them, by attracting in like manner also the attention of insects, which, visiting the flowers for their own profit, at the same time unconsciously bring to the plant the great advantage of cross-fertilisation. Hence we understand that bright-flowered varieties, whenever produced by any cause, might be preserved by natural selection, and at last remain the only survivors among all the concurrents of the same species. Thus, the occasional appearance of gaily-coloured varieties granted as a matter of fact, and the peculiarities of colour supposed to be hereditable, we are enabled by Darwin's theory to explain the variety of colours met with in flowers. But we should always bear in mind that we are at present quite ignorant of the chemical processes by which certain colours are produced in the flowers, and of the physical or organic causes by which these chemical processes were effected when they first appeared and are effected in every subsequent generation. Reflecting on the first origin of the adaptation of flowers to the cross-fertilisation by insects, and considering that the oldest and most primitive phanerogamous plants which still exist, the Gymnospermæ, are exclusively fertilised by the wind (are *anemophilous*), whilst the enormous majority of Angiospermæ is provided with flowers adapted to cross-fertilisation by insects (*entomophilous*), we cannot doubt that the original manner of fertilisation of phanerogamous plants was fertilisation by the wind, and that the first plants which adapted their flowers to cross-fertilisation by insects were anemophilous ones, either Gymnospermæ or the next descendants of them. Nevertheless the flowers of many Gymnospermæ (Abietinæ) present a beautiful colour, which attains its culmination during the disseminating of the pollen.* This beautiful colour is apparently neither of any use to these plants, which are regularly cross-fertilised by the wind, nor can have been inherited from ancestors to which it was useful. We may therefore also in this case, without hesitation, regard the colour as a merely accidental phenomenon, which, secondarily produced by the more active chemical processes during the time of flowering, disappears again in the same degree as the intensity of development decreases in the cones. Probably the gaily-coloured perianths of the entomophilous Angiospermæ have originated in a similar manner.

Independently of possible physical effects, natural selection is evidently without any influence as to colours, unless animals are attracted or repelled by them. Consequently not only the first origin of bright-coloured flowers, but also the change of colour in the flowers after the ovaries are set, is altogether foreign to the effects of natural selection. It is as indifferent to an entomophilous plant whether its flowers, after having been fertilised, grow paler or darker, as it is to an anemophilous plant whether its flowers are attractive to insects or not. In most cases, indeed, flowers change while fading into paler and less conspicuous colours, but often also their colour remains unaltered or even grows more conspicuous. Old flowers of *Melampyrum pratense*, for instance, which, not having been cross-fertilised by insects, regularly fertilise themselves, are always reddish-yellow, whilst younger ones are yellow.

As to *Fumaria capreolata*, alluded to in Mr. Moggridge's letter (NATURE, vol. ix. p. 423) I have never had the opportunity of observing its flowers myself, but from Hildebrand's account ("Jahrb. f. wissensch. Bot." vii. p. 452) I believe that it is restricted to regular self-fertilisation, cross-fertilisation by insects not, indeed, being impossible, but taking place very exceptionally; for it has lost, probably from permanent disuse, the elasticity of the cap formed by the inner petals, which in other fumitories secures cross-fertilisation in case of the repeated visits of insects. If this presumption of mine be right, it would the more explain Mr. Moggridge's observation; for in this case the colour of the flowers of this fumitory, inherited from ancestors to which it was quite useful, would be almost useless to this degenerated descendant, and therefore almost withdrawn from the influence of natural selection. HERMANN MÜLLER

Lippstadt, April 4

* See Strassburger's memoir in "Yenaische Zeitschrift," vi. band, 2 heft, pp. 249-261.

Conference for Maritime Meteorology

SOME of your readers may have noticed in the Report of the Proceedings of the Meteorological Congress at Vienna that it was decided to be advisable to convene a fresh Conference for maritime meteorology, in order to reconsider the decisions of the Brussels Conference in 1853.

The matter was handed over to the Permanent Committee, and by them delegated to a sub-committee composed of the following members:—

Prof. Buys Ballot (Holland)
Prof. Mohn (Norway)
Capt. E. Mouchez (France)
Dr. G. Neumayer (Germany)

with myself.

The sub-committee have nearly decided on a form of programme for the proceedings, and there are hopes that the Conference will meet in London in the month of August or so. Endeavours will probably be made to induce H.M.'s Government to issue the invitations, and thereby to give an official character to the Conference. ROBERT H. SCOTT

Herbert Spencer and *à priori* Truths

ABSENCE from town has delayed what further remarks I have to make respecting the disputed origin of physical axioms.

The particular physical axiom in connection with which the general question was raised, was the Second Law of Motion. It stands in the *Principia* as follows:—

"*The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.*

"If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively. And this motion (being always directed the same way with the generating force), if the body moved before, is added to or subducted from the former motion, according as they directly conspire with or are directly contrary to each other; or obliquely joined, when they are oblique, so as to produce a new motion compounded from the determination of both."

As this, like each of the other laws of motion, is called an axiom;* as the paragraph appended to it is simply an amplification, or re-statement in a more concrete form; as there are no facts named as bases of induction, nor any justifying experiment; and as Newton proceeds forthwith to draw deductions, it was a legitimate inference that he regarded this truth as *à priori*. My statement to this effect was based on the contents of the *Principia* itself; and I think I was warranted in assuming that the nature of the laws of motion, as conceived by Newton, was to be thence inferred.

The passages quoted by the *British Quarterly Reviewer* from Newton's correspondence, which were unknown to me, show that this was not Newton's conception of them. Thus far, then, my opponent has the best of the argument. Several qualifying considerations have to be set down, however.

(1) Clearly, the statements contained in the *Principia* do not convey Newton's conception; otherwise there would have been no need for his explanations. The passages quoted prove that he wished to exclude these cardinal truths from the class of hypotheses, which he said he did not make; and to do this he had to define them.

(2) By calling them axioms, and by yet describing them as principles "*deduced* from phenomena," he makes it manifest that he gives the word axiom a sense widely unlike the sense in which it is usually accepted.

(3) Further, the quotations fail to warrant the statement that the laws of motion are proved true by the truth of the *Principia*. For if the fulfilment of astronomical predictions made in pursuance of the *Principia* is held to be the evidence "on which they chiefly rest to this day," then, until thus justified, they are unquestionably hypotheses. Yet Newton says they are not hypotheses.

Newton's view may be found without seeking for it in his letters: it is contained in the *Principia* itself. The scholium to Corollary VI. begins thus:—

"Hitherto I have laid down such principles as have been received by mathematicians, and are *confirmed* by abundance of experiments. By the two first Laws and the two first Corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola; experience *agreeing* with both," &c.

Now as this passage precedes the deductions constituting the *Principia*, it shows conclusively, in the first place, that Newton did not think "the whole of the *Principia* was the proof" of the

* It is true that in Newton's time, "axiom" had not the same rigorously defined meaning as now; but it suffices for my argument that, standing unproved as a basis for physical deductions, it bears just the same relation to them that a mathematical axiom does to mathematical deductions.

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