

position of the body of the bird to the plane of the horizon is observable. The miracle is always performed by the use of the appropriate means.

Cannes, February 12

ARGVLL

I AGREE with "J. R." that the term "hovering" is likely to be misunderstood. I used it because it had been used in the earlier correspondence in NATURE to which I referred. If "J. R." (or any other of your correspondents on this subject) has never seen a hawk hanging in motionless poise above a hillside, I would ask leave to refer him to NATURE, vol. viii. pages 86 and 324, for a description of the act.

February 19

HUBERT AIRY

I HAD once a very unusual opportunity of observing accurately the flight of buzzards, from the summit of Acro-Corinthus. As this unique natural fortress rises sheer from the plain, on the side toward Attica, to the height of eighteen or nineteen hundred feet, a group of these birds, hanging at that height above the surface, were thus brought in a line with the eye. I could detect the minutest movement of wings or tail. Again and again there were considerable intervals, of many seconds' duration, during which one bird and another would hang, with pinions horizontally outstretched, absolutely motionless, neither descending nor drifting, but as if his balance in the air were one of delicately adjusted equipoise. And when, by a just perceptible movement of wing, he stirred again, it seemed rather to be to change his position than that he needed any kind or degree of effort to maintain it. The kestrel is an unfortunately chosen bird for Mr. Hubert Airy's observation, because though it hangs for a minute or two over the same spot watching its prey, it is always "by short and rapid motion of its wings"; from which fanning motion it has acquired, I think, its popular name of windhover, and not because, as Mr. Airy supposes, it is upborne by the wind. But were my Corinthian buzzards upborne by the wind? There was none. The day was one of dead calm. No doubt of necessity there was some upward current of air from the sun-warmed surface of the ground by which the birds profited; but if at all sufficient to sustain them, their actual gravity, when in that position and so willing it (by which I mean nothing so absurd as that gravitation can be counteracted by the *vis vite*, but that by inflating its lungs, and perhaps suspending its respiration, the bird may have the power at will of lessening its comparative weight in the air), must be very near to that of the atmosphere around and underneath them. It is evident that Mr. Airy could only claim my observation as being in favour of his theory if there had been a breeze from Attica striking against the face of the citadel. There was none perceptible; and I drew the attention of my companions to the curious problem presented by such an ease of flight.

Bregner, Bournemouth, February 13

HENRY CECIL

P.S.—Will you allow me just to mention that the letter reprinted from NATURE by Dr. George J. Romanes in his "Animal Intelligence," as mine, is by Mr. Merlin, our present Consul in Athens. I sent it, but he wrote it, and the observation is wholly his.

#### The Auroral Meteoric Phenomena of November 17, 1882

MR. BACKHOUSE remarks in his letter (NATURE, vol. xxvii. p. 315): "It would be well to ascertain whether such a motion (in a curve) would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was a straight line."

When a straight line lies within or without the (celestial) sphere, on whose surface we wish to trace the perspective projection of that line (the eye being placed in the centre of the sphere), the perspective of the line will of course always be a great circle. When inversely the apparent path of the same meteor, seen from *one* place of observation is a great circle, the true path must lie in a plane. When the apparent paths, seen at the same time from two different places, not situated in the direction of the apparent path, are both great circles, the true path lies in two different planes, and *must be a straight line*. Now Prof. Oudemans at Utrecht says positively that the apparent path of the phenomena of November 17 was a great circle, cutting the horizon (and also the equator) in two opposite points. Of the English observers I will cite Mr. Saxby (p. 86), who describes "the trajectory as much flatter than that of the

stars." Moreover the general fact is, as I proved in my paper, that this trajectory, having been seen of regular form and consequently probably of equal curvature in its whole length, intersected the great circle of the horizon in two opposite points, and therefore must have been a great circle itself. The above-mentioned condition being fulfilled, I was under the necessity of taking the true path as a right one. I think this peculiarity indicates the meteoric nature of the phenomenon and of all the auroral arcs: (*les arcs proprement dits* of my theory) showing as great circles of the sphere. In fact a curve cited by Mr. Backhouse, lying at equal height above a terrestrial parallel, will show itself *but in one case* as a great circle, namely where the observer is within its plane. From all other places it will be seen as a small circle of the sphere. In this case is the apparent boundary of an aurora in the north, the arch of the dark segment cutting the horizon in two not opposite points.

I dare not occupy more space to answer Mr. Backhouse further on the influence exercised by cosmic matter on terrestrial magnetism, and the consequence of the general direction east to west of these currents when passing in the neighbourhood of the earth, but I think that this direction east to west *must* be deduced from the observed facts.

I am much obliged to Messrs. Petrie and Muirhead for their information. As to the remark of the former on the spectrum observed by Dr. Rand Capron, I think that the auroral character of some phenomena will be proved the best when it shows the auroral lines, whatever may be the origin of its light. When its other properties point out its meteoric character, a strong argument is found in favour of the cosmic theory of aurora.

H. J. H. GRONEMAN

Groningen, Netherlands, February 14

#### The Orbit of the Great Comet of 1882

I AM very much obliged to those gentlemen who have kindly given me the information required in my letter published in NATURE, vol. xxvii. p. 314.

They all agree on the same point, which confirms my opinion that in all the good observations the same or very nearly the same point of the head was observed during the brightest appearance of the comet.

I remarked especially in the sketches shown to me by Mr. A. A. Common, who was the first to see the comet in England, on September 16, and who continually made careful observations of it, that, although the nucleus was seen since October 30 divided into two parts, always one of these (which I shall call the main part next to the following end of the nucleus) remained the brightest. Mr. Common in every drawing marks this part with the word "brightest." At the Washington Observatory also this same bright point was always observed with the transit instrument, as it is stated by Mr. W. C. Winlock in his letter (NATURE, vol. xxvii. p. 129).

Mr. W. L. Elkin, Cape Observatory, in a communication to the *Astronomische Nachrichten*, No. 2490, speaks about this orbit. He used the first observation made at the Cape on September 8, the observation [of the] disappearance of the comet at the sun's limb on the 17th of the same month, and a normal observation on November 17, to calculate either a parabolic orbit or an elliptic one; but none of these gave the positions of the comet according to intermediate observations.

Mr. Elkin believes it is possible to take as the most probable value of  $\frac{1}{a}$  the value 0.0075, and consequently the comet has a

very long period, while Mr. Morrison in his calculation of the orbit had  $e = 0.9998968$ , and a period of 652.5 years.

As errors of observations are of course inadmissible, it is now the question to study what produces such great differences in calculating the orbits.

Are they due to disturbances during the comet's passage through the solar system, and especially at its passage through the sun's corona? or are they due to the hypothesis specified by Mr. Elkin and others that the centre of the nucleus is not the point gravitating around the sun? This question cannot be decided but by a careful discussion of all the positions of the comet during the whole period.

The observations before perihelion are of course very important. Unfortunately at the Cape the astronomers were prevented making observations between September 8 and September 17 because of bad weather; but there are some observations made in Melbourne and in other observatories before September