Fireball

READING W. G. Smith's remarks on lightning in last week's NATURE (p. 241), recalls to my mind a ball I saw during a storm in the autumn of 1881. The storm had lasted some time, and I sat reading a little back from an open window but facing it. Suddenly it became so dark that I could no longer see. I dropped my book and looked out. A ball of fire was passing through the window into the room. It moved very slowly onwards and downwards towards me, and became almost stationary over my book. At first I thought it rested upon it, but I soon saw it was moving slowly across. Having passed over the book, it turned in the direction of my hand, paused just beneath it, and then sank towards the carpet. At this instant a peal of thunder crashed over the house—it was the very loudest I have ever heard. ANNIE E. COCKING

The Elms, Bedford Park, Chiswick, W., July 14

Butterflies as Botanists

THERE can be no doubt, as pointed out by Fritz Müller in your last issue (p. 240), that the habits of insects often indicate affinities in plants. There is doubtless a strong affinity between the Solanaceæ and Scrophularineæ; the small oval pollen is almost identical in both. The habits of fungus parasites sometimes disclose similar relationships, often more real than is at first apparent; we have an example of this in the fungus of the potato disease, *Peronospora infestans*. This parasite is almost peculiar to the Solanaceæ, being especially destructive to Solanum, Lycopersicum, and Petunia, but at times it invades the Scrophularineæ and grows on Anthocersis and Schizanthus. It is not common to find one parasitic fungus attacking the members of two natural orders of plants, but other examples could be given.

W. G. S.

A Cannibal Snake

ABOUT eighteen months ago, just previous to my leaving India, at Devalah in the Wynaad, the horsekeepers chased and killed a large cobra, 5 feet 4 inches; previous to death it was thrown down in front of the door of our house, when, after a good deal of twisting and wavy contortion of the body, it disgorged a small rock snake over 4 feet in length. I had heard of the same thing before in India, so that I do not think cannibalism in snakes is so uncommon as Mr. Evans thinks.

JOHN FOTHERINGHAM

96, Netherwood Road, West Kensington Park, W., July 12

FOURTH NOTE ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY

IN my communication to NATURE (vol. xxix. p. 528) I described the use of alternating currents and the telephone for the above purpose, and promised to endeavour to obtain at least an approximate measurement of the E.M.F. developed in the secondary coil of an induction apparatus. This promise I now propose to fulfil. But before proceeding to the special subject of the present note, I should wish to draw attention to a paper which appeared on the 15th of the same month in the *Asclepiad*, by that able experimentalist Dr. B. W. Richardson. He therein describes not only experiments made with the large induction coil of the Polytechnic, but also others made as early as 1868 in conjunction with the late Mr. Becker, the object of which was to obtain a measure of the resistance of animal structures.

"The results," says Dr. Richardson, "were not fully satisfactory. They were variable even when the conditions under which the experiments were made were entirely the same. This variability we found to be due to decomposition of the animal substance, a decomposition which, however feeble the battery, was sufficient to destroy the precision we desired to obtain." Putting the more recently coined word "polarisation" for decomposition, this expresses exactly the difficulty described by me in my first note. "It was, however, possible," says the doctor, "to make out that blood conducted better than any other structure of the body, and better than water." I can now fully corroborate this excellent observation, and perhaps extend its application.

Physiological and even pathological fluids, such as the serum of dropsy, conduct far better than muscle, bone, and nerve. One instance out of many may serve. In the very first case recorded in my communication to NATURE (vol. xxviii. p. 151) the lowest resistance obtained from foot to foot was 2300 ohms. The patient was then very emaciated, but quite free from dropsy. Towards the end of the case, which after death proved to be one of ulcerative endocarditis, as I had considered it to be during life, slight but distinct dropsical effusion in the lower extremities set in; the resistance sank at once to 700 ohms, and I had to discontinue my observations from the evident change of electrical conditions. I have since verified the same fact many times, and on it I partly found the belief, already several times stated, that "the human body, in spite of its large amount of liquid constituents, follows a similar thermal law of resistance to that influencing solid conductors, though in a very much higher ratio" (NATURE, vol. xxviii. p. 152).

Dr. Richardson does not seem to have attempted to determine the resistance of the living body, which Du Moncel, in 1877, did, and with fairly accurate, if unpleasant, results (NATURE, vol. xxix. p. 528). On the discovery, however, in 1879, of Prof. Hughes's electric balance, he resumed his observations, this time with an alternating induction current, though he does not himself notice the important change. His results are unfortunately taken in arbitrary units on the graduated scale of 200 parts originally applied to Prof. Hughes's instrument. If there is any way of reducing these fictitious to absolute values, my work will be both lightened and assisted by a proved observer. Blood-clot and serum, white and gray nervous substance, muscle, bone, coagulated albumen, gelatine, and pus were all tested. Some of the results were excellent. For instance, fat, which by one experimenter has been stated to increase the conductivity of the body, is found by Dr. Richardson, as I also have found it, to be an absolute non-conductor. It is almost unnecessary to say that, with so skilled a chemist and physiologist, all proper temperature corrections and other similar precautions were most strictly observed.

I can now proceed to the main topic of my present note. On receipt of the Wurzburg dynamometer it was put in adjustment, and a strenuous effort made to compare the indications given with a constant and an alternating current, to both of which it is sensitive. But the movable suspended coil made of an ivory core, with a double weight of silk-covered copper wire, hung by a platinum hook, and dipping by its other termination into a vessel of strong sulphuric acid by means of a platinised platinum plate, is very heavy; takes a long time to get to its full deflection, thus allowing the battery to run down sensibly, and, what is worst of all, has a tendency to "integrate." By this I mean to sum up, by its mechanical inertia, a large number of small, intermittent pulls as given by the reversed current, into an almost identical deflection (less, of course, losses) with that given by the one steady pull of a continuous current. In spite of its beautiful workmanship, it had to be discarded for the present research. Somewhat in despair, I fell back on a similar instrument, shown by me at the Oxford meeting of the Physical Society in June 1882, and there heavily abused. The moving coil in this is made of silk-covered aluminium wire to insure lightness, and the bifilar suspension is made of the silver-gilt wire used for military epaulettes and facings. It is the work of my own poor hands.

Herr Obach then stated, and the statement was repeated in your columns, that this material had already been used by Messrs. Siemens for their "dust-recorders," but had failed by difficulty of making contact. On testing my little toy, I found its resistance had not altered in twenty-five months one fraction of an ohm, and that it moved briskly up to its maximum, standing there quite long enough for a good observation. Indeed, in spite of its condemnation by a jury of experts over two years ago, it was still so lively that I thought it better to check extra swing by a small platinum paddle 1 cm. square moving in sulphuric acid.

On a metre scale, at one metre distance, the reflected image in a telescope gave ·365 mm. deflection[1] with the whole induction current from Prof. Kohlrausch's metre-bridge, as described in my last note.

The object now was obviously to obtain an independent measure of the actual E.M.F. to which this deflection was due. The quadrant electrometer, or some other delicate potential measurer, of course suggested itself. A trapdoor portable, kindly lent me by Prof. McLeod, refused to take notice of my wretched little currents, limited as they are by human susceptibility. I do not possess a quadrant, nor will the Royal Society, though twice asked, lend me one. Here again my friends at Cooper's Hill came to my rescue, and I have to express my thanks, not only to Prof. McLeod, but also to Prof. Stocker and his excellent demonstrator Mr. Gregory, for their assistance. With my Kohlrausch induction bridge in a big bag I journeyed to Egham, and thence on foot to Cooper's Hill.

The formula to be made use of was obvious. It is given in Prof. Adams's Cantor lectures, and has been kindly verified for me by Prof. Hopkinson. In it the needle is connected with one pair of quadrants, so that $V_3 = V_1$. In this case—

$$\text{Deflexion} = \frac{k}{2}(V_1 - V_2)^2.$$

Prof. Adams has since shown me a different, and perhaps better, way of working, which I intend to make use of in the future. It was found that the two fine quadrant electrometers at Cooper's Hill College were unavailable; the one given by Lord Salisbury not admitting of the needle being placed in connection with either pair of quadrants, the other being disabled by some casual contact. We therefore with heavy hearts made a last struggle with the old Elliott pattern and single quadrants. This succeeded admirably, and on a mean of the four best out of six observations, we obtained a deflection of 107 with the intermittent current. "In order to be quite sure," Mr. Gregory wrote to me next day, "of the true value of the mean deflection we obtained, I have executed measurements with different numbers of cells. In these, the negative pole was to earth, the positive being connected at will to either pair of quadrants, and the needle also at will to either pair, giving four readings for each observation. I give only means, which agree well.

E.M.F.	Defl.	k
21 volts	32·75	·149
29 ,,	63·75	·151
47 ,,	161·75	·146
	Mean ...	·1486

k was calculated from the formula

$$\delta = \frac{k}{2}(V_1 - V_2)^2.$$

By calculation, using the mean value of k, the E.M.F. to give a deflection of 107 came out 38. By observation, using an E.M.F. of 38 volts, the deflection was 107·25. This agrees so well with the calculated value that it will be easy to evaluate the E.M.F. corresponding to any deflection by the above formula."

The effect of rapid alternations seems to be to lessen the deflection, though Mr. Glazebrook stated, in a paper read before the Physical Society, that with between 10 and 120 contacts per second the result, in charging a condenser, was not perceptible.

[1] The bridge arrangement being entirely disconnected.

On the whole therefore, though I agree with Mr. Gregory that we have not obtained a measure of the maximum E.M.F., but only an integration, disregarding sign, the approximation is, I hope, superior to any made before, and affords a good general basis for farther work.

W. H. STONE

GAS-BURNERS[1]

THE economist who wished to point the moral of a healthy competition in industrial commerce could scarcely find a better instance to his hand than the progress made by gas illumination under the impetus given in the last few years by the rise of electric lighting. It is not overstating the case to say that greater improvement in the use of gas has been made since Jablochkoff introduced his electric candle than in the previous sixty years' history of gas lighting. Compared with the recent development of invention, the long period of non-competition appears almost stagnant. With the introduction of electricity arose a popular demand for "more light." With a new illuminant competing for favour, consumers growled more openly at "bad gas" and high gas bills. Each advance of the electric light was greeted with acclamations by the popular voice, shareholders began to tremble, and gas shares came down with a rush. It was time for gas managers and manufacturers to bestir themselves. The happy days of a monopoly in light seemed over. The consumers have reaped the benefit. Under the stimulus of competition the price of gas has been lowered, impurities have been cut down. Some half a dozen years ago the great London Companies were endeavouring to prove before a Parliamentary Committee that coal-gas could not be purified from bisulphide of carbon without creating such a nuisance as to be intolerable. Their object was to do away with the lime purifiers, made necessary by the regulations of the Gas Referees, and to use only oxide of iron. Since the advent of the electric light not a word has been heard about the impossibility of purifying coal-gas by lime. On the contrary, every effort is now made to supply gas as free from sulphur as possible. But while the gas has thus been improved in quality and lowered in price, a still greater improvement has been effected in the methods of burning it. By the application of the regenerative principle to gas-burners, the illuminative value of coal-gas has been doubled.

But in spite of the great advances made in gas-burners, the public have by no means yet reaped the full benefit. Owing to the carelessness of gas-fitters and the ignorance of consumers, the great majority of those who light their houses by gas waste at least 20 per cent. of their gas as an illuminating agent. If the flame smokes, or flickers, or gives a poor light, most people put it down to "bad gas," when in reality the burner is unsuitable, or worn out, or the supply pipes (nearly always too small) are choked. To all who burn coal-gas in their houses, and are troubled with "bad gas," we can heartily recommend "Gas-Burners, Old and New," by Owen Merriman.

This little book, published at a price which places it within the reach of a large public, describes very plainly in popular language the evolution of the best modern burners of Sugg, of Bray, and of Siemens, from the original "cock-spur burner" of Murdock, and Accum's "tube with a simple orifice, at which the gas issues in a stream, and if once lighted will continue to burn with the most steady and regular light imaginable, as long as the gas is supplied." The illustrations are all that can be desired.

Owen Merriman has taken pains to insist on the two great *desiderata* of gas-burners—high temperature and low temperature, but we think he has gone too far in attempting to give a popular "theory of luminous combus-

[1] "Gas-Burners, Old and New." By Owen Merriman. (London: Walter King, 1884.)