

Fertilisation of the Fumariaceæ

THE accompanying note has been given me by my friend Mr. J. Traherne Moggridge, and I should feel obliged if you would insert it in NATURE with the view of eliciting the communication both of other similar phenomena, and of some explanation of them.

Mentone, March 18

Note on apparently useless Colouring in the Flowers of a Fumitory (*Fumaria capreolata* var. *pallidiflora*, *F. pallidiflora* Jord.)

I observe that in this plant at Mentone the flowers attain their brightest colouring after the ovaries are set, and when fertilisation is no longer necessary, or indeed possible. During the period previous to impregnation, the flowers are pale and nearly white, and the pedicels erect or horizontal; afterwards they become pink, or even crimson, and the pedicels are recurved, and the colour of the petals, which retain their form and position until the ovary has nearly attained its full size, intensifies with the lapse of time.

If the reverse had been the case there is little doubt that we should have regarded the bright colouring as specially adapted to attract insects, and as existing for that purpose, insects being, according to Prof. F. Hildebrand,* important agents in the fertilisation of fumitories; but here, as the brighter flowers are those which no longer need or are capable of profiting by the interference of insects, this explanation ceases to be possible.

This little fact, therefore, would seem to be one which might be classed with those which teach us that, side by side with the developments and modifications which are plainly beneficial to the organism of which they form a part, there are others, which, as far as we can see, are neither useful nor harmful to their possessor, though they may, and frequently do, supply features which especially attract our attention and admiration.

J. TRAHERNE MOGGRIDGE

OCEAN CURRENTS

TWO papers which Mr. Croll has recently published "On the Physical Cause of Ocean Currents" (*Philosophical Magazine* for Feb. and Mar. 1874), bring the main question at issue between him and myself into very distinct view; and as the results of the *Challenger* Temperature-survey of the Atlantic, lately made public by the Admiralty, afford (as it seems to me) important data towards the settlement of this question I shall be glad to be allowed to point out what seem to me their chief bearings upon it.

The position taken by Mr. Croll is, that all the great movements of ocean-water, deep as well as superficial, depend on the action of winds upon its surface. And whilst freely admitting that Polar water finds its way along the floor of the great ocean-basins into the equatorial area, he affirms that this is merely the reflux of the current which has been driven into the Polar basins by the agency of winds.

On the other hand, it is fully recognised by myself, that the *current* movements of *surface*-water are, for the most part, produced by the agency of winds; but these movements, I contend, all belong to a *horizontal circulation, which tends to complete itself*,—a surface in-draught being produced wherever a surface outflow is kept up, as we see in the horizontal circulations of the North and South Atlantic, the North and South Pacific, and the Indian Ocean, depicted in Mr. Croll's own map. But I maintain that the *deep* movements of ocean-water are the result of a *vertical circulation*, which is maintained by the continuance of a disturbed equilibrium between the Polar and equatorial columns, occasioned by the surface-action of Polar cold and equatorial heat.

As Mr. Croll is unable to understand why I should speak of Polar cold, rather than equatorial heat, as the *primum mobile* of this vertical circulation, and accuses me of an ignorance of the fundamental principles of

physics in so regarding it, I may be allowed first briefly to explain myself; since others may experience the same difficulty, from some want of precision on my part in stating my case. The eminent physicists, however, with whom I have had the advantage of discussing this point, do *not* share Mr. Croll's objection, but hold my statement to be perfectly correct.

Heat applied to the *surface* of any body of *fresh* water, whether by solar radiation, or by the experimental application of a heated plate, will raise the temperature of the *surface-film*, without producing any downward convection. Limited downward convection, however, is occasioned in *salt* water by the sinking of the surface-films which are concentrated by evaporation; but this convection I found in my Mediterranean observations, which have been fully confirmed by those of the *Challenger* in the equatorial area, to be practically limited to the first fifty fathoms. Water in a long trough may thus be superficially heated (as I have experimentally ascertained), by the application of surface-heat to one-sixth of its length, until the temperature of its whole surface-film is raised to 100° or more; but the further application of surface-heat expends itself in vaporisation, and does not communicate itself in any sensible degree to the mass of water beneath, which, therefore, *can not be put in motion* by such application. On the other hand, the moment that *surface-cold* is applied, a downward convection is produced, as Mr. Croll may easily ascertain for himself if he will only try the experiment; and the continued application of such surface-cold to any one portion of the surface will maintain a constant movement through the entire mass of the liquid, until thermal equilibrium is restored by the cooling-down of the whole. But if the restoration of this thermal equilibrium be prevented by the application of heat to another part of the surface, the disturbance of equilibrium will be kept up, and a *vertical circulation* maintained, as long as these two opposing agencies are in operation. If Mr. Croll cannot see that this *must be* the case, I am not responsible for his failure to apprehend that which theory and experiment alike sanction.

I re-affirm, then, that *cold* applied to the *surface* has exactly the same motor power as *heat* applied at the *bottom*; and that its motor agency is more potent than that of heat applied at the surface, simply because the former is diffused by convection through the entire mass of the water, which it keeps on *cooling* and *moving*, whilst the latter is limited to the surface-film, and expends itself in producing evaporation.

Mr. Croll objects to this, that, if it were true, nearly the whole mass of oceanic water must have an almost Polar temperature. I accept this issue; and refer to the *Challenger* temperature-soundings, as justifying it. If he will look at the section taken across the equator, he will find that—as I had *predicted*—Polar water there lies within a very short distance from the surface. At less than 100 fathoms' depth, the temperature falls from 78° at the surface to 55°, and the isotherm of 40° is reached at about 320 fathoms. Below this lies a *stratum of more than 2,000 fathoms' thickness*, whose temperature, ranging downwards from 40° to 32°.4, shows it to consist mainly of Polar water. And as, from the data supplied by the Mediterranean and Gulf of Suez temperature-soundings, a body of equatorial water secluded from all connection with the oceanic circulation might be expected to have the uniform (or isothermal) temperature of 75° from 50 fathoms downwards, it is clear that the influence of Polar cold here extends itself upwards within 100 fathoms of the surface.

Again, Mr. Croll says that I have made no allowance for the *excess* of salinity in equatorial water, which, according to him, must counterbalance the increase of specific gravity produced in Polar water by the reduction of its temperature. Here, again, he is unfortunate

* "Ueber die Bestäubungsvorrichtungen bei den Fumariaceen," in Pringsheim's "Jahrbücher," vol. vii. part 4, p. 423 (1870). Reviewed in "Bull. Soc. Bot. de France," xix. (1872), p. 145.

as regards his facts. He appears to have overlooked the observations proving the *lower* salinity of inter-tropical water, which I had cited as furnishing an additional indication that Polar water is constantly rising from the bottom towards the surface in the equatorial area. These observations have been most remarkably confirmed by those taken by the physicists of the *Challenger*. For, whilst in the extra-tropical area the sp. gr. of surface-water was in excess of that of bottom-water, in the equatorial area it was reduced to an almost precise correspondence with it, due allowance for temperature being of course made.

According to Mr. Croll's doctrine, the whole of that vast mass of water in the North Atlantic, averaging, say, 1,500 fathoms in thickness, and 3,600 miles in breadth, the temperature of which (from 40° downwards) as ascertained by the *Challenger* soundings, clearly shows it to be mainly derived from a Polar source, is nothing else than the reflux of the Gulf Stream. Now, even if we suppose that the whole of this stream, as it passes Sandy Hook, were to go on into the closed Arctic Basin, it would only force out an equivalent body of water. And as, on comparing the sectional areas of the two, I find that of the Gulf Stream to be about 1-900th that of the North Atlantic underflow; and as it is admitted that a large part of the Gulf Stream returns into the Mid-Atlantic circulation, only a branch of it going on to the north-east; the extreme improbability (may I not say impossibility?) that so vast a mass of water can be put in motion by what is by comparison such a mere rivulet—the north-east motion of which, as a distinct current, has not been traced eastward of 30° W. long.—seems still more obvious.

Lastly, the *Challenger* observations in the South Atlantic have proved exactly what I had anticipated, viz., that the bottom-temperature is lower, and that the Polar underflow lies much nearer the surface in this ocean than in the North Atlantic. Now this case appears to me to afford the *experimentum crucis* between Mr. Croll's doctrine and my own. For my prediction of this result was based on the fact, that, as there is here a perfectly open communication between the Polar and equatorial areas, the vertical circulation would take place more freely. On the other hand, according to Mr. Croll's doctrine, it would have been expected that there should be a far smaller reflux, or no reflux at all. For, though a portion of the equatorial current passes southwards when it meets the coast of South America, there is no ground whatever for believing that it ever goes near the Antarctic circle; and if it did find its way thither, there is no "closed basin" from which it can drive back a return current.

As it is usually considered in scientific inquiry that the verification of a prediction affords cogent evidence of the validity of the hypothesis on which it is based, I venture to submit that so far my case has been made good.

WILLIAM B. CARPENTER

THE DEATH OF DR. LIVINGSTONE

THE daily papers have obtained from the London office of the *New York Herald* the following telegram, containing details of the death of Dr. Livingstone, dated Suez, Sunday, March 29:—

"The *Malwa* arrived off Suez at eleven on Saturday night, having Mr. Arthur Laing and Jacob Wainwright on board, with the body of Livingstone. He had been ill with chronic dysentery for several months past. Although well supplied with stores and medicine, he seems to have had a presentiment that the attack would prove fatal. He rode a donkey, but was subsequently carried, and thus arrived at Muilala, beyond Lake Bemba, in Bisa country, when he said, 'Build me a hut

to die in.' The hut was built by his followers, who first made him a bed. He suffered greatly, groaning night and day. On the third day he said, 'I am very cold; put more grass over the hut.' His followers did not speak or go near him. Kitumbo, Chief of Bisa, sent flour and beans, and behaved well to the party. On the fourth day Livingstone became insensible, and died about midnight. Majuahra, his servant, was present. His last entry in diary was on April 27. He spoke much and sadly of home and family. When first seized he told his followers he intended to change everything for ivory, to give to them, and to push on to Ujiji and Zanzibar, and try to reach England. On the day of his death the followers consulted what to do. The Nassick boys determined to preserve the remains. They were afraid to inform the Chief of Livingstone's death. The secretary removed the body to another hut, around which he built a high fence to ensure privacy. They opened the body and removed the internals, which were placed in a tin box and burned inside the fence under a large tree. Jacob Wainwright cut an inscription on the tree as follows:—'Dr. Livingstone died on May 4, 1873,' and superscribed the name of the head man Susa. The body was preserved in salt, and dried in the sun for twelve days. Kitumbo was then informed of the death, and beat drums and fired guns as a token of respect, and allowed the followers to remove the body, which was placed in a coffin formed of bark, then journeyed to Unyanyembe about six months, sending an advanced party with information addressed to Livingstone's son, which met Cameron. The latter sent back a bale of cloth and powder. The body arrived at Unyanyembe ten days after advanced party, and rested there a fortnight. Cameron, Murphy, and Dillon together there. Latter very ill, blind, and mind affected. Committed suicide at Kasakera; buried there. Here Livingstone's remains were put in another bark case, smaller, done up as a bale to deceive natives who objected to the passage of the corpse, which was thus carried to Zanzibar. Livingstone's clothing, papers, and instruments accompany the body. When ill Livingstone prayed much. At Muilala he said, 'I am going home.' Chumah remains at Zanzibar. Webb, American consul at Zanzibar, is on his way home, and has letters handed to him by Murphy from Livingstone for Stanley, which he will deliver personally only. Geographical news follows. After Stanley's departure the Doctor left Unyanyembe, rounded the south end of Lake Tanganyika, and travelled south of Lake Bemba, or Bangeoleo, crossed it south to north, then along east side, returning north through marshes to Muilala. All papers sealed. Address Secretary of State, in charge of Arthur Laing, a British merchant from Zanzibar. Murphy and Cameron remain behind."

These details are few but intensely touching. We believe that the Peninsular and Oriental Company's Bombay steamer *Malwa*, with Dr. Livingstone's body on board, is due on April 13 at Southampton. The body will be landed at that port and conveyed to London, by railway, for interment in Westminster Abbey; it is to be regretted that the faithful Chumah does not accompany his master's remains. It is impossible that Government will fail in doing what the whole civilised world takes for granted it will do—pay all possible honour to the remains of H. M. Consul, and of probably the greatest traveller that this or any other country ever produced.

REPORT OF PROF. PARKER'S HUNTERIAN LECTURES "ON THE STRUCTURE AND DEVELOPMENT OF THE VERTEBRATE SKULL"

THE new Hunterian Professor, Mr. W. Kitchen Parker, has just completed his course of eighteen lectures at the College of Surgeons, embodying in them the results of his researches on that most difficult problem, the deve-