

been watched here without indication of an associated star-shower. Still, the circumstance that certain meteoric displays have occurred contemporaneously with other phenomena, is interesting (though not, perhaps, significant, as intimating any physical connection), and such records should always be preserved, as possibly having a value which further observations may elucidate.

During the great meteoric shower of November 13, 1866, several observers detected a faint diffused light of an auroral character in the northern sky.

In 1880 there were bright displays of auroræ on August 11, 12, and 13. Whilst watching the Per-seid meteor-shower on those nights, I noticed the successive appearances of streamers and light condensations in the northern quadrant. On the 13th the sky was much lighter than usual, though no streamers or bright glows were visible. The moon set before 10h., yet at midnight the air was not dark; objects around were discernible with remarkable facility beneath the luminous gauze of the aurora apparently diffused over the sky.

During the present month I have been engaged in a series of observations of the Geminid meteor-shower. On December 7, 8, 9, and 10, I especially noticed singular light-radiations, like broad films of faint white cloud, in the northern region. These were very striking on December 9, and persistent during several hours. The most conspicuous of these radiations stretched out of the horizon in the north-north-east, and ran obliquely a considerable distance east, where it enveloped the stars of Virgo and Leo. The effect was somewhat similar to that of the zodiacal light, but in the present case the phenomenon had an evident tendency to remain in the vicinity of the horizon. On its upper side I distinguished very faint indications of streamers, with alternating spaces rendered very dark by contrast. The appearances were, however, so constant, that they can hardly be associated with the characteristic mobile forms of ordinary auroræ. The sky generally was very light, and I have specially noticed this fact on several other occasions this month, when an observer might readily have assumed that the moon, in one of her quarters, was present in the firmament.

A suffused glowing of the whole sky such as that now recorded has not infrequently been visible here in past years during the progress of meteoric observations. Though no definite auroræ (in its normal features) can be described, there is obviously some modification often present imparting to the firmament those peculiarities of aspect and tint which are far too striking to escape notice. I believe that scarcely a very clear night passes but there may be traced, with a critical eye, some feeble traces of auroræ, or their closely-allied phenomena. Could these ever-varying sky-tints be studied in a fine climate, I feel assured we might look for some interesting results.

Bristol, December 12

W. F. DENNING

The Supposed Fall of an Aërolite in Naples

THE late beautiful meteoric display, which was well seen here, has given rise to a somewhat ludicrous incident. The local papers on Sunday evening and Monday contained an account of an aërolite that had fallen in the Strada Fiorentino, one of the principal thoroughfares of the town; that this stone weighed between 6 and 7 kilogrammes, and had nearly struck some people who were passing. This announcement was followed by a description of the stone from the pen of one of the professors in the University, together with an account of meteorites in general.

Such a display of erudition, coinciding with the bombardment our earth has had from Biela's comet, prevented my sleeping all night, and, as early as etiquette would allow, I paid a visit to the house of the two professors, the *happy* (?) possessors of this would-be aërolite. The first examination convinced me that we had to deal with a shoemaker's lapstone of Vesuvian lava, the patina being nothing more than the polish of grease, dirt, with wear and tear. A small fragment was given me, which, after being sectionised, showed a typical leucitophyre of Vesuvius—probably the lava of 1631 from "La Scala" quarries.

I should not have written to you had it not been that such confirmation had been given by men of position, whom I have now obliged to admit their mistake. Probably, however, the report of the fall of this supposed aërolite has already spread, so that I fear it may be included in lists of historic meteorites.

I may say that the stone had probably fallen or been thrown from one of the roofs of the neighbouring high houses.

December 9

H. J. JOHNSTON-LAVIS

The Rotation-Period of Mars

IN the number of NATURE of November 26 (p. 81), Mr. Proctor mentions one or two points in my investigation of the rotation-period of Mars, requiring correction.

The first is, that I did not use Proctor's final result for the period, but one which he published in 1869, differing 0.02s. from the former. As I intended to determine from the whole series of Mars's drawings the correction of the rotation-period, it was perfectly indifferent what value I adopted in my calculations; the only condition was, the error should not be so great as to cause an erroneous interpretation of the Mars pictures of former years. A difference of 0.02s. in the adopted rotation-period changes the position of the markings in the drawings of Huygens and Hook but 2°.9, so that for my purpose I could adopt Proctor's value of 1869 as well as that of 1873. I chose the former, as it seemed, after a preliminary reduction of Schroeter's observations, to be nearer the truth; but my results would have been absolutely the same had I chosen the second.

The second remark of Mr. Proctor's is the following:—"Prof. Backhuysen, like Mr. Denning some time since, has taken Kaiser's result uncorrected for the clerical errors—very seriously affecting it—which I detected in 1873." As I have used only Kaiser's original observations, and no result whatever, corrected or uncorrected, I must conclude that Mr. Proctor has not read my paper very accurately; when he does so, he will see that he is wrong. At the same time he can see that on p. 58 the time of Hook's observations is given, "March 12, 12h. 20m. and 12h. 30m.," and from the indication on p. 55, that there is an integral number of revolution-periods of 24h. 37m. 22.74s. between 1862, November 1, 9h. 55m. mean time Berlin, and 1672, January 1, 22h. 11.0m. mean time Paris, he may conclude, after a slight calculation, that I did not count the years 1700 and 1800 as leap-years. My results are, therefore, free from the errors Mr. Proctor indicated; I hope I made no other.

It seems, however, very difficult to avoid them wholly. Mr. Proctor, for instance, who occupied himself so much with the subject, writes, in the above-mentioned number of NATURE of November 26: "Kaiser counts three days too many in comparing Hook's observation with his own: one day, through a mistake in correcting for change of style and two days (apparently) from counting the years 1700 and 1800 as leap-years." That number of *three* days must be *one* day, for Kaiser indicated as the time of Hook's observations, March 13, 12h. 20m. new style, instead of March 12; by this error the number of days from 1862, November 1, till Hook was one day too small; but, by counting 1700 and 1800 as leap-years, Kaiser added two days too many, so that the total error was one day, and not three. Mr. Proctor's conclusions, based on the latter assumption, are naturally erroneous.

H. G. VAN DE SANDE BAKHUYZEN

Leyden Observatory, December 9

Ventilation

IN NATURE for December 10 (p. 132) I note the suggestion:—"In all new buildings where efficient ventilation is desired, it would be preferable to construct a shaft at one side of, or surrounding the chimney-flue, with an inlet near the ceiling of the room, and the outlet at the level of the chimney-top, so that the air escaping from the room would have its temperature kept up by contact with the chimney, thus aiding the up-draught, whilst the risk of reflux of smoke would be avoided." In building my own house some eight years ago this system was adopted in every room, the outlet over the chandelier being carried across to the side of the chimney of the same room, the two flues being carried up side by side to the chimney stack, each outlet having its own cowl. In practice this has proved a total failure, from the simple fact that the fire-flue is both longer, owing to its starting at a lower level, and that it is also hotter than the other. In the absence of any fire there is a strong upward current in both, but the instant the fire is lighted the upward current in the ceiling ventilator stops, and in a few minutes is reversed, the cold air and collected smoke from the chimney outlet coming in with such force that we have been compelled to make up every ceiling ventilator in the house except one, which, although useless when a fire is lighted, is not a nuisance. Many other experiments in automatic ventilation were tried, so that in case one system failed others might be available, and I regret to say that the only useful remnant of the experiment is the ventilation from the entrance hall, which