

subsequent consideration at all, we may suggest a very different direction in which they might be modified in actual fact, and in which their spirit would yet be even better represented than by a literal fulfilment. It was pointed out that in taking the photographic plates of stars down to the 14th magnitude in parts of the sky where brighter stars existed, these with the exposure necessary to obtain the 14th magnitude would be very much over-exposed. And it was suggested that it would be advisable to take a second series of plates, as already mentioned (see Resolution 17). Now in some parts of the sky no second series of plates are, from this point of view, at all necessary; whilst in others not one or two, but many series of plates would be necessary in order to do justice to the various magnitudes in that particular part of the sky. For the present this is not the point at issue, but it may serve as an illustration of the sort of interpretation of the resolutions which we should consider legitimate.

In order to come to a proper judgment on the legitimacy of the derivation of Dr. Gill's proposal from the resolutions it is necessary to make some statements, which are not new, but of which the true significance does not seem to have been universally appreciated:—(1) When the plates are obtained they are actual representations of the stars as existing at a given time, and for every purpose except spectrum analysis are as good, if not better, than the visible heavens. If with these plates we have the absolute places of a certain small number of known stars, we have then all the data to make them valuable, either in the present or in the future. (2) The many questions concerning the stars which it is hoped a photographic chart of the heavens would do a great deal towards settling, such as their distribution, their proper motions, their changes of magnitude, and the presence of minor planets, of new stars and the like, can all be best treated by a direct comparison of plate with plate, in any of the various ways in which this can be done. (3) In order to obtain the best results from such an agent as photography it is necessary to use it in its own proper way; and astronomers must recollect that old methods of procedure adapted to other instrumental means may most probably be out of place. We might considerably enlarge on these statements, but for our present purpose it is sufficient to call attention to them.

Now, if Dr. Gill's catalogue were successfully constructed—and there are, alas, many difficulties in the way—its utility in the direction of comparison of our sky with that of the future is wholly limited by one condition, that in the future another exactly similar catalogue be constructed, occupying a similar time. Even then, if any changes were found by means of this comparison of catalogues which might very well be made in the course of fifty or one hundred years, the natural and indeed the proper thing to do would be to immediately compare the original plates. But can it be possible that any man or number of men really think of dealing with such a subject in such a way? If, on the other hand, the object of a catalogue be merely to allow of comets, minor planets, and other bodies being located, surely it would be better to measure the plates as occasion arises, and not to catalogue 2,000,000 stars on the off-chance of having some twenty or thirty positions to settle in the course of a year. And, further, such a catalogue would have this enormous disadvantage, that whilst in some parts of the sky stars of the 11th magnitude would be fairly well spread, in the Milky Way we should have stars clustered in such enormous quantities that it would be an extremely difficult thing to even identify them: in fact, speaking roundly, we should say that if such a catalogue were made, two-thirds of the stars catalogued would lie in the Milky Way. If, contrary to the opinion we have expressed, it is decided to form a very large catalogue, surely it would be better to determine the places of a certain number of stars, of such magnitudes as are found available, in each square

degree, and make these the reference stars from which the positions of the other stars on the plate could be obtained.

We are therefore of opinion that, supposing limitless time and money available for such a purpose, the advantages of constructing this catalogue would be doubtful; but even if we waived all these objections and agreed that such a catalogue would be a "nice thing to have," or admitted that since men of the ability and reputation of Admiral Mouchez and Dr. Gill consider such a catalogue necessary it is heresy to inquire the why and wherefore, there would still be left the serious objection that to form a chart of the heavens is the first thing to do, and, take it in as simple a form as possible, it will quite possibly tax the energies of astronomers to their utmost; and that stellar photography being as yet in its infancy it is suicidal to attempt anything which will commit us to a course of action extending over more than a very few years. We could not give a better illustration of the dangers of the opposite procedure than has been supplied by Admiral Mouchez himself. In a recent article he has suggested that there have lately been such improvements in the sensitiveness of plates that we could now go to the 15th magnitude instead of the 14th. With a little ingenuity and less arithmetic it could easily be shown that the whole plan of operations would have become hopelessly futile and obsolete before half the time allowed by Dr. Gill for its completion had elapsed.

But not for one moment do we wish to appear lacking in sympathy with those who have spent and are spending so much time and thought on this subject; it is our great anxiety for the success of the work in which they are co-operating which makes us eager to protest as far as we can against the grand mistake of attempting too much.

THE INCURVATURE OF THE WINDS IN TROPICAL CYCLONES.

THE question of the incurvature of the winds in tropical cyclones is one of such importance to mariners, to enable them to judge their position in a storm, and to escape the hurricane around the central calm, that no apology is needed for adding my independent testimony to that of Prof. Loomis, whose conclusions, given at length in his recent well-known memoir, "Contributions to Meteorology," are quoted in Mr. Douglas Archibald's paper on M. Faye's work "Sur les Tempêtes" in last week's NATURE (p. 149).

In the preparation of a forthcoming work on the weather and climates of India and the storms of Indian seas, I have lately had occasion to re-investigate the above question in the case of cyclones in the Bay of Bengal, on the evidence afforded by the numerous original memoirs and reports prepared by Messrs. Willson, Eliot, Pedler, and other officers of the Indian and Bengal Meteorological Departments; my object being the practical one of determining directly the bearing of the storm-centre from a ship's position; and instead, therefore, of measuring the angle between the wind direction and the nearest isobar, as was done by Prof. Loomis, I have measured with a protractor the angle included between the former and its radius vector, in all cases in which the position of the storm's centre has been ascertained on sufficient evidence. In one other important condition I have also departed from the method pursued by Prof. Loomis. I have restricted the measurements to wind observations of ships at sea, within the influence of the storm, and to those of good observatories on the coast, subject to the same proviso; and have taken no account of those of inland observatories. This difference of procedure is probably the reason that the amount of the incurvature shown by these measurements is somewhat different from

that obtained by Prof. Loomis, though the general fact of a great incurvature is thoroughly confirmed. My results are as follow :—

(1) The mean of 132 observations between lats. 15° and 22° , within 500 miles of the storm-centre, gives the angle 122° between the wind direction and its radius vector.

(2) The mean of 12 observations between the same latitudes, within 50 miles of the storm-centre, gives the angle 123° .

(3) The mean of 68 observations between N. lats. 8° and 15° , within 500 miles of the storm-centre, gives the angle 129° .

The observations within 50 miles of the storm-centre in the south of the Bay are too few to afford any trustworthy result.

For seamen's guidance, the following practical rules may be formulated :—

(1) In the north of the Bay of Bengal, standing with the back to the wind, the centre of the cyclone bears about five points on the left hand, or three points before the beam.

(2) In the south of the Bay, it bears about four points on the left hand, or four points before the beam.

(3) These rules hold good for all positions *within the influence of the storm*, up to 500 miles from the storm-centre. On the north and west the influence of the storm rarely extends to anything like this distance, but it does to the east and south.

Since much of this evidence, afforded by the Bay of Bengal cyclones, has been before the public for many years, it is incomprehensible to me how a man of M. Faye's scientific eminence can still assert that in the tropics "the wind arrows display an almost rigorous circularity." If, as may possibly be the case, he relies on the evidence of Mr. Piddington's memoirs, ignoring all subsequent work, it is only necessary to examine those memoirs to find that his data do not bear out that author's conclusions. In the charts which accompany Mr. Piddington's later memoirs, the wind observations are, as a rule, not shown, but only the ships' courses, and the author's interpretation of the positions and tracks of the storms. But the evidence is always fully given in the text, and it will be found that when the wind arrows are plotted therefrom, and are sufficiently numerous to allow of the position of the storm's centre being determined, which is far from being generally the case, they are reconcilable only with spiral courses, having a considerable incurvature.

I do not propose now to enter on a formal criticism of Mr. Piddington's work, the great merit of which, as that of a pioneer in the field of storm-science, no one more fully recognizes than myself; but so much seems necessary in explanation of the apparent glaring discrepancy between his results and those of modern workers in the same field.

The evidence of the cyclones of the Bay of Bengal, those tropical cyclones to which M. Faye appeals as authoritative on the validity of his views, is, then, conclusive against him. There is a strong influx of the lower atmospheric strata into a tropical cyclone, proving, in the most unquestionable manner, the existence of an ascending current over the vortex. This fact is quite independent of any views that may be entertained as to any theory of cyclone origin and movement of translation, but any such theory must harmonize with the fact, and hence I conceive that it is fatal to M. Faye's views. With these, in so far as they are theoretical merely, I have no present concern, but it is obviously a matter of high importance to seamen that they should not be misled as to the facts of the wind's movement in cyclones, and it is because the promulgation of such views as M. Faye's tends to perpetuate an old and now exploded error of fact, that I have to put in my protest against them.

HENRY F. BLANFORD.

Folkestone, June 15.

NOTES.

IT should have been stated in our paragraph last week relative to the opening of the Laboratory of the Marine Biological Association at Plymouth that the President, Prof. Huxley, who has given unremitting care to the affairs of the Association during the last three years, would be present if he were not prevented from taking part in any public proceedings by the state of his health. In the absence of the President, one of the Vice-presidents of the Association, Prof. Flower, will preside. The Honorary Secretary, Prof. Ray Lankester, who founded the Association, and has conducted its affairs to the present issue, will also be present.

MR. J. J. H. TEALL, who now holds a foremost place among the petrographers of this country, has just been appointed to the Geological Survey. We understand that he will be specially charged with the study of the crystalline schists and the problems of regional metamorphism, and that he will be closely associated with the field officers who are mapping these rocks in different parts of Scotland. The Survey is to be heartily congratulated on this appointment. The staff is now remarkably strong, but the problems with which it is confronted are among the most difficult in geology. These problems have never been attacked by such a united force of field geologists and microscopists, who, working together with one common aim, will no doubt raise still higher the scientific reputation of the Survey, increase our knowledge of the history of the most ancient rocks, and throw light on some of the most puzzling questions in geological science.

THE electors to the Mastership of Downing College, Cambridge, have, by a unanimous vote, chosen Dr. Alexander Hill, Fellow of the College, to succeed Prof. Birkbeck. Dr. Hill's claim to the appointment sprang from his success as a teacher and worker in biology. No appointment to a Headship has been made on this ground alone since the revival of natural science at the Universities.

ON the 4th inst., Dr. Maxwell T. Masters was elected a corresponding member of the Institute of France, in the Botanical Section, in place of the late Prof. Asa Gray. Besides Dr. Masters, the following names appeared on the list of presentation: M. Treub, of Batavia; Mr. Triana, of Paris; M. Warming, of Lund; M. Wiesener, of Vienna. Dr. Masters obtained 39 votes; M. Triana, 5; M. Treub, 1.

THE Sorbonne, consulted as to the proposed creation of a Chair for the teaching of Darwinian theories, has not expressed disapproval of the scheme suggested by the Municipal Council of Paris. It has appointed a committee to report on the matter; and it is expected that no serious opposition will be offered to the proposal.

WE are glad to learn that a pension of £50 has been granted to Mrs. Balfour Stewart from the Civil List.

ON May 25, a complimentary dinner was given at the Queen's Hotel, Manchester, to Prof. Schorlemmer, of the Owens College, by his former pupils, to celebrate the occasion of the conferring of LL.D. upon him by the Senate of the Glasgow University, and to offer their congratulations. In the absence of Sir Henry Roscoe, who had been expected to take the chair, Mr. R. S. Dale, one of Prof. Schorlemmer's eldest pupils and friends, presided. Numerous congratulatory telegrams and letters were received by Dr. Schorlemmer, and early in the evening a letter was read from Sir Henry Roscoe, expressing regret that he could not be present, and testifying to his high appreciation of the ability of his old friend and colleague. Among those from whom congratulatory telegrams were received were Dr. Pauli, Director of the firm of Meister, Lucius, and Brüning, in Hoechst; Prof. Berntsen, of the Badische Anilin und Sodafabrik, in Ludwigshafen; and Prof. Hermann Kopp, of Heidelberg, the historian of chemistry, who spoke