

conflagration has not reached stand in the clearest relief as they are seen for probably the last time; but in a dozen spots, at both sides of the bridges, sheets of flame and awful volumes of smoke rise to the sky and positively obscure the light of the sun. I am making these notes on the Trocadéro. Close and immediately opposite to me is the Invalides, with its gilded dome shining brightly as ever."

Another as follows:—"As I drive along the green margin of the placid Seine to St. Denis, the spectacle which the capital presents is one never to be forgotten. On its white houses the sun still smiles; he will not refuse his beams spite of the deeds which they illumine. But up through the sunbeams struggle and surge ghastly swart waves and folds and pillars of dense smoke; not one or two, but I reckon them on my fingers till I lose the count."

Twenty-four hours later, the change has come. "The rain is now falling heavily, has been falling heavily all day, and may do something for burning Paris. The sound of artillery has died away;" and from another writer:—"A heavy smoke hangs over Paris and rain is constantly falling."

I believe it has often been remarked that rain generally follows a heavy cannonading, but in this case there is an almost unexampled artillery fire and tremendous conflagration at the same time, accompanied by a sudden and violent change in the atmospheric conditions. From where I am writing we noticed a remarkable change on Thursday morning, and about 2 P.M., after intense closeness and oppression, a rain of a tropical character set in for twelve hours or more. On many occasions in Queensland, I noticed that in seasons of drought, after extensive grass fires, causing intense heat, heavy thunderstorms generally followed.

GEORGE PEARCE SEROCOLD

Rodborough Lodge, Stroud, May 27

Alleged Daylight Auroras

SEVERAL letters having appeared in recent numbers of NATURE, giving what the writers consider to have been undoubted instances of aurora visible in the daytime, you will, I hope, allow me to state the reasons why I still adhere to the views expressed in my former communication on this subject.*

And, first of all, I must beg your correspondent Mr. Jeremiah not to think me uncourteous if I dismiss at once, as unworthy of serious criticism, the cases which he has dug out of monkish chronicles. It is likely enough that some of these old records may be imaginative descriptions of nocturnal auroras, and as such they are not without interest, but I cannot admit them as competent witnesses on a point of nicety.

A more modern instance adduced by the same correspondent will be found at p. 7 of NATURE for May 4, under the title "Aurora Borealis, seen in the daytime at Canonmills." In this case it is difficult to know what relation is intended between the title and the account which follows. The account describes the clearing off of the clouds in a mass from the north-west, with the production of an "azure arch," the centre of which "reached an elevation of 20°." If I reply to this that the clearing off of clouds is not an aurora, even though they clear off in a compact body from the north-west, leaving an "azure arch," I may be met by the rejoinder that nobody said it was; and yet I strongly suspect that the writer had some confused idea that he was describing an auroral arch, and I am certain that nine out of ten readers, misled by the heading, would take the same view. Stripped of the cloud-phenomena, all that remains of the Canonmills aurora is the appearance of some "very faint perpendicular streaks of a sort of milky light," which could be traced across the segment of blue sky, but were "extremely slight and evanescent." Considering the probability that the observer regarded the cloud-arch as auroral, which it certainly was not, and considering how his judgment would be likely to be biased by that idea in the interpretation of "extremely slight and evanescent" appearances, I think we may fairly regard this testimony as particularly weak.

In NATURE for Dec. 8, 1870, Mr. Cubitt describes and figures a double auroral arc which he saw in broad daylight on the 25th October. It was "some 25° above the horizon, and almost due east." In my first letter I expressed a doubt of the correctness of this observation on the ground that auroral arcs are not seen in the east. My criticism has since been challenged on two distinct issues. Mr. Jeremiah insists that an auroral arc may

extend towards the east, and that what Mr. Cubitt saw may have been the eastern extremity of a northern arc. A reference to Mr. Cubitt's letter and illustration will show at once that if what he saw was any part of an arc, it was the apex and not an extremity. But another correspondent, Mr. Reeks, in NATURE of Dec. 29, 1870, in criticising my remark, makes a statement which is more difficult to answer. He affirms positively that in Newfoundland he has many times seen the arch nearly due east, that is, as he explains, with "the extremities pointing N.N.W. and S.S.E." I would suggest, however, in reply to this statement, that in an extensive auroral display there may be fictitious arches, produced by the accidental correspondence of streamers on either side of the "cupola." An arch of this kind may easily extend from N.N.W. to S.S.E., spanning the entire heavens. It is essentially different from the true auroral arc, which, until much stronger evidence to the contrary is adduced, I shall still believe to be invariably transverse to the magnetic meridian. Obviously, Mr. Cubitt's arc was not of the kind that Mr. Reeks describes.

I pass on to a record of daylight aurora, which, more than any other that I have seen, demands a careful investigation. I refer to "An Account of an Aurora Borealis seen in full Sunshine, by the Rev. Henry Ussher, D.D.," said to be taken from the Transactions of the Royal Irish Academy for 1788, and quoted by the Rev. T. W. Webb in NATURE for May 11. Dr. Ussher's account, it must be admitted, is most particular and complete. He describes "whitish rays ascending from every part of the horizon, all tending to the pole of the dipping needle, where, at their union, they formed a small thin and white canopy similar to the luminous one exhibited by an aurora at night." Nothing can be more precise. But is it not also a trifle too wonderful? Surely, if any part of an aurora is to be seen by daylight, it must be just one here and there of the most vivid beams. That the whole phenomenon should be visible at noon-day in all its completeness, just as at night, even to the faint extremities of the streamers in the magnetic zenith, is to my mind so entirely inconceivable that not even the authority of a doctor of divinity can command my faith in it. I can much more easily believe that the sky presented a remarkably symmetrical arrangement of radiating cirri, and that the observer, impressed by the recollection of the aurora of the previous evening, persuaded himself that the "rays coruscated from the horizon to their point of union." The confirmation by "three different people" is of little value unless their observations were independent.

To those who have no clear conception of the difference between cirrus and aurora, the foregoing arguments will be meaningless. Some persons write very loosely of "luminous cirri," and I have even seen described the transformation of cirrus cloud into aurora as it grew dark. I believe that there is no connection between the two phenomena beyond an occasional and purely accidental similarity of form, and that when the two co-exist, the cirrus, instead of being the seat of the aurora or deriving luminosity from it, only serves to obscure its brightness, and, if dense enough, may appear in the form of dark bands across the auroral light, the latter being, as I conceive, at a very much greater elevation.

I adverted in my former letter to the argument that may be drawn from the non-visibility in the day-time of other lights comparable with the aurora, and I will only now add the following suggestion. If the auroras that occur in this country are occasionally visible in daylight, it might be supposed that the much grander displays of the Arctic regions would be habitually visible in daylight. But is the fact so?

Clifton, May 23

GEORGE F. BURDER

Aurora Australis

TRAVERSING the Indian Ocean 44° S. 65° E., I observed, September 24th, 1870, 4h. till 13h. Greenwich time, a south polar light of great intensity and splendour. After my arrival at Manado (Celebes) I was just writing a few lines about it for the readers of NATURE, with the purpose of knowing whether at the same time an aurora, or at least disturbance of the magnetic needle, had been observed on the northern hemisphere, when I saw in NATURE (Nos. 49, 50, and 51, 1870), several interesting descriptions of aurora borealis observed September 24 in England, &c. I am not aware whether many observations of southern polar lights have been recorded, but I remember that those which Cook described in the year 1773 were coincident with aurora borealis observed in Friesland, and others observed in 1783

* NATURE, vol. iii. p. 126.

at Rio Janeiro were coincident with polar lights in the northern hemisphere. At all events I believe that the attention of men of science is not sufficiently directed to this coincidence of northern and southern polar lights, at least not as much as it deserves in respect to the theory of polar lights at all; and I should be very glad if, in consequence of this notice, authorities would discuss this highly interesting phenomenon in NATURE.

I shall later, according to my diary, accurately describe the display of this splendid aurora australis, and mention the influence which it perhaps or probably had on the abnormal meteorological phenomena, which I observed during the succeeding days.

ADOLF BERNHARD MEYER

Manado (Celebes), January 9

P. S.—I beg to contribute to the records in NATURE of earthquakes, &c., over the whole globe:—

November 20, 1870, afternoon, an at first vertical, then horizontal, rather heavy shock at Manado.

January 28, 1871, 4h., a slight, very local shock in a part of Manado.

Manado (Celebes), March 5

The Eclipse Photographs

As an ardent and not inexperienced votary of photography, I am fully alive to the value of photographic evidence, and regard with enthusiasm each fresh victory which photography achieves, yet I cannot myself look with any very great degree of satisfaction upon the photographs of the late solar eclipse either as examples of photography or as evidence contributing to our knowledge of solar physics. In saying this I make no reflection whatever upon the ability or efforts of those by whom the pictures were produced. On the contrary, I am aware that when these pictures were taken the first grand requisite of photographic success—a clear view of the object to be represented—was scarcely to be obtained. Briefly; from a technical point of view, the pictures are of but indifferent definition, and the identity of the coronal rifts in the Cadiz and Syracuse photographs not satisfactorily conclusive, in addition to which in the picture by the American observers, the so-called coronal light extends a long way over the lunar disc, which seems to me to preclude the possibility of its being other than a phenomenon of terrestrial meteorology. A few weeks ago, when the sky appeared almost cloudless, I observed a beautiful lunar halo, very much resembling the so-called corona, which I apprehend no one would attribute to anything but atmospheric moisture. Why, then, in the instance of a sky burdened with innumerable clouds, should we attribute the halo of light surrounding the solar disc to other than atmospheric causes, even though there should be something which might be mistaken for a coincidence in two distinct photographs of one or other of the rifts which were characteristic of that halo?

Manchester, May 26

D. WINSTANLEY

Eozöon Canadense

PERMIT me to state that the presumed "important bearing" on the so-called "Eozöon Canadense," of the principal fact noticed in the communication entitled Palæozoic Crinoids, which appears in NATURE of May 25th, is discussed in a paper by Dr. Rowney and myself, contained in the forthcoming number of the Proceedings of the Royal Irish Academy, now on the eve of publication. The paper referred to is a reply to the articles by Drs. Dawson and Sterry Hunt, which appeared in the last (second) number of the Proceedings.

Glenoir, near Galway, May 29

WILLIAM KING

WITHOUT going into the vexed question as to whether Eozöon Canadense is or is not of organic origin, I may be permitted to express some surprise at the new, and, to say the least of it, startling theory broached by Mr. Perry in last week's NATURE, of the vaporous formation of a certain limestone. The only facts brought forward in support of this view are, its occupying pockets, its foliations, and its conformation with irregularities of surface in the pre-existing rock. All these could be as well accounted for on the supposition of deposition from aqueous solution, without doing violence to the fact that carbonate of lime is not volatile at any temperature.

E. T. H.

THE INEQUALITIES OF THE MOON'S MOTION

THE following is an abstract of the method of computing the inequalities in the motion of the moon which are due to the action of the planets, proposed by Prof. Newcomb in the paper presented to the Academy of Sciences of Paris on April 3.

When we consider the movements of the sun, moon, and earth, under the sole influence of their mutual attraction, the position of each of these three bodies in space will be given in terms of eighteen arbitrary constants, and of the time. The problems of the relative movement of the moon around the earth, and of the movement of the centre of gravity of the earth and moon around the sun, have been solved with a degree of approximation sufficient at least for the purposes of astronomy. Thus, we have the co-ordinates of any two bodies relatively to a third, or relatively to the centre of gravity of the system, in terms of twelve elements and of the time. It only remains to add the expressions for the uniform movement of the centre of gravity in a straight line, to have the general expressions for the co-ordinates of each body.

We have then only to consider the action of the planet to vary the eighteen elements according to the method of Lagrange, to have the movements of each of the three bodies under the influence of the attraction of the planet. Unfortunately, the expressions thus obtained are at first extremely complicated. We have to compute a coefficient corresponding to each combination of the elements taken two and two. The entire number of the coefficients is,

therefore, $\frac{17 \times 18}{2} = 153$. And each coefficient con-

tains eighteen products of the partial differential coefficients of the co-ordinates of the three bodies relatively to the elements. These latter differential coefficients are so complex that the formation of any one product would be a considerable labour. The direct formation of the coefficients required is therefore impossible. The paper in question is principally devoted to an explanation of the simplifications which may be introduced into the problem.

It is first shown that all the coefficients formed by combining any one of the six elements which fix the position of the centre of gravity with any of the twelve elements of the relative motion, vanish identically, while the combinations of those six elements with each other give only the principle of the conservation of the centre of gravity. This leaves only sixty-six combinations. It is then shown that, if the elements are divided into two classes, the first class being the mean longitudes, the longitudes of the perigees, and the longitudes of the nodes of the sun and moon, and the second the mean distances, eccentricities, and inclinations, the coefficients vanish whenever the two elements combined belong to the same class. The number of coefficients is thus reduced to thirty-six, and they are simply the differential coefficients of six functions of the elements of the second class. These functions are formed an extremely simple process when we have the rectangular co-ordinates expressed as functions of the elements and the time.

The remainder of the process is simply one of the development of a very complex perturbative function, and is of no especial interest.

THE HELIOTYPE PROCESS

AT one of the recent *soirées* of the Royal Society given by General Sabine at Burlington House, Messrs. Edwards and Kidd exhibited at work the new heliotype process, whereby photographic pictures can be very rapidly copied in by the aid of the printing-press. The process is very inexpensive, and so rapid that if one of the pages of NATURE were sent to the works, it could be