

cate a transit, March 22, 1877, which probably did not occur. And yet, viewed in connection with a reported transit, October 24, 1876, seen by Mr. W. G. Wright at San Bernardino, California, and illustrated and fully reported in the *Scientific American* of November 18, 1876, which circumstance Leverrier probably had no knowledge of, was by no means as unsatisfactory as the public imagined; for practically *Wright's transit and Leverrier's hypothetical period mutually confirmed each other*. The *Scientific American* Supplement of August 27, 1881, published some remarks I sent them, which may have reached England. One point I directed attention to was that Leverrier indicated that a conjunction was due September 21, 1876, and I found that there were thirty-three days between that and October 24, 1876, so if Leverrier took 176 synodical periods from Lescarbault to September 21, it was nearly the same thing to take 177 to October 24; but extending the interval to January 1, 1750, there would be much nearer similarity in the synodical periods to accord with Wright's transit, October 24, 1876. I also noticed that the ratio of displacement of the node from Lescarbault and Lummis was 7 days retrograde in 3 years' advance, and on that data, applied to Wright's transit, another transit would be due $11\frac{2}{3}$ days earlier in 1881, while Leverrier, in October, 1876, remarked that *for a transit at this node we must wait till about 1881*. My computation made it fall due, therefore, October 12 or 13, 1881, and I was anxious that it might be looked for. The computation made the Hawaiian Islands the most favourable place; but although I believe it was not seen there, nor was it observed from Sacramento or Salt Lake City, where Mr. W. R. Frink looked for it with a 4-inch aperture achromatic telescope, we have no evidence to show whether it might not have occurred in Europe or elsewhere, and been noticed if it had been looked for.

Sacramento, California

A. F. GODDARD

[The subject of this communication is a very interesting one, as relating to the possibility of changes on the sun's surface being due in some way to the positions of the various planets of the system. But before this relation can be considered as established, it will be necessary to increase the accuracy of our solar information by collecting our past observations, as well as by securing a set of daily observations for the future.—ED.]

An Extraordinary Meteor

I BEG to send you the following, in case you consider it worth inserting:—At about 1.10 a.m. on the night between November 18 and 19, whilst going in the s.s. *Bokhara* in the Red Sea, about midway from Aden to Suez, the quarter-master on duty called me, saying he had just seen a new comet, or shooting star, which was still visible many minutes after its first appearance. He said that whilst he was looking out ahead, or in a northerly direction, he suddenly noticed the effect of a bright light shining from astern, and on turning round saw a very bright shooting star still moving from left to right, and slightly downwards, in the south, at an altitude of about 40°. The star speedily disappeared, but left a bright train of light behind it, which continued so long (from five to ten minutes he guessed) that he thought I might like to see it. I came on deck a little before a quarter past one by the ship's clock, and found a streak of light which I estimated as 8° or 10° in length, and rather less than half a degree in width, apparently stationary, midway between Sirius and Canopus, and nearly as bright as the comet, the head of which must have risen half an hour or more previously. I watched the streak till half-past one o'clock, when it seemed sensibly fainter, though still a conspicuous object, notwithstanding the presence of the moon, the comet, and a number of bright stars. Whilst watching I noticed two small meteors shooting from left to right across the southern sky, which struck me as probably belonging to the same group as the large one whose train I was watching.

At half-past one o'clock I went below, and did not return on deck till 5 o'clock, when the apparition had disappeared. The quarter-master told me afterwards that it had faded away soon after I left the deck, but he believed that from first to last it had remained conspicuously bright for more than half an hour.

Clewer, December 6

B. R. BRANFILL

British Rainfall

I AM just preparing to issue to all the observers of rainfall known to me, blank forms for the entry of their records for the

year shortly about to close. This staff now exceeds 2000, but still as they are not unfrequently rather clustered there are many parts of the country where additional records are needed. I have no doubt that records are already kept in many places unknown to me, and I shall be glad if you will allow me to invite communications from any one who has kept an accurate record, and to supply either those already observing or contemplating doing so with a copy of the rules adopted by British observers, and with all necessary blank forms—all, I may perhaps as well add, free of charge, as our greatest requirements are *ample and accurate records*.

G. J. SYMONS

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Swan Lamp Spectrum and the Aurora

IN NATURE, vol. xxv. p. 347, is a description of the spectrum of carbon as found by Professors Liveing and Dewar in a Swan lamp rendered incandescent in the ordinary way. Finding one of these lamps only feebly lighted by ten pint Grove cells, it occurred to me to test it by the secondary current. The coil was nominally a 6-inch spark one, but little battery power was used, and the spark considerably reduced. One wire was connected with the filament holders, the one made into a little coil and laid on the top of the lamp.

The first effect was a fine silver glow filling the lamp, and showing Plücker tube changes when the circuit was reversed. This gave a carbon spectrum of bright lines. Soon, however, the colour of the discharge changed to pink, and the carbon spectrum gave way to a nitrogen banded one. A yellow spark had been noticed where the wire lay on the top of the lamp, and it was evident air had found its way into it.

At one point perforation had taken place by a single spark, while near this the glass was pounded into a sponge-like mass by a series of these.

The sodium lines due to disintegration of the glass were observed in the spark and glow. I was much struck by the rapidity with which, as what was probably only a small quantity of air found its way into the lamp, the nitrogen-spectrum swept away and took the place of the carbon one, a matter which seems to present another difficulty to the favourite theory which makes the aurora, with its bright, sharp unrecognized lines, an electric discharge in rarified air.

J. RAND CAPRON

Guildown, November 30

The Aurora

ALREADY we have for the height of the "auroral beam" the varying estimates of 44, 170, 200, and 212 miles, and assuming the correctness of any one of the three last figures, we seem drifting from the improbable to the impossible, for are we not told by Messrs. De la Rue and Müller (NATURE, vol. xxii. p. 24) that while at 81.47 miles' height, the discharge is "pale and faint, at 124.15" no discharge could pass? Lest this addition to the aurora's mysteries be for want of definite particulars in the observations, I add mine as nearly as I can—Time 6h. G.M.T. and a few minutes? Altitude of moon above horizon 28°. Distance from moon's centre to centre of beam as it floated above 2°, direction east to west (nearly). Lat. 51° 13' 46" N., Long. 0° 28' 47" W. (observatory).

Guildown, Guildford, December 11

J. RAND CAPRON

Fertilisation of the Speedwell

IF Mr. Stapley, who wrote on this subject in last week's NATURE, can refer to Dr. H. Müller's treatise on the relations between flowers and insects, in the first volume of Shenk's *Handbuch der Botanik* (now publishing as part of Trewendt's *Encyclopædie der Naturwissenschaften*), he will see that his own observations are very similar to those of Dr. Müller. The latter, however, refers to and figures the Germander, not the Common, Speedwell. Is it possible that Mr. Stapley—who speaks of the *Veronica officinalis* as having larger flowers than the *V. hederifolia*, whereas they have flowers of about the same size—mistakes the *V. chamædrys* for the *V. officinalis*?

The insects which Dr. Müller found bending down the stamens, as Mr. Stapley describes, were small Diptera chiefly of the genera *Ascia* and *Melanostoma*. He mentions this also in *Kosmos*, iii. p. 497, and a few pages earlier (*ib.* p. 493) he gives a large drawing of *V. urticæfolia*.