OUR BOOK SHELF

Deschanel's Natural Philosophy. Edited by Prof. J. D. Everett. Sixth edition. (Blackie and Son, 1882.)

PROF. EVERETT'S admirable adaptation of "Deschanel's Natural Philosophy" is so well known as a text-book, that it needs no commendation from us. We heartily welcome this sixth and greatly improved edition. Amongst the new items we notice that the chapter on thermodynamics has been amplified and re-written; and other parts of the book devoted to heat have also been improved, particularly those relating to the apparent minimum density of water, and to conduction of heat. We notice also a useful note on the mathematical treatment of the periodical variations of underground temperatures. The section dealing with electricity and magnetism has also been greatly improved. The elements of electric testing by Wheatstone's bridge and resistance coils are now included. The modern dynamo-electric machines and such recent inventions as the electric pen and the induction-balance are described. Rowland's experiments on electric convection-currents, and Planté's secondary battery are also mentioned; though it appears to us that by a slight slip of the pen in the paragraph dealing with Planté's researches his "rheostatic machine," which is in reality a compound condenser of mica plates, is described as a species of commutator (like that of Müller) for his secondary batteries. There is another slip in the paragraph on the use of the galvanometer for measuring transient currents, for it is stated that the quantity discharged through the galvanometer is proportional to the swing of the needle, whereas by the well-known ballistic formula of Maxwell, it is proportional to the sine of half the angle of the first swing. These are however minor points. In the section on Light and Sound little has been changed; the more recent measurements of the velocity of light, and the phonograph, being the most important additions. It is a pity that in the optical formulæ the editor does not use the same notation as in the accepted Cambridge text-books. The problems, which in former editions were lumped together at the end of the book, are in this new edition placed at the ends of the separate volumes, a change which is a great boon to teachers and students who find it most convenient to buy the separate parts. Why the date of 1882 should be put upon a work which appears in October, 1881, is one of the mysteries of publishing which lies beyond the pale of scientific criticism.

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. No notice is taken of anonymous communications.]
- [The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Struggle of Parts in the Organism

As the Duke of Argyll does not appear to have quite understood the meaning which I intended to convey in the paragraph of my review to which he refers, I should like to state that meaning a little more explicitly. This I should have done in the first instance had I not shared the feeling which he expresses, that "a purely scientific journal" is not an appropriate place in which to discuss the relations of science to theology, and I shall now hope to show that in my review I did not transgress the border of any such debatable ground.

My remarks were limited to the "Argument from Design as elaborated by the natural theologians of the past generation," the material of which was furnished by "the endless number and complex variety of those apparently purposive adaptations of structures to functions which are everywhere to be met with in organic nature." By this limitation I intended every one

conversant with the writings of these theologians to understand that I alluded only to the Argument from Design as this was expounded by the school of Paley, Bell, and Chalmers, and which amounted to inferring that particular instances of adaptation were so many separate pieces of evidence pointing to as many "operations of special design." This is the form of teleology which I conceive Mr. Darwin's writings to have completely "subverted," for these writings have shown that in natural selection we have a general law whose operation is presumably competent to produce most of the adaptations previously ascribed to special design. This form of teleology is what I called in my review "scientific teleology," and I did so because it embodied what is, in the full sense of the term, a scientific theory; certain definite facts or results were observed, and of these results the immediate cause was inferred. Therefore this endeavour to explain the causation of special mechanisms in organic nature properly admits of being discussed in the pages of a scientific periodical; it is as purely a scientific hypothesis as is that of natural selection.

But the Duke of Argyll clearly attaches to the term "design" a much wider signification than that which I expressly and intentionally assigned to it. For he uses the term in its most unlimited sense, and says: "There are many minds, including some of the most distinguished in science, who not only fail to see any contradiction between evolution and design, but who hold that the doctrine of evolution and the facts on which it is founded have supplied richer illustrations than were ever before accessible of the operation of design in nature," &c., &c.

Here and elsewhere the Duke clearly alludes to the whole question of Theism, or of Mind as the First Cause, and not to the narrower one of this or that particular mechanism in nature as the result of immediate and special design. Now teleology in this larger sense, or the doctrine that behind all the facts open to scientific inquiry (special mechanisms, physical causes, and general laws) there is "Mind and Will" as the ultimate cause of all things—teleology in this sense is a general theory of things which it does not fall within the scope of scientific method to examine. In contradistinction to the cruder teleology of Paley, which, as I have said, may properly be called "scientific," this may be called "metaphysical"—if we use these terms as they are used by Lewes to denote respectively a theory that is verifiable (or the reverse) and a theory that is not. The school of Paley thought that the existence of a designing Mind in nature could be proved by a purely inductive method; Mr. Darwin has since shown that such is not the case; therefore this system of teleology is a scientific system, and, like many other theories of the scientific class, it has had to yield to fuller knowledge. But there remains the metaphysical theory of an ultimate design pervading all nature and blending into one harmonious cosmos what the Duke calls the "combination and co-ordination of physical causes"; and this theory, I quite agree with him, "no possible amount of discovery concerning the physical causes of natural phenomena can affect," either by way of proof or of disproof. But this has nothing to do with the special question between Darwinism and "the argument from design as elaborated by the natural theologians of the past generation"; and therefore I shall not discuss the merits of the theory in these columns.

GEORGE J. ROMANES

"The Micrococcus of Tubercle"

AN article on "Disease Germs," by Dr. W. B. Carpenter, in the current number of the *Nineteenth Century*, contains the following:—"Another line of inquiry which has obviously the most important bearing upon human welfare is the propagability of the micrococcus of tubercle by the milk of cows affected with tuberculosis, a question in regard to which some very striking facts have been brought before the Medical Congress by a promising young pathologist"—naming myself; and I hope that I am sufficiently grateful to a veteran in science for his complimentary if not altogether accurate reference to my work. What I did say at the recent Medical Congress, and at much greater length in a small volume entitled "Bovine Tuberculosis in Man" (London, 1881)—Dr. Carpenter will find it, I think, among his books—was not anything about "the micrococcus of tubercle," but about a variety of somewhat technical morphological details in respect to which certain cases of tuberculosis in man resembled the tuberculosis or "pearl disease" of the bovine species. I did indeed introduce half a page at the end of my essay to show how clear

was the issue between my view of tuberculosis communicated from the cow and the view which Dr. Carpenter has been expounding, and I hope you will have room for the passage:—"The doctrine of a tuberculous virus was stated by Klebs in 1868, and has been advocated by him, as well as by Cohnheim, in recent writings. In its latest form this doctrine asserts the existence of a specific minute organism to whose agency the infection is due. The minute organism is called by Klebs *Monas tuberculosis*. The method of proof which I have followed in this work makes it impossible that the infective agency of a minute organism should in any way come into my view of the communication of bovine tuberculosis to man. I have rested the whole case upon certain minute identities of form and structure in the infected body, due to the mimicry of infection. Among other points there were the leaf-like and cord-like outgrowths of the pleura and peritoneum, these being the early stages of the lentil-like or pearl-like nodules and their connecting threads; the lymphatic glands, with distinct nodular formations in their substance; the lungs, with smooth-walled closed vomicae or with encapsuled nodules. In the new formations generally there was a particular pattern of microscopic structure, in which giant-cells and epithelial-like cells figure largely, and there was a relatively high degree of vascularity. In all these points the disease in man is a mimicry of the parent disease in the bovine animal. That mimicry is not only in single features, but it is of the whole disease. It is possible to conceive of the juices and particles of the primarily diseased body acquiring a kind of spermatric virtue which gave them the power to communicate the specific disease as a whole and in all its several manifestations to another body in which they should happen to lodge. But it is hardly possible to think of a neutral living organism being charged with the power of conveying so complex details of form and structure from one body to another" ("Bovine Tuberculosis in Man," pp. 103, 4).

25, Savile Row, W., October 24

C. CREIGHTON

A Kinematical Theorem

PROF. MINCHIN'S Theorem in NATURE (vol. xxiv. p. 557) may be proved easily by considering the motion as due to the rolling of one closed curve on another back into its first position, their lengths being of course commensurable. If you measure y for the rolling curve from the straight line which forms the envelope, and x along that line, then the differential of the area between the envelope and the fixed curve is easily seen to be $ydx + \frac{1}{2}y^2d\omega$, where $d\omega$ is the angle turned through by the rolling curve, and is equal to ds multiplied by the sum of the curvatures at the point of contact, which we shall call σ . The summation of the former part is a multiple of the area of the rolling curve, and therefore the same for all lines; that of the latter is half the moment of inertia of matter distributed over its perimeter with density σ , about the line in question. The result is therefore the well-known property of equi-momental ellipses. Similar reasoning, with the use of the property of the centre of inertia of a system, leads to the further result that when the perimeter of the envelope is of constant length, the line touches a circle, and different values of the constant correspond to concentric circles. In the same way by a property of the centre of inertia we may also prove immediately the known theorem that when the area traced out by a point is constant, the point lies on a circle, and different values of the constant correspond to concentric circles; and we may extend it to areas traced on a sphere.

JOSEPH LARMOR

54, Antrim Road, Belfast

IF Prof. Minchin will refer back to the *Bulletin des Sciences Mathématiques et Astronomiques* for August, 1878, he will, I think, find in a paper by M. Darboux the theorem stated by him under the above title in NATURE, vol. xxiv. p. 557.

C. LEUDESORF

Pembroke College, Oxford, October 21

"The Dark Day"

REFERRING to the account of the phenomenon in New England on September 6 last (NATURE, vol. xxiv. p. 540), and in Mr. Harding's letter (p. 557), let me refer your readers to a succinct account of the occurrence on May 19, 1780, which they will find in Webster's Dictionary, "Explanatory and

Pronouncing Vocabulary of the Noted Names of Fiction, &c." In *Public Opinion* (June 4, 1881) there is an account of a precisely similar occurrence on the morning of Sunday, November 8, 1819, known, it is remarked, as the "Phenomenon of 1819." The account of this phenomenon is very explicit, and the details furnished correspond so closely with the event of May 19, 1780, that a doubt might be felt whether there had been two such days, or whether there had not been some mistake made in regard to the date given. I wrote to *Public Opinion*, making inquiries (see *Public Opinion* of June 11, 1881, p. 755), but no reply has hitherto appeared to my inquiries. I may observe that the year 1819 would not coincide with any one of the sun-spot cycle of eleven years from 1780 to which the *New York Nation* refers.

A. TREVOR CRISPIN

6, Melbury Terrace, Harewood Square, N.W., October 22

OWENS COLLEGE SCIENCE AND LITERATURE FELLOWSHIPS

THE first award of these Fellowships, of the annual value of 100*l.* each, which are intended to encourage original investigation, was made on Friday last by the Council of the Owens College. These are remarkable as being the only fellowships given in any University or College in the United Kingdom solely for the encouragement of research. They are not awarded on the results of examination, but after consideration of documentary or other evidence. Every holder of a fellowship is expected to devote his time to the prosecution of some special study, and before the close of the year to give evidence of progress by the preparation of a thesis, the delivery of a lecture, or the completion of some research. He may also be called upon to render some service to the College either by acting as occasional examiner or by giving instruction by lectures or otherwise to the students.

Of the thirty candidates four gentlemen were elected to Fellowships. Of these one is awarded to Mr. Alfred Sidgwick, B.A. of Lincoln College, Oxford, in the Department of Logic; two were awarded in the Department of Chemistry, namely, one to Dr. Bohoslav Brauner, of the University of Prague, who has already published several papers on original subjects, some from the laboratory of the Owens College; and a second to Mr. Harry Baker, Dalton Chemical Scholar of the College, who has likewise published several papers in the *Journal* of the Chemical Society. These two gentlemen will continue their researches, devoting the whole of their time to original investigation. In the Department of Biology an award has been made to Mr. H. Marshall Ward, B.A., F.L.S., of Christ College, Cambridge, at one time a demonstrator in the Owens College, who has recently distinguished himself as Government cryptogamist in Ceylon, in an investigation of the cause of the coffee disease.

THE AGE OF THE IGNEOUS ROCKS OF ICELAND

DURING a recent visit to the south-west part of Iceland, one or two points connected with the general geological structure of the island came under my observation, which I do not remember to have seen noticed before, and which seem to me to be of sufficient interest to be put on record. It is well known that the rocks of the island are of very different ages, some going back to the Miocene period, while others are quite of yesterday's date. It is also perhaps a general belief that the volcanic forces may have continued to be more or less active from the time that the older Miocene basalts and tuffs were erupted down to our own day. I doubt very much whether there is any evidence to justify this conclusion, and will presently mention some of the facts which lead to a very strong suspicion that a prolonged period of repose supervened after the accumulation of the Miocene rocks, and before the eruption of the later lavas, &c., had begun. The Miocene group consists of a vast