cal Telescopes with Silvered Glass Specula,' by Mr. J. Browning.

'On the Heat attained by the Moon under Solar Radiation,' by Mr. J. P. Harrison.—When the author brought forward the subject of lunar insulation a year ago, he showed by a single diagram that the surplus or accumulated heat in the moon, beyond what it radiates off into space, or to other matter, owing to the long-continued action of the sun's rays upon her crust, would necessarily reach its maximum several days after the date of complete illumination. The mean duration of solar radiation for the whole periods of the first and third quarters being in fact in the proportion of 4·25 : 11·25; and, consequently, the days on which the moon's surface opposite the earth would be longest withdrawn from, or exposed to, the sun's heat; or, in other words, the days on which the moon completes her first and third quarters would be not far removed from the day and date of her maximum and minimum temperature. He had since learnt that Herr Althaus, some few years back, approximately estimated the temperature of the moon at 840° F. on the 22nd day of the lunation, seven days after the day of full moon. His method was to measure the sun's radiation by the pyrheliometer, and then, applying the results to the moon, to deduce from the extent of her area the amount of heat intercepted; his measure of the moon's capacity for heat was that of Quartz. Assuming this deduction to be correct, the heat attained by the moon would approach very closely the temperature at which iron appears red in twilight, and it exceeds the fusing point of tin and lead. Unfortunately the estimate cannot be compared with that made by Sir John Herschel, which was confined to the moon's heat at the period of complete illumination, and which (without any definite temperature being named) it was stated would be far in excess of boiling water. But as the moon's crust would, at the last quarter, have been exposed to some 180 additional hours of uninterrupted solar radiation, it is probable that the total heat attained must be very great indeed. Whatever this may be, the maximum will, it is believed, occur as stated, at or near last quarter. The date of the greatest cold in the moon appears to be less certain; for though the German physicist already cited arrives at the conclusion that it would be found about half a day after new moon, the problem is more complex, and the author could not but think that it must occur later in the lunation; at the period, in fact, when, as was said before, the region of the moon opposite the earth has been the longest time unexposed to the sun's rays. (He was throughout speaking of the moon's hemisphere turned towards us as "the moon.") If a temperature of —92° Fahr. occurs at the time fixed by Althaus, it would suppose a fall of 940° Fahr. (or 522° Cent.) in about eight days. It is true that bodies at very high temperatures cool, both in air and in vacuo, with great rapidity; yet it has been proved that the rate of cooling is greatest in air, by reason of its conduction and convection of heat. This is one of the laws laid down by Dulong and Petit, and admitted by those whose judgment in the matter is most to be relied on. Still the author had thought it desirable to submit the point to experiment in the large air-pump at Kew, where the velocity of cooling, shown by a thermometer with a half-inch bulb coated with lamp black, for temperatures a little above the boiling-point, was found, for the first 100°, 25 per cent. quicker in the glass filled with air, than in the exhausted receiver. Thus it would seem that the absence of an atmosphere might in the case of the moon favour an accumulation of heat, though in a different manner from that in which the presence of air and vapour affects the earth, where the slight heat stored up in her crust would be speedily lost if it were not for the counter-radiation to her surface from cloud and vapour. As regards the theory that the solar rays would have no power to heat matter if surrounded by dry air or ether, there would seem no reason to believe that this is the case. It would be necessary that the observations which are supposed to point to that conclusion should be verified by trustworthy and independent testimony before the possibility of a result so unlooked for is admitted. Sir H. Davy, he was informed by an eminent physicist, satisfied himself by experiment that absorption of heat from the cool points of the electric light took place in vacuo. Indeed, his own experiment with the solar rays upon the blackened bulb of a mercurial thermometer in the 16-inch receiver already referred to, though undecisive as regards the relative speed of heating in air and vacuo (for which the use of the sun as the source of heat presents a difficulty in the case of experiments which succeed each other with the same apparatus at an hour's interval), yet showed a gain of 160° Fahr. in two minutes (or 71° 11′ Cent.) in a vacuum of about one-eighth of an inch. Also in several experiments with thermometers with both black and blackened glass bulbs enclosed in exhausted two-inch globes, lent to me by Mr. Casella, one with a lamp-blackened bulb in a globe filled with air made for the purpose, the thermometers in the exhausted globes, and more especially the one with the blackened bulb, were found to rise quicker and read higher in equal intervals of time than the one in the globe filled with air. On a view of the whole case at the present time, there would seem to be reason to believe that the sun's rays must penetrate the moon's crust to a depth that would prevent the possibility of her acquired heat being easily or speedily dissipated.

'On an Error in the usual Method of Obtaining Meteorological Statistics of the Ocean,' by Mr. F. Galton.—The meteorological statistics of the ocean have been hitherto obtained by extracting observations from the logs of different ships, and by sorting those that were made in different geographical divisions of the ocean into corresponding groups. The usual geographical divisions are bounded by each fifth degree of latitude and longitude. They therefore are 300 miles in length, and have an average breadth of 150 miles. Each of the groups is treated as if it were composed of observations taken at irregular periods, by a single person, stationed at a fixed observatory in the centre of the group, that is to say, the mean barometer. Thermometer and other elements are determined by computing the simple mean of all the recorded observations. The proportion of winds that blow to the different points of the compass is computed in a similar manner. Only one limitation is exacted in respect to the admission of an observation into a group. It is, that it should not have been made at an interval of less than eight hours from any other observation made by the same ship already included in the group. Were it not for this limitation, a zealous observer might contribute hourly or yet more frequent observations, which, by their multitude, would prevent the scantier observations of other ships from having a just influence on the general average. In an extreme case of this description, the weather met with by a single ship, on one particular voyage, might mainly govern the computed results. In a recent report on the condition of the Meteorological Department of the Board of Trade, by Mr. Farrer, Capt. Evans and the author, they had pointed out many objections to the existing methods of computing ocean statistics. The object of the present paper was to draw attention to yet another objection, and to show that an additional limitation is required before an observation ought to be admitted into a group. The objection the author made was, that the observations by a sailing ship are more numerous in respect to antagonistic winds or calms than in respect to favourable weather. Therefore, as some parts of the ocean are mainly frequented by outward-bound, and others by homeward-bound ships, the means of the recorded observations in those squares must differ materially from the true average weather. When favourable winds are blowing, a ship is rapidly wafted across the area of observation, and comparatively few observations are made within it. The wind may continue blowing, but the ship is unable to record its continuance after it has left the area in question. On the other hand, if an antagonistic wind blows, or if calms or light breezes prevail, then the ship is delayed within the area and continues making observations during the whole or nearly the whole period of their continuance. The author's objection would be of little consequence if the areas into which the ocean is divided for the purposes of meteorology were so large that no ship could cross them without experiencing frequent changes of wind. But this is by no means the case in the five-degree squares. Even if a ship's course lay along the diagonal of an average "square," the length of passage within it would be only 335 miles, and would be traversed in less than five eight-hourly periods with a favourable wind. Taking one course with another across the square, some cutting through a mere corner of it, some crossing it lengthways and some breadthways, an average of three eight-hourly periods, or one day, would be an ample estimate. Now in the ocean regions of variable winds, the changes of the wind are, on the average, much less frequent than once in a day. We might fairly estimate them as lasting in the same quadrant, for an average of not less than three days, or nine eight-hourly periods, at a time. The length of time during which ships are windbound in the English Channel, where the changes are unusually rapid, confirms this rough estimate. On this hypothesis, a favourable wind would, on an average, be recorded three times by a ship sailing across a five-degree square, and an unfavourable wind or a calm of the same real duration would be recorded nine times; therefore the observations contributed by a ship resemble observations made at a fixed observatory under instructions that only three eight-hourly observations were to be taken during the continuance of winds, say, from the northerly quadrant, but that when the wind was in the southerly quadrant the observations were to be continued during the whole of its duration. No one would be inclined to accept the means of these observations as a just statement of the weather, yet this is precisely what is given by the method of compilation adopted by the Meteorological Department. The weather under which a ship enters a square may be of any description whatever, except that of an absolute calm in a sea without a current; therefore it has no bearing on the present question. It must further be observed, that the error pointed out not only affects the winds, but it affects all the meteorological elements so far as they are correlated with the winds; the temperature and dampness are especially affected by it. The method the author proposed, by which this error may be obviated in future work, is to impose a limitation to the observations received, in respect to interval in distance, in addition to the existing eight-hourly interval in respect to time. He proposed that observations should not be included in the groups, unless the places where they were made were at least as far asunder, *measured in the direction of the ship's general course* (and not counting tacks), as she could traverse with a favourable wind in eight hours. Thus on an average not more than three observations would be accepted from a single log-book in any five-degree ocean square. He did not possess data to show how far the accuracy of the existing wind-charts is impaired by the neglect of this cause of error; but he presumed it was only in certain parts of the ocean that it would exercise considerable influence. It is sufficient that he should point it out as one to be guarded against for the future; for he trusted that the whole of the work in the Meteorological Office would be submitted to re-computation, and an improved method of handling and grouping the observations would be adopted, in accordance with the recommendations of that Report to which he had already alluded.

Section B.—CHEMICAL SCIENCE.

President—Dr. H. Bence Jones.
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THURSDAY.

This Section was held at the School of Art,—H. Bence Jones, Esq., M.D., presiding.—In the course of his opening address, the President remarked that, from the foundation of the British Association, in 1831, no practising physician had been President of the Chemical Section. For centuries the union of chemistry and medicine