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. ST. GEORGE MIVART

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 ntil 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

* "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.

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 indraught 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 *vertica 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 isocheimal) 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