

different effects of iridectomy in cases of acute and chronic glaucoma. Dr. Johnson then proceeds to describe an operation which he terms scleral paracentesis, and describes as new, but which we have seen performed both by Mr. Hancock and by Mr. Power many years ago. In point of fact, Mr. Hancock's operation was a scleral paracentesis, and his view, which is not altogether incorrect, and was based on observation, was that in glaucoma a circumcorneal depression could be seen which he imagined to be due to the ciliary muscle, and his section, made with the same instrument recommended by Dr. Johnson, namely, a Wenzel's double-edged knife, was made through the sclera with the object of dividing the ciliary muscle; and the excellent results obtained in some cases show clearly that the escape of the vitreous which followed the incision, accompanied, when the anterior chamber was opened, by the aqueous humour, was quite enough to afford relief to all the symptoms and to restore vision, even if the spasm of the ciliary muscle was quite imaginary. We do not, however, wish to deprive Dr. Johnson of the credit of having thought out this method of procedure, though he may rest assured that he will meet with many cases of chronic glaucoma that will derive no benefit from scleral paracentesis, and that he will have to be careful in promising success from his operation in such cases.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

#### An Unnoticed Factor in Evolution

Two observed biological facts seem to oppose great difficulties to any explanation on evolution principles; difficulties admitted by evolutionists as well as their opponents. I mean—

(1) The fact that varieties produced by artificial selection, however divergent, are always fertile among themselves, while species supposed to have been produced naturally by an analogous process are often not mutually fertile even when very slightly divergent; and

(2) The fact that species evidently derived from a common ancestor, and differing only in small points of marking, though not fertile with one another, are often found side by side in places where it would seem that cross-breeding must prevent any division of the ancestral species into divergent branches.

The first seems to require that a period much greater than that of artificial selection should be necessary to produce sterility between descendants from the same ancestor; a supposition which would require an almost incredible period for evolution as a whole. The second seems to require that many species now intermixed should once have been geographically separated, sometimes in cases where this is very difficult to imagine. Both these difficulties are completely removed if we suppose mutual sterility to be not the *result* but the *cause* of divergence.

As far as can be judged, "sports" are as likely to occur in the generative elements (ova and spermatozoa) as in other parts of the body, and from their similarity in widely unlike groups it seems certain that a very slight variation in these elements would render their owner infertile with the rest of its species. Such a variation occurring in a small group (say the offspring of one pair) would render them as completely separate from the rest of their species as they would be on an island, and divergence (as Wallace has sufficiently shown) would begin. This divergence might progress to a great or a small extent, or even be imperceptible, but in any case the new species would be infertile with the species it sprang from.

If this theory be admitted, we must distinguish between varieties and species by saying that the former arise by spontaneous variations in various parts of the body, and only gradually become mutually infertile (thus becoming species), while the latter arise sometimes in this way, but sometimes by spon-

taneous variations in the generative elements, and are in this case originally mutually infertile, but only gradually become otherwise divergent.

I would suggest the following tests, and should be glad of any facts, from experience or from books, which can help in applying them:—

(1) If this theory is true we ought to find species (incipient) mutually infertile, but not otherwise distinguishable; and

(2) We ought to find that island and other isolated species which have arisen not by limited fertility but by geographical instead of physiological separation are often mutually fertile even when as widely divergent as the artificial varieties of dogs or pigeons.

EDMUND CATCHPOOL

The Grove, Totley, Sheffield, October 23

#### Earthquake Measurement

IN an article on "Earthquakes" in last week's NATURE (p. 608), Dr. H. J. Johnston-Lavis takes exception to the records of earthquake motion which I have published, on the ground of their complexity, and pronounces the Plain of Yedo unsuitable for earthquake observations.

Now this seems to me to be a very eclectic way of treating earthquakes. We can measure earthquakes only where we find them, and I suppose the first qualification in a site for an earthquake observatory is that there should be plenty of earthquakes. The Plain of Yedo possesses this qualification in a very high degree; and if the disturbances which occur in it are of a very much more complex character than our *a priori* notions about earthquakes may have led us to expect, it is not the Plain of Yedo that is to blame.

I fully agree that on a rocky formation the results will be different from those I found on an alluvial plain, but the instruments and methods which have been successful on the one are just as applicable to the other. The seismometers which have been used in Japan will serve to measure, with equal accuracy, earthquakes of a similar degree of destructiveness in other places, whatever be the nature of the ground. And several of the types already employed need little more than a change of scale in their construction to suit them for such formidable convulsions as the Ischian earthquake, to which your correspondent refers.

In describing and figuring a number of proposed seismographs, Dr. Johnston-Lavis has very frankly disclaimed a technical knowledge of mechanical construction, and for that reason all minute criticism of his suggestions may be withheld. If however he will refer to the *Transactions* of the Seismological Society of Japan, or to my "Memoir on Earthquake Measurement," he will see that some of the devices he suggests are not new. The plan of registering the amplitude of a pendulum's motion relatively to the earth by making the bob draw up a thread through a hole in a plate fixed below it was used some years ago by Dr. G. Wagener; and a massive slab free to roll on spherical balls formed in 1876 the seismometer of Dr. G. F. Verbeck. It was re-invented a year or two ago by Mr. C. A. Stevenson, and described by him before the Royal Scottish Society of Arts. The theory of the apparatus is discussed in §§ 31-32 of my memoir. Dr. Johnston-Lavis's plan of recording the azimuth of a movement by means of numerous electric contacts and "a pile of electromagnets" is a very retrograde step from the perfectly successful method, used in Japan, of resolving all horizontal movements into components along two fixed directions, these components being independently recorded in conjunction with the time.

Speaking of the use of the common pendulum as a seismometer, the author says that by using a short pendulum we may measure oscillations of short period, and by using a long pendulum we may measure slow earth-tiltings. Almost the reverse of this is the case. A short pendulum acquires, by earth movements of short period, a swing which cannot be distinguished from the movements we wish to measure, and whose extent depends on the accidental agreement of its period with theirs; but a short pendulum can be properly used to record slow earth-tiltings, with respect to which it is sensibly dead-beat. A long pendulum can be used to measure short-period movements; it can also be used (and its only advantage over a short pendulum is greater sensitiveness) to measure slow tiltings.

For vertical motion Dr. Johnston-Lavis condemns (but without giving any reason) my own and another vertical-motion seismograph—which theory and experience agree in proving

trustworthy—and proposes an instrument in which a weight drives a clock-train furnished with a centrifugal speed-indicator. The changes of apparent weight of the driver caused by the earth's up-and-down motion are to cause fluctuations in the speed of the driven train, which are to be recorded in conjunction with the time. The plan is, I think, new, but a less direct method of measuring vertical movement could scarcely be imagined. The fluctuations in speed will follow the changes of pull exerted by the driver with diminished amplitude and retarded phase, and superposed on them there will be fluctuations following no rule, due to inconstant friction and to mechanical imperfection of the train, as well as the continuous acceleration which follows the starting of the mechanism. To interpret the records would be altogether impracticable.

The design of a seismograph is a problem in applied dynamics which has of late years received a number of very satisfactory solutions. Of instruments capable of determining earthquake movements in absolute measure, and with reasonable exactness, there is now no lack; and it would be a pity if their wider employment were in any way retarded by the publication, on the authority of Dr. Johnston-Lavis, of suggestions which may fairly be said to lie outside the sphere of practical seismology.

University College, Dundee, October 27 J. A. EWING

### The Sky-Glows

THE description of the sky-glows as seen by Prof. A. S. Herschel may justify an account of some seen near the University of Virginia, Virginia, during the past spring, from notes made at that time.

February 25.—For several days before this date there were (if one may so call them) the normal glows at and after sunset. On this day there was seen a single pink ray with well-defined edges, about 4° broad, perpendicular to the western horizon, reaching half way to the zenith.

March 24.—Ten minutes before sunset, the sun being behind a small cloud, the bright oval “glare” in the west, which preceded nearly all the after-glows, was seen with its centre at an elevation of 15° (all these heights are rough estimates). It was 10° in diameter, and was surrounded by a band of a hazy reddish ashen colour (this band was usually seen with the “glare”) about 5° wide, which deepened in tint towards the horizon, and there spread out on each side of the “glare” so as to form a somewhat triangular support for it. At 6.30 the sun set. No colour had yet appeared on the eastern horizon. The “glare” now seemed almost triangular in shape, with the deepest ashen tints at the lower corners. As the sun descended, the “glare” diminished in intensity from the apex of the triangle. At 6.35 there was a ruddy colour on the eastern horizon, which spread in a triangular shape, apex upward, to a height of 25° to 30°, and at 6.40 was an exact image of the “glare” in the west, except that there were clear red tints instead of ashen, which were deepest at the lower corners of the triangle. The colour triangle then gradually rose from the eastern horizon, apparently following the sun, till at 6.48 the pink tint appeared in the western sky, increased in intensity, and was deepest at an elevation of 60°. The colour in the east was now gone. (Several attempts were made to observe the passage of colour across the zenith, but in no case was there success.) The western horizon was dazzling topaz-yellow, above the yellow pale blue, then faint pink to the deepest pink. The pink gradually descended toward the horizon, and when within 20° merged into the ordinary sunset colour at 7.0. The general phases of the glow were as follows:—Triangular ashen haze with oval “glare” in west, base of triangle on the horizon at sunset. Ten minutes later, triangular ruddiness in east, with base on the horizon. Another ten minutes, pink in the west. Ten minutes more, colour disappears. This succession was also noticed on March 15. On March 4 the glow in the west reached its most intense colour twenty minutes after sunset, but lasted twenty minutes, disappearing forty minutes after sunset.

On this evening (March 24) at 6.45, a cloud in the western sky, there being then no pink there, at an elevation of 35°, was coloured pale pea-green. This colour of the clouds floating at an elevation of 35° was seen on other days, while the clouds above and below retained their ordinary appearance.

March 26.—After the same phenomena as detailed in the last, even to the colour of the clouds, twenty minutes after the disappearance of the first glows, at 7.20 there was a pale rose-glow at an elevation from the western horizon of 30° to 35°, which

reached almost to the Pleiades, of which six were then visible. This second glow lasted about twenty minutes, and seemed to descend to the horizon. It was almost identical with the first, but fainter.

March 29.—Same as preceding, without second after-glow; tints extended 60° to 70° from horizon.

These after-glows were noticed more or less during April, July, and September, and here in Cambridge during this month there have been several vivid displays.

Harvard College, Cambridge, Mass.,

October 23

I BEG to inclose you an extract from a letter lately received by me from my cousin, Mr. Leeming, in the hope that it may interest some of your readers.

ELLEN A. DAY

Greycoat Hospital, Westminster, October 24

*Extract from a Letter written by Thomas Leeming, Surgeon and Naturalist on Board H.M.S. “Gulnare,” on the Admiralty Survey off Newfoundland*

“Gallois, Hermitage Bay, Newfoundland,  
September 12, 1884

“There is one thing I have more than once forgotten to mention to you, that is, an unusual appearance in the sky there has been now for some months, which I think must be connected with the red sunsets of last winter. In the finest weather the sun has always about it a haze (not watery) extending some 20° or 30°, white in the day-time, but as the sun nears the horizon the sky has a pale salmon or ochrey tint. In the immediate neighbourhood of the sun, the sky is of a vivid whiteness. This appearance continues some time after sunset. I have tried more than once to reproduce this effect, with water-colours, but without success. Let me know if you have observed or heard of anything of the same kind. I may also mention that there has been until lately a great scarcity of stars; even on the fairest and darkest nights very few visible under the third magnitude, and the Milky Way scarcely to be seen at all. Things, however, are mending in this respect.”

### Peculiar Ice Forms

WALKING up from Chamounix to the Montanvert a fortnight ago, I came upon a form of ice which I think can hardly be of common occurrence, as I have not met with any description of it, and have only once before seen it, and then also on the same mountain side, and under similar conditions of season and weather.

The bank, which in this particular spot slopes at an angle of about 45°, and faces the north, is bare of vegetation for some 30 feet in depth, and 100 to 120 feet in length, the hillside above being clothed with moss, ferns, and the usual undergrowth. This bare slope was almost covered with a coating of ice nearly four inches in depth, and of very curious structure, being formed in four layers, the three upper layers each about an inch in depth, and the lowest, which rested on the soil, being from five-eighths to three-quarters of an inch. Each layer was composed of an aggregation of filaments or elongated crystals, one-sixteenth of an inch and downwards in diameter, and all of a length equal to the thickness of the layer, ranged side by side like organ-pipes or basaltic columns, and with pyramidal ends; the bottom points of one layer resting on the top points of the one below, so that the layers could be easily detached one from the other. The whole mass was pierced by vertical cylindrical cavities from half to a quarter of an inch or less in diameter, and in most cases penetrating from top to bottom, so that a pencil-case could be dropped through endways. A horizontal section presented somewhat the appearance of Gruyère cheese, minus the colour of course, and with the solid part showing the crystalline form described above.

The mass had evidently been pushed up from below, because, while the ice itself was perfectly white and colourless, it was covered at the surface by a layer of dirt which might very likely have concealed it from observation if it had not happened to be broken. There was a good deal of snow higher up—nine inches at the Montanvert—and the weather was fine, with bright sunny days and hard frost at night. This particular part of the bank was in shade all day, and hardly thawed at all. I imagine that the porous detritus forming the surface of the bank was underlain by hard rock (though it did not occur to me at the time to ascertain if it was so, and at what depth), and that the water