

of the sandstones. Both Dr. Dawson and Dr. Linnarsson therefore long ago expressed the opinion that the Bilobites of Sweden and America must have been trails of some animals. In order to explain this mode of occurrence so that it might not appear as proof against the vegetable nature of the bodies, Saporta takes refuge in a somewhat curious manner of fossilisation described and illustrated by woodcuts in the review referred to. As I feel sure that every one who has made himself acquainted with true modes of fossilisation will immediately be aware that the process adopted by Saporta is indeed most improbable, it will be superfluous to dwell any longer on that question. But even granted that the plants *sometimes* should occur in this way—which statement I, however, think must be due to some confusion as to the real facts—such an occurrence could never be regarded but as a very rare exception to the general rule. And it therefore does not explain why the Bilobites should *only* occur in this, for true plants, exceptional way, (on the under surfaces of the slabs), *never* as true fossils embedded in the rock. This mode of occurrence harmonises, on the other hand, perfectly with the view that the Bilobites are trails of some animals, while it *cannot* be explained on the supposition that they are true organic bodies.

One arrives precisely at the same conclusions on studying their external structure, which possesses pretty great analogies as well with the trails of *Limulus*, long ago described by Dawson, as with those of other Crustaceans, described by myself. It is true that Saporta lays great stress on some superficial markings which are to be seen on some of the French specimens; but those who have studied not only the French Bilobites, but also those from Sweden or America, will soon be aware that the markings referred to are quite accidental. It is indeed surprising that Saporta, while adopting my views concerning Cross-chorda, does not see that the Bilobites are somewhat analogous forms, though much larger. There is consequently no reason why they should be regarded as other than the trails of Crustaceans.

As for Eophyton, it is a pity that this should still be mentioned as possibly of organic origin. It occurs precisely as true trails on the under surface of the slabs; it is found in every system from the Cambrian to the present time, where it can still be studied on the seashores; all the different forms, under which it presents itself are also still to be seen there. Although it thus has been *proved* that it cannot be any organism, Saporta still adheres to the opposite opinion. Now, if he had read through my work, he would have learnt that I by experiment have demonstrated that Eophyton can not only be produced by drifting plants, but also by the tentacles of *Medusæ* or other soft bodies. Now there are casts of *Medusæ* associated with Cambrian Eophytons of Sweden, and their habits were probably—as I have elsewhere¹ tried to show—similar to those of the existing *Polyclonia frondosa*, which creeps on the mud by means of its tentacles, and it is therefore likely that the Cambrian Eophytons are of this origin.

It is further stated that “the Chondrites of the Flysch, strongly impregnated as they are with carbonaceous matter, are admitted on all hands to be Algæ, and the author asks how the same origin can be denied to casts of specifically identical Chondrites of the Cretaceous and so on to the Liassic forms.” This argument is, however, a real “*petitio principii*,” for it is so far from the actual state of things that the Chondrites of the Flysch are on all hands admitted to be Algæ, that many authors, and among them Dr. Th. Fuchs, of Vienna, whose excellent and exhaustive studies of the Flysch are everywhere known, hold a quite opposite opinion. And as for the supposed carbonaceous matter, it is not much better with this, as will be shown from a communication from Dr. Fuchs published in my work referred to: “The supposed carbonaceous nature of the Chondrites of the Flysch is in my opinion a *perfect mistake*. They are certainly very often quite black, but even in such cases they consist only of dark marl, *not* of coal.”

Much more might be said on the fossil Algæ, but as I am about to combat the views held by Saporta more fully in a special work, I will here only add that I have found no statement whatever in his work referred to which would tend to alter my opinion, that almost all the “*Algæ incertæ sedis*” in Schimper-Zittel’s “*Handbuch der Palæontologie*” are not vegetable fossils.

A. G. NATHORST

Stockholm, April 9

¹ A. G. Nathorst, “Om aftryck af Medusar i Sveriges Kambriska layer.” (*Svenska Vetenskaps Akademiens Handlingar*, Bd. xix. No. 1, Stockholm: Norstedt och Söner, 1881.)

DR. NATHORST has certainly shown that many of the markings referred to Algæ by some authors might be tracks left by moving animals on a soft mud, but is there reason to suppose that there are conditions under which submarine surfaces of very soft mud with minute tracks have, or could ever have been preserved. On the other hand there is no question about seaweed having existed in Palæozoic and Mesozoic times, and either some of the markings in question are their prints, or no traces of them are preserved. Now it is an uncontroverted fact that even the most indestructible of all vegetable tissue, that of the Coniferæ, has been met with in the same condition of fossilisation, *i.e.* a projecting cast in sandstone on the under side of a slab, and without any internal trace of tissue or even of colouring due to carbon or iron, and Saporta has offered a satisfactory explanation of the origin of such casts. From the relative rarity with which terrestrial plants have been thus preserved, Nathorst almost derides Saporta’s application of this explanation to fossil Algæ, yet it is by no means improbable that this may be their normal mode of preservation. The decay of dead olive-green seaweeds in water must be very rapid. The decomposition of some among them sets in almost immediately under water, and a colourless mucilaginous fluid is given off copiously. I have not watched the whole process of decay, but my impression is that the entire substance in some species would eventually pass away in a structureless glairy mass, and therefore that nothing but a hollow impression could ever be preserved. Casts of these would be more likely to be preserved in sand or mud than mere tracks, because the substance of the weed would occupy them, and prevent them from being immediately filled with the same quality of matrix as the surrounding rock, and until what would afterwards be a line of cleavage had been produced. So far therefore from its being exceptional for fossil seaweeds to appear as casts projecting from the under surface of the overlying mud, this is likely to be the normal condition in which fossil algæ are preserved. This is apart altogether from the question whether any of the Palæozoic markings are Algæ, for, these differ so considerably from any existing forms, that in the absence of internal structure it is quite unlikely that there will be any general agreement respecting them. The observations do not apply to the Rhodospiræ, which scarcely enter into the question. Some simple experiments on the decay of seaweeds in fine sand under water, which any one at the seaside could make, would help to throw light on the subject. J. S. GARDNER

The Weather and Sunspots

IN NATURE (vol. xxvii. p. 551) Mr. Williams ascribes the great cold of March, 1883, at the Riviera, to the absence of sunspots. There is the less reason for ascribing this cold to sunspots, as till now much more evidence goes the other way. And may it not be contended that this evidence is in favour of warm weather, with minimum sunspots in the tropics or in summer alone. The months of November to March, 1877–78, especially February and March, were so warm over an extensive area, especially in the interior of North America and Western Siberia, that the mean temperatures were nearly without precedent, while in no extensive country of the world the temperature was much below the average.

I give some data for March, 1874 (a season with a considerable number of sunspots), at Suchum-Kale, on the east coast of the Black Sea, a place in the same latitude as Cannes, and similarly situated in respect to sea and mountains; it is sheltered from cold winds, and much warmer than the surrounding country.

The observations in Russia being made at 7 a.m. and 1 and 9 p.m., and no minimum-thermometer used, the minima cannot be strictly compared.

The mean temperatures for a long average at Nizza (which are about the same as at Cannes) are January 47°·1, March 51°·8; at Suchum-Kale, January 43°·0, March 47°·8, being at both about 4° colder. Taking the mean of minimum and maximum as the daily mean at Cannes, and that of 7 a.m. and 1 and 9 p.m. as the daily mean at Suchum, we have: Coldest days of March, 1883, at Cannes, 10th, mean 35°·5, or 16°·3 below average; 11th, 34°·5, or 17°·3 below average; lowest minimum on the 11th, 24°·1, or 27°·7 below monthly mean temperature. Coldest days of March, 1874, at Suchum, 3rd, 19°·9, or 27°·9 below average; 4th, 20°·5, 27°·3 below average; 5th, 20°·8, 27°·0 below average. The lowest temperature at 7 a.m. was, on the 6th, 16°·4, or 31°·4 below average monthly temperature. Thus it is seen that at

Suchum, in the same latitude and in a very similar situation as Cannes, in March, 1874, a year with a considerable number of sunspots, there were three days which were more than 27° colder than the average, while in March, 1883, with little or no sunspots, the coldest days mentioned by Mr. Williams at Cannes was only 17°·3 colder than the average.

I want only to show by this example that if it is wished to prove anything as to the varying intensity of the sun's rays, a large number of observations in distant countries should be given, especially in middle latitudes, the work of Dové having well proved that there is always a compensation to a certain extent between cold and warm areas, and a very great number of these deviations being certainly due to causes which have nothing to do with anything beyond the earth's atmosphere.

St. Petersburg, April 17

A. WOEIKOF

Sheet Lightning

LOOKING to the south and south-east from the Bel Alp, the play of silent lightning among the clouds and mountains is sometimes very wonderful. It may be seen palpitating for hours, with a barely appreciable interval between the thrills. Most of those who see it regard it as lightning without thunder—*Blitz ohne Donner*, *Wetterleuchten*, I have heard it named by German visitors.

The Monte Generoso, overlooking the Lake of Lugano, is about fifty miles from the Bel Alp as the crow flies. The two points are connected by telegraph; and frequently when the *Wetterleuchten*, as seen from the Bel Alp, was in full play I have telegraphed to the proprietor of the Monte Generoso Hotel, and learnt in every instance that our silent lightning coexisted in time with a thunderstorm more or less "terrific" in Upper Italy.

May 12

JOHN TYNDALL

I AM glad to find that M. Antoine d'Abbadie's remarks confirm in the main those which I have made on the above subject in NATURE (vol. xxviii. p. 4), especially as to the occurrence of lightning at a great altitude as observed in low latitudes.

In stating that he has frequently observed "thunder without lightning, and lightning without thunder," does M. d'Abbadie mean that, like every one else, he has observed thunder without observing lightning, and lightning without observing thunder? Or have we here a living advocate not only of the dumb lightning, but of the dark (lightningless) thunder?

The thin and local fogs which are not uncommon in thundery weather readily transmit the illumination of a distant flash of lightning. It seems highly probable that in such cases the lightning may be occasionally supposed to be an electric discharge occurring in the fog itself, just as a flickering aurora seen above thin clouds has often been supposed to have its habitat in the clouds themselves.

The suggestion of M. d'Abbadie is a fair one, and I for my part shall be glad to undertake observations of "sheet lightning" this summer in conjunction with any one resident about forty miles from this place, the observers interchanging reports by the earliest post after the hour of observation.

W. CLEMENT LEY

Ashby Parva, Lutterworth, Leicestershire

Hydrogen Whistles

IN his interesting communication on the above topic (NATURE, vol. xxvii. p. 491) Dr. Francis Galton has inadvertently fallen into a mistake which quite seriously affects the numerical deductions which follow. He erroneously assumes that "the number of vibrations per second caused by whistles is inversely proportional to the specific gravity of the gas that is blown through them."

It is well known that the number of vibrations is inversely proportional to the *square root* of the density or specific gravity of the gas. Hence for hydrogen, as compared with air, the number of vibrations per second produced by a given whistle would be increased only about 3·6-fold instead of 13-fold, as he estimates it. Similarly the number of vibrations by the use of hydrogen in the little whistle when set at 0·14 inches would be only about 86,533, instead of 312,000.

Berkeley, Cal., April 12

JOHN LE CONTE

[THE objection of your correspondent is valid. I am informed independently and by high authority that the velocity of sound in hydrogen must be considered as barely fourfold greater than in air, the number of vibrations per second emitted by a hydrogen whistle being increased in the same proportion.]

In making my earlier estimate I had been misled by an erroneous statement in a work that is still of much general credit and authority, to which I referred for ascertaining the velocity of sound in different gases, as it happened to be the book then nearest at hand, and as I have no special knowledge of the subject. It was the first edition of the *Penny Cyclopædia*, where in the article "Acoustics," p. 95, I lit upon the following passage, which professed to give the precise information I wanted:—"Thus air being about thirteen times as heavy as hydrogen, the velocity of propagation in the latter is about thirteen times that in the former." I need not take up your space by quoting the paragraphs before and after this, which emphasise and corroborate the statement and make it clear that it was no slip of the pen. Possessors of this Cyclopædia (I know nothing of subsequent editions) would do well to look out the passage and put a note of warning by the side of it.

The fourfold gain, or nearly so, of the hydrogen whistle is not to be despised. It is sufficient to establish its rank as the emitter of the largest number of aerial vibrations per second of any instrument yet contrived. My little whistle, of about 1 mm. bore, requires a very small supply of air, a bag that I fill with a single expiration containing enough to keep it in continuous sound for many minutes. As yet I have not got a portable holder for pure, dry hydrogen, but a well-known chemist is kindly making an experiment of one for me.

FRANCIS GALTON]

The Pillar of Light

I HAVE frequently observed this phenomenon. The first time I saw it was on April 8, 1852, when I saw it here at sunset, and on April 11 I saw it at sunrise when I was in the Irish Channel, near to Port Patrick, where I was laying a submarine cable.

In the *Monthly Notices* of the R.A.S. vol. xii. p. 185, there are several notices of its having been seen at that time in various places. I saw it last on April 6 this year, when it had the same appearance as previously, which is well represented by Mr. Symond's drawing on p. 7, except that the lower part is too bright, and it looks more correct when shaded with a pencil. The pillar is always perpendicular to the horizon and to the sun's position. I saw the zodiacal light several times in February, extending as far as the Pleiades, and at an angle of about 45°. I think it is highly probable that the pillar of light is caused by reflection from ice crystals, as we had very cold weather early in April, and have still. These atmospheric phenomena are often best seen reflected from a plate glass window.

Gateshead, May 9

R. S. NEWALL

Remarkable Lunar Phenomenon observed at Weston-super-Mare, August 21, 1861

AT about 8.30 p.m. a band of silvery light appeared proceeding from the lower margin of the moon, in a line perpendicular to the horizon. The width of this band was equal to the exact apparent diameter of the moon's disk. Slowly the band lengthened, until its upper portion reached beyond the moon to the extent of about two diameters, while the lower limb extended itself to about the length of four diameters, where its foot rested apparently on a light fleecy cloud. In a few minutes a similar band traversed the other at right angles, forming a perfect Latin cross, the brilliant face of the moon occupying the place of intersection. The arms of the cross were respectively about two diameters of the moon's face. The portion of the sky in which this occurred was clear, but clouds were slowly drifting from the west, and in ten minutes began to obscure this beautiful and unusual phenomenon.

The only record of any similar phenomenon which I can meet with is to be found in Lowe's treatise on atmospheric phenomena, wherein two instances are described. The observer of one was Dr. Armstrong, and the appearance was seen by him at South Lambeth on February 25, 1842. The other observer was Mr. Lowe himself, who was at Derby railway station when the phenomenon occurred. In both these instances, however, the crossbeam was absent. Although no hypothesis has been suggested to account for this appearance, it may be interesting to note that in the case recorded by Mr. Lowe, the very