

specimens of *Potentilla reptans*, less than half an inch in diameter, and even in unexpanded buds, were the pollen-tubes penetrating the stigmas.

I call attention to pollen-tubes, because, unless they be observed, one cannot feel absolutely certain that the flowers are really self-fertilised; and even then, that fact must be associated with the relative positions of anthers and stigmas, and the resulting abundance of fruit.

Another point I would mention of importance is the necessity of observing the order of emergence of the whorls. The subsequent rates of growth may prove a source of deception, so that it is necessary to go back to the very earliest condition when the parts are little more than papillae, and if possible even before one or more of the whorls have put in an appearance at all. Now I find that in conspicuous flowers, with certain exceptions, the corolla is very often the last to emerge, though ultimately it attains by far the largest size when adult; that the stamens usually come directly after the calyx, which, if present, is always first, acting as a protecting and nourishing organ; and that the pistil comes next. Such an order results usually in protandry; but while conspicuous species, as *Stellaria Holostea*, and *Cardamine pratensis*, have the order, calyx, stamens, pistil, corolla, inconspicuous self-fertilising species are often as follows:—e.g., *Cerastium glomeratum*—calyx, pistil, stamens, corolla, and *Nasturtium officinale*, calyx, stamens and pistil (together), corolla. These examples, out of many collected, appear to point to an important connection between the order of emergence and development on the one hand, and cross and self-fertilisation on the other. The connection between these two orders of facts I take to be, as already stated, due to the fact that the energy of conspicuous flowers is diverted into the corolla, which thereby delays the development of the pistil; but when the corolla is arrested, the pistil recovers itself, and its growth is equal to or precedes that of the stamens, the result issuing in a synchronous maturity, and consequently self-pollination.

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Wallace's "Geographical Distribution of Animals"

ALLOW me to point out in NATURE a few errors which occur in Mr. Wallace's "Distribution of Animals," regarding the extinct mammalian fauna of India.

In the first place, there is a mistake regarding the locality of the Perim Island (vol. i., p. 362, vol. ii., pp. 157 and 221), from which Tertiary fossils have been obtained; in Mr. Wallace's book the Perim Island, at the entrance to the Red Sea, is the one referred to, whereas the true spot is Perim Island, in the Gulf of Cambay.

There is, therefore at present no known spot to the eastward of India which shows the former extension of its Tertiary mammalia into Africa and Europe, although such extension doubtless existed.

The extinct genus *Enhydriodon*, from the Siwaliks (e.g. vol. ii., p. 200), is always referred to as *Enhydriodon*.

In vol. i. (p. 122) the genus *Tapirus* is mentioned as occurring in the Miocene of the Punjab; this determination is on the authority of Dr. Falconer, who hastily examined a single tooth (now in the Indian Museum); this tooth, and others subsequently found, turns out to belong to the European Miocene genus *Listriodon*; the only other mentioned occurrence of a fossil *Tapir* in India, is by Mr. Clift, who figured the symphysis of a mandible (*Geol. Trans.*, sec. ser., vol. ii.) from Burma; this may, however, also belong to *Listriodon*.

In vol. ii. (p. 202) the genus *Ursus* is mentioned as having been described from the Siwaliks and the Nerbudda Valley; it has only been described from the latter locality, *Hyæna* being the Siwalik genus. A new species of tame *Ursus* has, however, been obtained this year from the Siwaliks, and will be subsequently described.

In vol. ii. (p. 212) *Hipparion* should also be mentioned as having been found in India as well as in Europe.

At p. 228 of the same volume, it is stated that *Elephas* has "perhaps one species Pliocene in Central India;" in reality there are two species undoubtedly from the Newer Pliocene of the Nerbudda Valley, viz., *E. nomadicus* and *E. (Stegodon) insignis*.

Vol. ii., p. 240, the genus *Hystrix* has been fossil in the Siwaliks of India as well as in Europe and America.

I may add that, as announced in the August number of the "Records of the Geological Survey of India," for the present year, I have determined the existence of a species of *Manis* (the

first fossil species of the genus) and of a Cetacean, with other new forms, from the Siwaliks.

RICHARD LYDEKKER,
Calcutta, August 27
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The Resistance of the Electric Arc

FOR the purpose of determining theoretically the best arrangement of cells for the production of the electric light, it was necessary to know the resistance of the electric arc. Not being acquainted with any source from which this information could be derived, we determined this resistance experimentally in two distinct ways.

1. The current from sixty new Grove's cells joined in series (and of which the immersed part of each platinum plate was about 13 square inches in area and of each zinc plate about 25 square inches) was used to produce an electric light with a Dubosq's lamp, when a small known resistance consisting of many metres of thick bare copper wire hanging in the air was also introduced in circuit. This wire was sufficiently thick for its resistance not to be sensibly altered by the passage of the current. The difference of potentials between the carbons was measured with a Thomson's quadrant electrometer, using the induction plate and compared with the difference of potentials between the two ends of the wire of known resistance. These two measurements were made rapidly one after the other and repeated very many times. Then since at any moment the same current is flowing through the electric arc and the wire, the two differences of potentials measured rapidly one after the other are proportional to the resistances.

The above method showed that the resistance of the electric arc varied considerably even when the light appeared quite steady, that the resistance was never more than 20 ohms, and had an average value of about 12 ohms.

2. On another occasion the current from eighty similar Grove's cells joined in series, which had been joined up for three hours, and used at intervals during this time for the production of the light, was sent through the coils of a differential galvanometer. In one circuit was a very high resistance and in the other the electric arc; each coil of the differential galvanometer was shunted with a wire of small resistance. Nearly the whole current, therefore, went through the arc. The shunts being properly adjusted to obtain balance, the resistance of the arc, as in the previous case, was found to vary much but never to exceed 29 ohms and to equal about 20 ohms when the light was best.

That the resistance would be larger than in the previous case was to be expected since the battery contained more cells, and a brighter light would, therefore, be obtained with the carbon points further apart.

At a convenient opportunity we hope to take time readings of the resistance together with photographs of the light on a revolving band of sensitive paper in order to determine the exact resistance corresponding with the brightest light for any particular number of cells.

The results, however, given above show that with cells such as we used, and which are the common Grove's cells employed in England, no attempt should be made to join any of the cells in parallel circuit until at least 200 have been joined in series, for since the resistance of each cell is about 0.2 ohms, 200 of them would have a resistance of 40 ohms, a resistance certainly less than double the electric arc for that battery corresponding with brightest light, and we have shown (*Telegraphic Journal*, March 15, 1873) that the cells of a battery should be joined in series until the battery of resistance is double the external resistance, at which point the battery should be joined in two rows each containing half the whole number of cells in series, and the two rows connected in parallel circuit.

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JOHN PERRY

Habits of Animals Transmitted to Offspring

BREEDING many horses yearly on my station, I notice, as a matter of course, some of their peculiar habits. In a semi-wild state on a run horses graze together in large or small companies, which "station hands" call "mobs;" these mobs wander at will over a large area of country, finding abundance of good natural pasture and water. Some years since a mare became solitary in her habits, always seeking one particular creek; whenever released from work she made off to her favourite feeding ground by herself; if "rounded up with a mob" she would take the earliest chance that presented itself of reaching

her usual haunt. One of her progeny some years after showed a similar liking for solitude; he was placed among several other horses (many of them he had known for years) on a small run intersected with bushy gullies, more or less rocky. He was soon missing, and search was made for him for some time without success; he was supposed to have come to grief in the bush; at length he was found, most unexpectedly, on a small patch of pasture between two rocky gullies thickly bushed; this spot was so difficult of access that a slight track had to be cut to get the horse back. Having been brought from a large station where he was bred and reared, he no longer enjoyed a great range by which he could place any long distance between his companions and himself; he displayed much tact and judgment in the way he secured the indulgence of hereditary habit, by discovering and reaching with difficulty an almost inaccessible solitude. One of the best and fleetest stock mares for the fast and hard work of "cutting out" was a beautiful creature notorious as an incorrigible kicker; she has most faithfully transmitted this vice to her offspring.

Peculiarity in the formation of the hoof has been handed down to descent after descent by a grand old mare who had this blemish as a slight counterpoise perhaps to her many virtues.

A particular strain of Dorking fowls, which I have had for thirty years or so, always shows a restless desire for rambling, and that too under the difficulty of meeting with much persecution when straying beyond their ample range. This special family always exhibits what may be termed the gift of locality.

Ohinitahi, N.Z.

THOMAS H. POTTS

Moon-Stroke

THERE is a popular belief that it is dangerous to sleep in full moonshine, as it is supposed to produce some injurious effect called moon-stroke. I have little doubt that the popular belief is well founded as far as the injury to some of those who have slept out at night is concerned, especially in full moonshine; nevertheless the injury is not, I think, due to the moon, but to another cause, which I shall here attempt to explain. It has often been observed that when the moon is full, or near its full time, there are rarely any clouds about, and if there be clouds before the full moon rises they are soon dissipated, and therefore a perfectly clear sky, with a bright full moon, is frequently observed.

A clear sky admits of rapid radiation of heat from the surface of the earth, and any person exposed to such radiation is sure to be chilled by rapid loss of heat. There is reason to believe that, under the circumstances, paralysis of one side of the face is sometimes likely to occur from chill, as one side of the face is more likely to be exposed to rapid radiation, and consequent loss of its heat. This chill is more likely to occur when the sky is perfectly clear.

I have often slept in the open in India on a clear summer night, when there was no moon, and although the first part of the night may have been hot, yet, towards 2 or 3 o'clock in the morning, the chill has been so great that I have often been awakened by an ache in my forehead, which I as often have counteracted by wrapping a handkerchief round my head and drawing the blanket over my face. As the chill is likely to be greatest on a very clear night, and the clearest nights are likely to be those on which there is a bright moonshine, it is very possible that neuralgia, paralysis, or other similar injury, caused by sleeping in the open, has been attributed to the moon, when the proximate cause may really have been the *chill*, and the moon only a remote cause acting by dissipating the clouds and haze (if it do so), and leaving a perfectly clear sky for the play of radiation into space.

Lucknow, August 26

E. BONAVIA

The Memoirs of the Geological Survey

I DESIRE through the medium of your columns to call attention to the fact that most of the admirable memoirs of the Geological Survey appear to be out of print. A week or two ago I ordered a number of these publications and was informed that at least half of them are out of print. Prof. Ramsay's "Geology of North Wales" is in this category and the fact is stated in the printed list, but in a letter recently received from

"Cutting out" is drafting a beast out of a mob, following it through all its wild rushes, twistings, and turnings, through perhaps many hundreds of cattle, never leaving it till it is fairly drafted out. This work often taxes the skill and energy of stockman and his horse pretty severely.

the professor he informs me that the work is being reprinted, and is expected to be published about the middle of next year. Without, in the absence of information, desiring to attach blame to any one, I shall be glad to know the reason why works admittedly of the highest value should have been permitted to fall into such apparent neglect.

WM. HORSFALL

Manchester, October 9

OUR ASTRONOMICAL COLUMN

CHACORNAC'S VARIABLE NEBULA NEAR ζ TAURI.—On October 19, 1855, Chacornac remarked that a star of the eleventh magnitude, north-preceding ζ Tauri, was enveloped in nebulosity, which was sufficiently bright up to the end of January following to occasion surprise that it had not been previously detected. The star had been repeatedly observed in 1854.

Chacornac gives the position of the star upon which the nebula was projected for 1852.0 in R.A. 5h. 28m., 35.6 N.P.D. 68° 52' 42". The form of the nebula was nearly rectangular, the longest side subtending an arc of 3½' and the shorter, one of 2½'. The star occurs in the zone observed at Markree, January 16, 1850, without mention of surrounding nebulosity.

On September 12, 1863, and January 25, 1865, D'Arrest observed the star with the Copenhagen refractor, on the last occasion "*coelo valde eximio*," without being able to detect any trace of the nebula. He estimated the star 11.12 m., and noticed another 13 m., about 40' preceding nearly on the parallel.

From Chacornac's position for 1852, it appears the star precedes ζ Tauri 12.5s., and is N. 4' 28". It may be recommended for examination during the approaching winter, particularly with telescopes of moderate dimensions, which in the case of another suspected variable nebula (Schönfeld, 1858) have been shown to possess decided advantage over the larger instruments.

OLBER'S SUPPOSED VARIABLE IN VIRGO.—Mr. Tebbutt of Windsor, N.S.W., communicates the results of some observations of this object and neighbouring stars, made in July and August of the present year. For 1876.0 he found:—

Star.	Magnitude.	R.A.			N.P.D.		
		h.	m.	s.	°	'	"
1	7	13	3	7.7	105	51	12
2	9½	13	5	17.6	105	51	15
3	8½	13	7	32.4	105	53	50
4	9	13	9	12.9	105	37	1

No. 3 is the supposed variable. See this column, 1876, April 13.

RELATIVE BRIGHTNESS OF URANUS AND JUPITER'S SATELLITES.—On the evening of June 5, 1872, M. Prosper Henry, at the Observatory of Paris, took advantage of the very close approach of Uranus to Jupiter (difference of declination only 1' 2" at conjunction) to compare the light of the satellites of Jupiter with the former planet. He found the brightness of Uranus was equal to that of the third satellite, which was nearest to Uranus at the moment. If there existed any difference of light between the two others, it was to the advantage of Uranus, but in any case it was very small. The observations were made with the large Foucault telescope. So favourable an opportunity of making these comparisons may not occur again for a very long period.

BLANPAIN'S COMET, 1819.—A new reduction of the observations of this remarkable comet, taken at Paris, of which we have the particulars in detail, and recalculation of the elements thereupon, appears to lead to a somewhat longer period than was inferred by Encke, from the same observations as at first reduced. This somewhat longer period—a little over five years—would occasion a near approach of the comet to the planet Jupiter at the previous aphelion passage, and it is easy to see that the observations would allow of so close a proximity at this