vegetable morphology, experimental physiology, etc., physiological chemistry is rarely mentioned.

We need to inquire whether this is due to a