SCIENCE.

FRIDAY, AUGUST 15, 1884.

COMMENT AND CRITICISM.

A MARKED feature of recent scientific work is the tendency to international co-operation. Problems too large to be undertaken by a single institution, or even by one nation, are thus successfully solved. Two examples suggest themselves. The first of these is the largest piece of astronomical work yet undertaken. Since 1870 a dozen observatories have been actively engaged in preparing a catalogue of about a hundred thousand stars in the northern hemisphere, a part of the sky being assigned to each observatory. The Greely expedition recalls the second example. This was one of a dozen expeditions fitted out by various governments to secure simultaneous meteorological observations for one year at different points within the arctic circle. Other examples might be added, all tending to show that co-operation is likely to yield results of lasting value.

We have on several occasions called attention editorially, or through our contributors, to the advantages likely to follow the organization of an international scientific association properly formed; and the responses which have come to a recent appeal are to-day referred to in our notes. Besides the inspiration the individual members would gain from attendance at its sessions, such a society would inspire great confidence in the work that it might undertake. It would then become comparatively easy to secure proper means for investigation. Observers, too, would be much more willing to aid in a research in which there was little danger of needless duplication.

A CORRESPONDENT calls our attention to the omission of the Henry Draper medal in our brief list of honors founded in this country for scientific research. Both this and the Watson medal were overlooked; as we were under the impression that the gifts of Mrs. Draper and Professor Watson were wholly in aid of, rather than as rewards for, research. This last is the case in part with the Watson fund, the income of which is directed to be expended 'for the promotion of astronomical science.' But in making the National academy of sciences his residuary legatee, Mr. Watson also provided that a gold medal of the value of one hundred dollars, with a further gratuity of one hundred dollars, should be given "from time to time to the person in any country who shall make any astronomical discovery, or produce any astronomical work, worthy of reward, and contributing to our science." The fund is of recent date, and no award of the medal has yet been made; but a part of the expenses of the eclipse expedition to Caroline Island was paid from the fund.

The fund given by Mrs. Draper to the national academy, to commemorate one of its members, the late Dr. Henry Draper, is also very recent, and no award has yet been made. A gold medal of the value of two hundred dollars is to be awarded, not oftener than every two years, 'to any person in the United States of America, or elsewhere' (with preference, other things being equal, to an American), 'who shall make an original investigation in astronomical physics' meriting such an award. This award, like the Lawrence Smith medal, can be given only for investigations made or published since the last preceding award.

One is tempted to speculate on the comparative value of funds given in direct aid of scientific research, and of medals or gratuities rewarding successful discovery or searching investigation. The former, as the endowment of research, must surely produce the more immediate practical results; while the latter signalize the victories of science, and, when properly administered, direct public attention to what is of true value. But in the probable

extension of such foundations as the latter in this generous country, does there not lurk a possible danger, — a danger that their bestowal will fall into hands incapable of proper administration? If any one think this danger remote, let him reflect on the ill-judged selection of recipients for honorary degrees in many of our best universities and colleges. Let such foundations remain, as now, in the hands of those whose position has been gained solely by research, and the danger vanishes.

THE standard of light adopted by the Paris electrical conference last April is the amount of light emitted by a square centimetre of melted surface of platinum at the point of solidification. It was believed that advantage could thus be taken of a physical constant (namely, the melting-point of platinum) upon which could be based all our present changing and unsatisfactory photometric standards. The adoption of this standard has been much criticised, for it does not seem to lend itself easily to actual photometric tests. Werner Siemens proposes that a piece of platinum foil should be enclosed in a cavity provided with a conical opening 0.1 of a square centimetre; this piece of platinum to make part of an electrical circuit, the current in which can be so regulated that a comparison with any light can be made at the moment of fusion. The temperatures of solidification and fusion of platinum do not differ sensibly from each other, and Siemens believes that the error introduced by taking the temperature of fusion instead of that of solidification would be small. The use of an electrical current to produce fusion has certain advantages, for the time of fusion can evidently be deferred until the proper moment. Preliminary experiments have shown that the light emitted from an opening 0.1 of a square centimetre in section by Siemens's method is equivalent to nearly one and a half standard English candles.

Although the standard adopted by the Paris conference seems to be based upon the unalterable laws of matter, it does not seem as if it would ever be practically adopted. Some form of the modern incandescent electric light, it seems to us, would afford a much better prospect of a standard light. It is difficult to maintain the steadiness of such a light for photometric purposes; but this does not seem impossible to accomplish. It is evident, that, if we could maintain an electrical current constant through a platinum wire or carbon filament in a suitable medium, we should have the means of reproducing the same amount of heat, and therefore light, from the same area. Unfortunately, carbon changes in resistance at the point of incandescence; and the resistance of platinum is not invariable under repeated heating and cooling in a comparative vacuum. An exhaustive investigation of the peculiarities of platinum or of iridium, under the effect of incandescence produced by the electrical current, would seem to be desirable before the French standard is accepted as a finality.

LETTER TO THE EDITOR.

Tornado predictions.

IN an article on 'Tornado predictions,' published in the July number of the *American meteorological journal*, a table of verifications is given, in which the average of successful predictions for several months is from ninety-six to ninety-eight per cent.

An examination of the table shows that this remarkably high percentage of verification is largely made up, *not* of successful predictions of tornadoes, but of successful predictions of no tornadoes. In justification of this method of verification, the writer says, "It requires as much and often more study to say that no tornadoes will occur, as to make the prediction that conditions are favorable for their development." If this explanation be accepted as satisfactory, what do the verifications signify?

A little consideration will show that the *absolute* value of these figures gives no basis from which to judge of the real success of the tornado predictions. The averages of ninety-six and ninety-eight per cent are mainly functions of the non-tornado days, with but slight modifications for the success or failure of the prediction of actual tornadoes. An ignoramus in tornado studies can predict no tornadoes for a whole season, and obtain an average of fully ninety-five per cent. The value of the expert work must, therefore, be measured by the excess which is obtained over the man who knows nothing of the subject. This is the only way to determine any significance in the method of verification above described. The excess is but one or two per cent, and poorly exhibits the present stage of progress in tornado studies. The injustice which is done is to be found in the method of verification adopted. In ascertaining the value of tornado or any other special storm predictions, the consideration of days on which no storms occur, and none are predicted, is entirely beside the question.

If the writer of 'Tornado predictions' will give the verifications obtained from positive predictions, and