

on November 27, 1872, offers a strong contrast to the comparatively prolonged duration observed at its recent return.

Bristol, December 6

W. F. DENNING

WHILE watching the meteor shower of the 27th ult. I observed what closely resembled the appearance of an aurora. There was seen extending along the horizon from about south to about west-north-west—perhaps further towards the north, for my view was there obstructed—and upwards for about 20° from the horizon, a faint reddish-pink luminous haze, varying fitfully in colour, becoming sometimes nearly white, and in intensity both as regards time and position. The greatest brightness noticed by me was nearly due south. Stars were clearly visible through it.

On referring to the letters in NATURE upon the shower of November 27, 1872—to refresh my memory upon other points—I found that appearances of an aurora on that evening are recorded by “several correspondents.” [Mr. Denning’s letter in NATURE, December 5, by Father Denza in Piedmont (NATURE, December 19), by Mr. Baber at Liverpool (same number), and the Hon. Mr. Newton and Mr. Bruce at Mauritius (“a pulsating coruscation, similar to the appearance of the aurora australis”), NATURE, January 23, 1873]. NATURE for January 16, 1873, contains a letter recording a “pale auroral light” seen at the same time as a shower on December 7, 1838, and Mr. Denning (April 24, 1873), records that the April shower was accompanied by “bright displays of aurora.”

Mr. Newton and Mr. Bruce add that “the instruments at the Observatory gave no indication of a magnetic disturbance.”

Some of your readers may be able to say whether any magnetic disturbance was observed on the evening of the 27th ult. I saw the auroral appearance about 7.15 p.m.

Rugby, December 7

J. B. HASLAM

P. S.—In a note received to-day in answer to my inquiry, the Superintendent of the Kew Observatory kindly informs me that at Kew the “magnetic curves for horizontal intensity, vertical intensity, and declination were remarkably steady throughout the whole of the 27th and 28th ult., being almost straight lines at the time of the meteoric shower.” He adds that no auroral effects were seen at Kew.—J. B. H. (Dec. 8.)

In case England has been clouded on the 27th, it may be well to state that the meteors were brilliantly seen in the Adriatic. A few were visible on the night of the 26th; on the 27th, at 16h. 30m. G.M.T., they averaged thirty per minute; at 17h. they had much increased, and were counted, at 18h. 10m., at seventy per minute, while at 20h. 40m. they had decreased to thirty per minute again; on the 28th very few were seen. During the rapid shower they were not equally distributed; for six or eight seconds only one or two were to be seen, and then, in a couple of seconds, perhaps eight would be counted, mostly seen simultaneously. The radiant-point was estimated at about 15° S. of the following end of Cassiopeia at 16h. 30m., and at about 3° S. of the preceding end at 20h. 40m. The trails were more persistent and brilliant in the latter part of the evening. One was distinctly seen by two observers to sharply bend its apparent course about 20°, possibly a case of perturbation by a non-luminous meteor, or else of splitting. A large number were as bright as first-magnitude stars, and many equal to Venus.

WM. F. PETRIE

s.s. *Tanjore*, November 28

FROM the accounts in NATURE and in the *Times*, it is evident that the display of meteors was much finer in the east of Switzerland than any of those mentioned by your correspondents. My attention was first directed to the shooting-stars shortly after 6 o'clock (local time here being about thirty-eight minutes in advance of Greenwich time). For half an hour after that time the fall was continuous, several meteors appearing together. In fact, so many were falling, that it seemed to me hopeless to attempt to count them, but I should think that they must have fallen, on a moderate computation at that time, at the rate of at least 200 a minute. Many of them were especially brilliant, and those falling near the mountains, which completely encircle this village, produced, I presume by irradiation, the curious appearance of passing between the spectator and the mountains. The richest period of the display when, looking from a window, four or five were seen together in one part of the heavens did not last for more than an hour, but the phe-

nomenon continued with less effect until 9 o'clock, when the sky which, until that time had been perfectly clear, became overcast. The height of the high-lying plateau of the Canton Grisons, more especially in the Engadine, and the remarkable absence of aqueous vapour, causes many more stars to be visible here than in the denser air of England, and this, no doubt, in large measure, accounts for the superior brilliancy of the display as witnessed here. This strangely affected the imagination of some of the peasants of this village, one young woman in particular spent the evening in tears and lamentations, momentarily expecting the end of all things.

J. F. MAIN

Wiesen, Canton Grisons, Switzerland

“Evolution without Natural Selection”

TWO or three points in Mr. Romanes’s letter in your issue of December 3 (p. 100), leave me no other alternative than to again ask you to insert the following few remarks. I beg to inform Mr. Romanes that with Darwinism my book has very little to do. It neither attempts to refute nor confirm the Darwinian hypothesis of Natural Selection. Neither is it an “emendation of Darwinism”; but the facts it contains seem to be an all-necessary supplement to the great naturalist’s hypothesis. It is to be regretted that at the present time so many naturalists accept the theory of natural selection as an exclusive explanation of the evolution of existing species. They unconsciously blind themselves to the existence of any other agent in the work of evolution. To them there can be, nor is, no other. No greater error could be made; and it is my firm conviction that as time goes on the theory of natural selection will gradually lose much of its present presumed universality. What is becoming more evident every day is that existing species do not owe near so much to natural selection for their evolution as extreme Darwinians would have us believe. What the remote ancestors of these species derived from its influence is another matter. How far its influence has been exerted on living forms is not for me even to conjecture; but certainly, so far as birds are concerned, the evidence of its influence is astoundingly slight in comparison with the number of species.

I am very pleased to see that Mr. Romanes has changed his opinion concerning “trivial specific characters,” and now admits that they are both numerous and important. But they cannot even be regarded as “insignificant” as compared with the great “organising work of natural selection.” For, according to the Darwinian theory, they should owe their very presence to its influence, but, unfortunately for the hypothesis, they do not. Once more I must strongly protest against Mr. Romanes saying that my book attempted to explain the *cause* of variation. It does nothing of the kind. Nor do I consider it fair for Mr. Romanes to infer that isolation, &c., do not explain the cause of variation, and therefore that they fail as evolutionistic agents. It would be just as fair and logical to say that the Darwinian hypothesis is a failure because it does not explain the cause of variation. Darwin must have a variation to begin with for natural selection to work upon; so must isolation. The cause of variation is one of the greatest secrets which Nature still retains in her keeping; but doubtless it will soon be wrested from her.

London, December 6

CHARLES DIXON

I HAVE not changed any of my views; but Mr. Dixon appears to change his within the limits of two consecutive sentences. For, immediately after his strong protest against my statement that he has attempted to explain the causes of variation, he complains of my want of fairness in not acknowledging the adequacy of the “evolutionistic agents” which he has suggested as “the causes of variation.” With this specimen of Mr. Dixon’s method of discussion before them, your readers may be able to sympathise with the failure which seems to have attended my efforts at expounding his essay.

The analogy between isolation and natural selection does not hold. For is it not obvious that while natural selection can be understood to operate in an explicable manner on the variations supplied to it, there is no analogous explanation to be given of the manner in which isolation can so operate—*i.e.* why isolation *per se* should preserve some of the variations and not others? That isolation is a favourable *condition* to the occurrence of trivial or non-adaptive specific change, I have not denied; but, on the contrary, expressly affirmed: I have only denied that it can be regarded as the *cause* of such change—and least of all in any way similar to that in which natural selection may be re-