

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—

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

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

$$\int_0^\infty \cos \frac{\pi}{2} (v^3 - mv) dv$$

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

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

Mr. Mohn measured this radius as $1^\circ 31'$. Using this value, and taking m as 1.25, I find β 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

shortly before that of the Ulloa's ring, $38^{\circ} 48' \pm 48'$, and $38^{\circ} 28' \pm 22'$. Thus the agreement between theory and observation is singularly perfect.

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"The Teaching of Elementary Chemistry."

IN reply to Prof. M. M. P. Muir's letter, I wish to say that, judging from his answer, Prof. Muir does not seem to consider it necessary in books of which he is senior author to secure that accuracy of which, from his criticisms of the writings of others, one would expect to find him the champion.

The first extract from the books mentioned sounds curiously to chemists. I consider the statement misleading inasmuch as it appears to convey an idea as to the constitution of caustic soda which is not that generally entertained by chemists; that this is not the intention of the authors, however, is manifest from p. 247 of the "Elementary Chemistry," where the usual view is stated.

It is utterly untrue and misleading to state that, "inasmuch as the result of passing chlorine over yellow mercuric oxide dried at about 100° is to evolve oxygen without forming chlorine monoxide, . . . it may still be justly said that in making chlorine monoxide 'we carry out a reaction in which oxygen is produced in presence of chlorine.'"

The facts are briefly these:—

(a) When chlorine gas is passed at ordinary temperature over yellow mercuric oxide, which has been previously heated to 300° – 400° , chlorine monoxide is obtained.

(b) When a large quantity of chlorine gas at ordinary temperature comes rapidly into contact with yellow mercuric oxide which has been previously dried at ordinary temperature, a violent reaction, accompanied with evolution of light and heat, ensues, and nearly pure oxygen is the only gaseous product. If both the chlorine and the mercuric oxide be kept cool by means of a freezing mixture, chlorine monoxide is the only gaseous product obtained. With intermediate conditions of temperature, &c., mixtures in varying proportions of oxygen and chlorine monoxide are obtained. (Pelouze, *Annalen der Chem. und Pharm.* Bd. xlv. 196.)

The formation of oxygen in the second case must therefore be due to the decomposition of already formed chlorine monoxide, or to the occurrence of a reaction the conditions of which render the existence of part of the chlorine monoxide impossible. I think the majority of chemists will agree with me that the appearance of oxygen under conditions which insure the non-existence of (or as itself a product of the decomposition of) chlorine monoxide, can scarcely be admitted as in any measure explaining the formation of the latter.

I do not consider it a "verbal quibble" to object to the use of the term "volatilized" as applied to the mechanical removal of particles of a solid substance.

As to the chemical properties of chlorine, bromine, and iodine, I should indeed be open to the gravest charges of non-acquaintance with chemical classification, had I suggested anything so idiotic as that, say, potassium hypobromite and potassium hypoiodite (if the latter exists) could be *identical*.

I called the passages I quoted misleading, because some of them at least were inaccurate. What amount of inaccuracy is required to make a statement misleading may be a matter for difference of opinion. Apparently it is so.

Prof. Muir states that he will decline to take any notice of my anonymous communications. This, at least, is safe ground; but I can wait for the second editions of the two books, and see if the inaccuracies are eliminated. In the second edition of "Elementary Chemistry" I hope Messrs. Muir and Slater will also describe the methods (omitted on p. 19) for removing air from oxygen. Whilst these methods remain unpublished, I prefer to remain

Z.

"Kinematics and Dynamics."

MAY I ask a short space in your columns to refer to a few points in Prof. Greenhill's review of my book on "Kinematics and Dynamics," published in your issue of February 16 (p. 361). I shall be as brief as possible.

(1) "In questions involving the size of the earth (pp. 74 and 80), it is the circumference and not the diameter which should be given in metres, the circumference being 40,000,000 metres," the reason being, I suppose, that in illustrative problems round

numbers should be employed as data, with the object of facilitating arithmetical calculation. There are doubtless advantages in this course, and in many problems I have adopted it. But should it be made an invariable rule? Problems based on exact data, such as the ones referred to, on pp. 74 and 80, have for many students a greater interest than those based on approximations.

(2) "The expression 'knots an hour' (p. 60) is irritating to a sailor." But the expression "knots" simply would be either misleading or puzzling to a student unacquainted with nautical abbreviations.

(3) "The formula $\frac{1}{2}v^2 = \frac{1}{2}v_0^2 + as$ is to be preferred to that on p. 34, $v^2 = v_0^2 + 2as$; in all cases the factor $\frac{1}{2}$ should go with the v^2 in the equation of energy." The formula quoted is not an equation of energy, but a kinematical equation. Equations of energy (see pp. 253, 256, 328) have in all cases the form approved by Prof. Greenhill.

(4) "In dealing with rotation, the author would do well to study Maxwell's geometrical representation of the direction by means of the screw, right-handed or left-handed." I have done so; but I find that students more readily grasp a specification of the direction of a rotation when it is made by reference to the face of a clock; probably because few of them are so familiar with right-handed and left-handed screws as they are with clock-faces.

(5) "In a linear strain the increment of distance of two points in the line of the strain is properly their *elongation*; while the ratio of the elongation to the original distance is called the *extension*, not the *elongation*, as on p. 167." And yet Thomson and Tait ("Elements of Natural Philosophy," § 139), Clifford ("Elements of Dynamic," p. 158), Minchin ("Uniplanar Kinematics of Solids and Fluids," § 78), and Ibbetson ("Mathematical Theory of Elasticity," § 53), all define elongation exactly as I have done.

(6) "The author, disregarding the vernacular use of the word 'weight,' defines the weight of a body as the force with which it is attracted by the earth" [I don't (see § 290); but let that pass], "but is at variance with his own definition in the statement of the majority of the subsequent examples, relapsing into the language of ordinary life." No references are given to these instances of backsliding. I have looked pretty carefully through the subsequent examples, and can find no case in which I have used the term referred to in any other sense than that given it by definition. I should be glad to have such slips pointed out to me, if there are any.

(7) "A collection of 500 different ways of spelling the name of the town of Birmingham has been made, and a similar collection could be made from the present treatise of different ways of expressing the simple ideas of the pound *weight* and the pound *force*." It is true that these ideas are expressed by English writers in various ways. And it seems to me desirable that a student should be made acquainted with them. Surely in holding that I should choose one phrase and stick to it, your reviewer is blaming me for not being one of the "mathematical precisionists" at whom he sneers.

(8) "This terminology culminates in the solecisms that on p. 477 we must suppose pressure to be measured in poundals on the square foot in hydrostatical problems; and that if the equation $w = mg$ is supposed to be used with absolute units, the weight of a body is measured in poundals; as if a mathematician asked in a shop for 'half a poundal of tea, or tobacco.'" It is not quite correct to say that, in the hydrostatical equations referred to, pressure must be supposed to be measured in poundals per square foot. In fact it may be supposed to be measured in terms of the unit of pressure of any derived system, as, e.g., the dyne per square centimetre, or even the pound-weight per square foot, provided only the density be measured in terms of the corresponding unit. I am aware that this mode of expressing hydrostatical equations is unusual, but it seems to me to have great advantages, and it was adopted both for this reason and for the sake of making the section on hydrostatics uniform with the rest of the book. With regard to the units in which weight should be measured, the practice of the tobacconist or the tea merchant is surely not our best guide.

(9) "Thus a mathematical precisionist, to express the simple idea of a force of 10 pounds, to be consistent should call it 'a force equal to the weight of the mass of 10 pound weights,' the absurdity of which is evident." The phrase inclosed in quotation marks is not quoted from my book. In my terminology the most precise of mathematicians would express the idea referred to