

ment is very detailed for an elementary book, but there is nothing beyond the capacity of those for whom it is intended. The author is of opinion—and we quite agree with him—that meagre accounts lead to inaccurate ideas, inasmuch as they are not of sufficiently general application. As far as desirable, and in accordance with the syllabus, simple experiments have been introduced. The main results of the *Challenger* Expedition are also explained, and illustrated by diagrams.

The astronomical portion leaves nothing to be desired. In addition to 150 excellent diagrams, there are ten maps, illustrating the distribution of temperature and pressure, volcanoes and earthquakes, &c. The diagram of the geological formations shows the general physical appearance of the strata, along with the characteristic fossils of each.

The book is beautifully printed, and is sure to win the favour of all who use it, whether as students or teachers.

#### LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

##### Dr. Whewell on the Origin of Species.

IN his essay on the "Reception of the 'Origin of Species,'" Prof. Huxley writes:—

"It is interesting to observe that the possibility of a fifth alternative, in addition to the four he has stated, has not dawned upon Dr. Whewell's mind" ("Life and Letters of Charles Darwin," vol. ii, p. 195).

And again, in the article "Science," supplied to "The Reign of Queen Victoria," he says:—

"Whewell had not the slightest suspicion of Darwin's main theorem, even as a logical possibility" (p. 365).

Now, although it is true that no indication of such a "logical possibility" is to be met with in the "History of the Inductive Sciences," there are several passages in the Bridgewater Treatise which show a glimmering idea of such a possibility. Of these the following are, perhaps, worth quoting. Speaking of the adaptation of the period of flowering to the length of a year, he says:—

"Now, such an adjustment must surely be accepted as a proof of design, exercised in the formation of the world. Why should the solar year be so long and no longer? or, this being such a length, why should the vegetable cycle be exactly of the same length? Can this be chance? . . . And, if not by chance, how otherwise could such a coincidence occur than by an intentional adjustment of these two things to one another; by a selection of such an organization in plants as would fit them to the earth on which they were to grow; by an adaptation of construction to conditions; of the scale of construction to the scale of conditions? It cannot be accepted as an explanation of this fact in the economy of plants, that it is necessary to their existence; that no plants could possibly have subsisted, and come down to us, except those which were thus suited to their place on the earth. This is true; but it does not at all remove the necessity of recurring to design as the origin of the construction by which the existence and continuance of plants is made possible. A watch could not go unless there were the most exact adjustment in the forms and positions of its wheels; yet no one would accept it as an explanation of the origin of such forms and positions, that the watch would not go if these were other than they were. If the objector were to suppose that plants were originally fitted to years of various lengths, and that such only have survived to the present time as had a cycle of a length equal to our present year, or one which could be accommodated to it, we should reply that the assumption is too gratuitous and extravagant to require much consideration."

Again, with regard to "the diurnal period," he adds:—

"Any supposition that the astronomical cycle has occasioned the physiological one, that the structure of plants has been brought to be what it is by the action of external causes, or that

such plants as could not accommodate themselves to the existing day have perished, would be not only an arbitrary and baseless assumption, but, moreover, useless for the purposes of explanation which it professes, as we have noticed of a similar supposition with respect to the annual cycle."

Of course, these passages in no way make against Mr. Huxley's allusions to Dr. Whewell's writings in proof that, until the publication of the "Origin of Species," the "main theorem" of this work had not dawned on any other mind, save that of Mr. Wallace. But these passages show, even more emphatically than total silence with regard to the principle of survival could have done, the real distance which at that time separated the minds of thinking men from all that was wrapped up in this principle. For they show that Dr. Whewell, even after he had obtained a glimpse of the principle "as a logical possibility," only saw in it an "arbitrary and baseless assumption." Moreover, the passages show a remarkable juxtaposition of the very terms in which the theory of natural selection was afterwards formulated. Indeed, if we strike out the one word "intentional" (which conveys the preconceived idea of the writer, and thus prevented him from doing justice to any naturalistic view), all the following parts of the above quotations might be supposed to have been written by any Darwinian. "If not by chance, how otherwise could such a coincidence occur, than by an adjustment of these two things to one another; by a selection of such an organization in plants as would fit them to the earth on which they were to grow; by an adaptation of construction to conditions; of the scale of construction to the scale of conditions?" Yet he immediately goes on to say: "If the objector were to suppose that plants were originally fitted to years of various lengths, and that such only have survived to the present time . . . as could be accommodated to it (i.e. the actual cycle), we should reply that the assumption is too gratuitous and extravagant to require much consideration." Was there ever a more curious exhibition of failure to perceive the importance of a "logical possibility"? and this at the very time when another mind was bestowing twenty years of labour on its "consideration." GEORGE J. ROMANES.

##### The Fog Bow.

THE complete theory of the rainbow, as developed by Sir George Airy (Camb. Phil. Trans., vi, p. 379, 1836), besides explaining the supernumerary bows, shows that the main bow has a radius somewhat smaller than that calculated on the ordinary geometrical theory. The smaller the drops the greater is the discrepancy. With the tiny drops composing a fog, the discrepancy is so marked that the bow receives a new name—the fog-bow, or "*arc-en-ciel blanc*." Mr. Mohn's (NATURE, February 23, p. 391) nearly simultaneous measurements of the fog-bow and Ulloa's rings afforded a capital opportunity of putting the theory to the test, for from the latter phenomenon we can readily calculate the average size of the particles.

Not having Airy's paper within reach, I have had to be content with the incomplete account given by Verdet ("*Leçons d'Optique Physique*," tom. i. p. 414). Assuming  $\mu = 1.333$ , I find for the angular discrepancy—

$$\beta = 0.467 m \left( \frac{\lambda}{a} \right)^{\frac{3}{2}},$$

where  $\lambda$  is the wave-length,  $a$  the radius of the drop, and  $m$  is determined by the condition that the integral—

$$\int_0^{\infty} \cos \frac{\pi}{2} (\omega^3 - m\omega) d\omega$$

should be a maximum. This integral was calculated by Airy for a series of values of  $m$ , but Verdet does not quote the results. Some rough approximations lead me to the conclusion that  $m$  lies between 1.0 and 1.3, and very much nearer the latter.

For the radius of the first Ulloa's ring we have

$$a = 0.82\lambda/a.$$

Mr. Mohn measured this radius as  $1^\circ 31'$ . Using this value, and taking  $m$  as 1.25, I find  $\beta$  is the circular measure of  $3^\circ 24'$ . The geometrical theory gives the radius of the rainbow  $42^\circ 2'$ . So in this particular case the fog bow should have had the radius  $38^\circ 38'$ . Mr. Mohn gives two measurements, taken