the water, became a great floating mass of ice in a week or less, a mile in extent and of great thickness, and carried those good explorers Weyprecht and Peyer helplessly about for a long time in 1873-74, in Barentz Sea, then landed them safe on one of the Francis Joseph Islands; thus leading to the discovery of this great Northern land. Snowdrifts would in the course of one winter (I have seen a fifteen feet depth of drift in one night) fill up most of the inequalities of surface, and thus the floeberg is complete.

Lieut. Greely says that these "floebergs are simply detachments from slowly-moving glacial ice-caps, from an ice-covered land in the neighbourhood of the Pole;" that "Dr. Moss (of the *Alert*) was certainly correct as to the universality of stratification in this ancient ice, and he concurred in the Doctor's opinion, that its salinity was due to efflorescence and infiltration."¹

First as to the formation of these floebergs; Lieut. Greely tells us they are detached from an ice-cap 1000 to 4000 feet thick near the Poles. Surely if this were so, some of these great masses, which would show about 140 feet above water, would have been seen by Parry, when in the summer of 1827 he was in lat. 82° 45' N. (only thirty-nine miles south of Greely's extreme) to the north of Spitzbergen; but neither Parry nor any of the brave whaling captains, who have gone to high latitudes between Greenland and Spitzbergen—the great highway of northern ice in its southward drift—have ever seen anything of the kind.

In the Antarctic we all know that such ice-mountains (the source of which Sir James Ross's discoveries tell us of) drive down to lat. 60° S. near to Cape Horn; the natural idea is, that they would do the same thing in the Arctic Sea—in company with the great ice pack, through the wide high road above mentioned—and not confine themselves to the coast seen by Lieut. Greely's party.

A word or two on my own experience much further to the south.

When passing in my boats for 800 or 900 miles along the west shores of Hudson's Bay in 1846 and 1853, I saw several floebergs aground, some thirty or forty feet above the surface, so large and high that any one at a distance of a mile or so, would have mistaken them for true icebergs; they were merely a mass of floes forced together by strong winds. In such low latitudes (58° to 66°) these spurious icebergs all disappeared before autumn.

No true iceberg that breaks away from land-ice is ever found, as far as I know, to contain saline strata, as the late Dr. Moss found to be the case with the floeberg from which the crew of the *Alert* in 1875-76 took the ice, for drinking or making tea. Sometimes this ice was so salt as to be unfit for the purpose, although high above the sea-level. This result is attributed by both Dr. Moss and Lieut. Greely to "infiltration." I cannot understand how saline fluid could "infiltrate" upwards from the sea into ice—a solid—in which there would be no pores through which it could flow, apart from the fact of the greater specific gravity of the brine.

I do, however, know from personal experience that saline fluid does, under certain circumstances, percolate or filtrate downwards, converting sea-ice, previously saline, into a sufficiently fresh state to afford good drinking-water when thawed. This discovery, like a good many others of more importance, was accidental. In passing a piece of old ice—that is, of a former year's formation, which was known to be so by its wasted and rugged outline, as it stood some feet above the surrounding level ice-floe—I knocked a small piece off, and on putting it into my mouth, found it quite fresh. From that time, during sledge journeys of 1200 miles in the spring of 1847, I looked out for some old rough ice, before building our snow-hut for the night's shelter, so as to get water quickly.

Experience had taught me that a kettleful of water could be obtained much more rapidly and at a far less waste of fuel by thawing ice than from snow, because the latter, however closely packed, contained much air, which, at a temperature of zero or lower, required extra fuel to warm it up to 32° Fahrenheit; a kettleful of snow will give little more than a third of a kettleful of water, whilst the same measure of ice will nearly fill the kettle with water.

The fresh ice I speak of could not be part of an iceberg, because there were no bergs in the great bay where we were travelling. Moreover, if a piece of this ice (which was fresh at

a few feet above the sea-line) was chopped off on a level with or below the water-line, it was found to be saline.

How does this take place? Simply, I imagine, by the brine or saline fluid filtrating downwards through pores made by itself in the ice, as soon as the summer temperature became high enough to thaw the saline part, the fresh portion retaining its solidity, with the exception of the minute pores worn out as above described.

My belief is that the floebergs seen, and so named by the English Expedition of 1875-76, were formed of saline sea-ice, piled one floe over another, and that when the summer temperature penetrated them to a certain extent, the salinity filtered downwards as above described, but that certain layers or strata, either from not being subjected to a sufficient rise of temperature or from some other cause, still retained their saltiness.

All sea-ice has a surface-layer, more or less thick, of brine efflorescence, far more saline than the body of the floe. If, then, six or eight floes are forced up, the one over the other, there will be so many layers of these thin very saline strata.

I repeat that infiltration upwards in this case is contrary to all laws of gravitation, unless those learned in chemistry or physics can show that there is some powerful attraction or affinity to drag a saline fluid upwards through a dense solid.

This communication has gone far beyond the limits I intended, and yet is very short of what might be said on other parts of Lieut. Greely's lectures in Scotland. I must conclude by expressing my admiration of the great amount of geographical work done by this expedition, and the miraculous rescue of the few survivors where twenty-four hours' delay would have been fatal, resembling in this respect very closely the rescue of a part of a Government overland expedition in Arctic America sixty-four years ago, who, but for the arrival of friendly Indians with food and most tender nursing of them, could not have lived more than a couple of days longer.

4, Addison Gardens, Kensington, Nov. 28 JOHN RAE

P.S.—The *Scottish Geographical Magazine* has just reached me, by which I find Greely's mean temperature of his winter quarters to be -4° F. instead of +4°, therefore almost exactly the same as the temperature found by the English Expedition of 1875-76, instead of there being 8° difference—as I put it.—J. R.

December 7

The Recent Star-Shower

It being important to ascertain the duration of the recent shower of Andromedes, observations were continued here on the night of November 30. During a watch maintained for about four hours and a half between 5h. 30m. and 10h. 15m., ten Andromedes of most certain character, together with two other meteors, in reference to which some doubt existed as to their absolute identity with this stream, were recorded from a radiant-point carefully determined at 21° + 42½°. Thirty-one non-conformable shooting-stars were also seen from showers in Perseus and the region eastward.

It is therefore clear, from the results obtained on November 30, that the display had not lost its visible character, though it had evidently subsided into a state of great feebleness. It yielded certainly not more than three meteors per hour for one observer, and these were extremely faint.

On the evening of December 1 the sky was again clear. A prolonged watch of the region of Andromeda then revealed no trace of the display. Meteors were very rare, generally, all the evening. On December 4 they were very frequent, but the radiant-point near γ Andromedæ gave no sign. The ζ Taurids and Geminids (which are specially mentioned in the current number of *NATURE* (p. 108) as deserving observation during the present week) were both visible, and a number of contemporary streams had come actively into play. But, during long watches on the nights of December 1 and 4, there was no appearance of outlying Andromedes. The cessation of the shower definitely occurred between November 30, 10h. 15m., and December 1, 5h. 45m., after an observed duration of little more than five days. But this period unquestionably fails to represent the real duration, for, could observations have been made before moonrise on the early evenings of November 24 and 25, there is no doubt it would have been detected. We can hardly admit a sudden rise of the shower from invisibility on the 25th to a degree of richness on the 26th sufficient to give more than 100 meteors per hour. It is to be hoped that reports from other stations will throw some light on the visible development of this remarkable stream. In any case the extremely narrow limits of its display

¹ I met the late Dr. Moss at the British Association when he held some years ago in Dublin. We conversed a good deal on the above subject. I learnt from him, if my memory is correct, that the floeberg from which the crew of the *Alert* took the ice to thaw for their use, was found to have strata too saline to drink. This explanation I think requisite.—J. RAE.

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 south, 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 auroræ.”

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-