of these and other counties in Scotland is consequently thrown away. The railways and telegraphs of South Africa are far behind the age, the Orange Free State being shown as if still undisturbed by the engine's whistle. The cable (p. 5) to the Cape by the west coast is only partly shown, though correctly given in the German original; and the Bermudas are now in electrical touch with the world, though this does not appear on the map. There are surprisingly few errors of spelling, and for the general purposes of the newspaper reader the atlas is eminently serviceable.

Only a few physical maps are given, including two pages of familiar and rather feeble astronomical diagrams, the use of which must be very slight. The world in hemispheres is shown with an attempt at orographical colouring on the land, and there are bathymetrical charts of the Atlantic and Pacific Oceans. The latter are somewhat remarkable. The contour lines of depth are given as 110, 1100, 2200, 2750, and 3300 fathoms, those odd numbers corresponding of course to even depths in metres, but the occasional soundings dotted over the surface are stated not in fathoms, but in feet. Similarly the one small climate map shows isotherms drawn to represent centigrade degrees, but lettered with the approximate Fahrenheit equivalents. The system of showing the winds on this map is arbitrary, and the arrangement it implies is unnatural. The physical maps might indeed be excised without serious loss.

The plan of printing the hill-shading—usually much generalised—in brown, throws out the black lettering, admirably, while the rivers and railways are very clearly shown in most cases. The importance of clear maps in following any movement or distribution cannot be over-estimated, and the Switzerland (pp. 53, 54), Caucasus (p. 71), and Greece (p. 72), in this atlas are beautiful examples of artistic work, embodying ample detail without confusion.

OUR BOOK SHELF.

Types of Animal Life. By St. George Mivart, F.R.S. (London: Osgood, McIlvaine and Co., 1893.)

IT is reported of a negro preacher that, having omitted to note the source of his text, he counselled his hearers to search diligently for it, assuring them that in doing so they would find many other texts which would be for their souls' good. We are reminded of this anecdote in reading Prof. Mivart's new book on popular natural history. In our search for the types given in the table of contents we have been rewarded by finding a pleasing description of a goodly number of the higher animals. At the beginning of the fourth chapter we are told that the bull-frog has been selected as one type of animal life in order to introduce the group of *Batrachia*. Very little is said, however, about the bull-frog himself, though there is a figure of him in a deprecating attitude suggestive of some appreciation on the part of the artist of the somewhat shabby treatment this Batrachian elect has received. But there is much interesting matter concerning amphibians of all kinds, illustrated by reference to, or short descriptions of, some twenty genera. The first chapter is headed "Monkeys"; and similar headings would, we think, have been more appropriate throughout. As it is, the animal named is, in each case, merely a convenient starting-point for the consideration of the group to which it belongs. We do not know

whether the chapters embodied in the book have already done service in any form in America, but the animals selected suggest that such may have been the case. We have the opossum, the turkey, the bull-frog, the rattlesnake, the Carolina bat, the American bison, the racoon, the sloth, and the sea-lion; while the chapter which deals with, or starts with, the last-named animal begins thus:—"The sea-lion is a beast the sight of which must be familiar to very many Americans." The term "animal life" of the title of the work is shown by the contents to be applied to the dactylate vertebrata only, three-fourths of the volume being devoted to mammals, or beasts as the author prefers to call them.

Unfortunately the proofs have been carelessly corrected, so that misprints (e.g. fleshating beasts, p. 209), errors of fact (e.g. that *Notoryctes* has recently been discovered in America, p. 60), grammatical errors (e.g. only two kinds of elephant now exists, p. 207); a redundant "and" before "which," p. 217), and inelegancies (e.g. python-like headed reptiles, p. 149: the blind worm is popularly reported as being deadly poison, p. 146) have been suffered to remain. Technical terms have, as far as possible, been avoided; but it is questionable whether the use of such a term as "wing-wedge bone" is advisable. Surely those who can take in such names of places as "Eschscholtz Bay," and such local names of animals as "Catamiztli," could swallow "alisphenoid" without serious mental indigestion.

For the rest we have nothing but praise. A great deal of information is conveyed in a pleasant style. The illustrations, if not quite all that could be desired, are decidedly above the average. The reiterated allusion to the possibly independent origin of similar structures (or the independent origin of different structures, as it appears on p. 120) is, in our opinion, not out of place in such a book. Those who enjoy a smattering of knowledge, picked up from popular works, are apt to be so terribly dogmatic that it is well to urge them to keep their minds "free from prejudice and ready to receive all and any truth which may be demonstrable." C. LL. M.

Science Teaching in Schools. By Dr. Henry Dyer. (London: Blackie and Son, 1893.)

AN address given by Dr. Henry Dyer on science teaching in schools has been amplified and is now published in book form. The points dwelt upon appeal particularly to the managers and teachers of existing elementary schools, and of the secondary and technical schools now being organised in all parts of the country. In an appendix (which, by the way, is almost as long as the address itself) are given syllabuses of elementary science as taught under the London and Leicester School Boards, and the curricula of the evening classes of the Glasgow and West of Scotland Technical College. A commendable feature is the insertion of the courses of instruction at the Ecole de Commerce et de Tissage of Lyons and the Public Mercantile Educational Institute of Leipzig.

LETTERS TO THE EDITOR.

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Vectors and Quaternions.

PROF. MACFARLANE claims that his "fundamental rules for vectors are based on physical considerations, the principal one of which is that the square of a vector is essentially positive." His proof is virtually this:—The expression for the kinetic energy ($\frac{1}{2}mv^2$) is an essentially positive quantity. It contains one factor $\frac{1}{2}m$ evidently positive. Hence the other factor v^2 must also be positive. "But v denotes the velocity

which is a directed quantity." Unfortunately for this argument v does not denote the velocity in its complete conception—it simply measures the speed. The physicist may think of velocity as being a vector quantity; but in ordinary analysis the vector is not symbolised. We deal only with tensors and scalars. It would be well, I think, if the strict meaning of vector were clearly borne in mind. A vector is a directed line in space, and may be used to symbolise all physical quantities which can be compounded according to the well-known parallelogram law. Displacement is perhaps the simplest conception that can be so symbolised. Velocities, concurrent forces, couples, &c., are in the same sense vector quantities. Now it can be proved rigorously that quadrantal versors are compounded according to this very addition law. On what grounds, then, are they refused admittance to the order of vectors? If a vector cannot be a versor in product combinations, what is the significance of the equation $ij = k$? Regarding this Dr. Macfarlane vouchsafes no remark, save that it is possible to get along without its use. As he himself has not done so, such a possibility lies altogether outside our consideration. Again, I fail to see what "physical considerations" have to do with mathematics of the fourth dimension.

Dr. Macfarlane says that the "*onus probandi* lies on the minus men." To my mind there is no question of proof at all. That the unit vector a should fulfil the equation $a^2 = +1$ is a bare assertion on the part of Dr. Macfarlane and Mr. Heaviside supported by such words as "natural, simple, conventional," and the like. The equation $a^2 = +1$ is a pure assumption, having no better physical basis than the assumption that $a^2 = -1$. But in quaternions this is not the assumption. The assumption is—as Dr. Macfarlane admits—that products are to be associative. Hamilton, in fact, invented his calculus so as to have its rules differing as little as possible from the recognised rules of algebra. The commutative law had to go, but the others were kept (see *Preface to Lectures*, §§ 50—56). In the system he advocates, Dr. Macfarlane loses the associative principle, and—as I think I show in my paper—gains nothing but a positive sign and an undesirable complexity in transforming by permutations.

As a calculus, quaternions may be developed quite as readily from the conception of the product as from that of the quotient. But in my paper I was arguing against Prof. Gibbs's dictum that the quaternion as a quantity corresponded to nothing fundamental in geometry. The extremely simple geometrical conception of a quaternion as a quotient of two vectors sufficiently meets Dr. Macfarlane's query, "Is not the product always the simpler idea?" It is certain that the quotient of any two like quantities has always a meaning; the product is often meaningless.

In the particular geometrical development of quaternions which I indicate in my paper, it can be shown that the quaternion, originally defined as the quotient of two vectors, can also be represented as the product (Dr. Macfarlane inadvertently misquotes "quotient") of two quadrantal versors, and this quite independent of the truth that quadrantal versors obey the vector law.

Dr. Macfarlane evidently grudges Prof. Tait (properly, Kelvin and Tait) the use of any but quaternion symbolism. Of course, when ∇^2 occurs in ordinary non-quaternion analysis, it is used in the sense of the *tensor*, for only as such can it come in. This surely hardly needed to be pointed out. In quaternions there is no doubt whatever that $\nabla(\nabla\omega) = (\nabla\nabla)\omega = \nabla^2\omega$; and therein, as in all the higher physical applications, the flexibility and power of Hamilton's calculus are at once apparent.

In conclusion, let me say that no reasonable man can possibly object to investigators using any innovations in analysis they may find useful. But in the present case there is a very serious objection to the innovators condemning the system, from which they have one and all drawn inspiration, as "unnatural" and "weak," without in any way showing it so to be. That they can re-cast many quaternion investigations into their own mould does not prove their mould to be superior or even comparable to the original. Yet, in so far as they possess much in common with quaternions, the modified systems used by Gibbs, Heaviside, and Macfarlane cannot fail to have many virtues.

"His form had not yet lost
All her original brightness, nor appeared
Less than Archangel ruined."

Edinburgh University, May 29.

C. G. KNOTT.

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The Fundamental Axioms of Dynamics.

My reasons for holding that the fact that potential energy belongs to a system rather than a particle is hostile to the idea of the identity of energy, are briefly these. If two pieces of kinetic energy, are successively transformed and added to a system as potential energy, and then some of the potential energy is retransformed into kinetic, we cannot say *which* of the original kinetic energies thus makes its reappearance; for while both were potential they had no local habitation within the system, and so could not be distinguished from each other.

The objections to including the ether as one of the "bodies" between which contact actions occur, without further explanations, are admirably stated by Prof. Rücker; but I should like to go even further than he does, and point out that if "contact action" means only "action at constant distance" it has not yet been shown how, by such action, kinetic energy comes to be transferred from one body to another. For if the bodies "move over the same distance," and have at any moment the same velocity, their kinetic energies are both increased or decreased *together*; whereas what we wish to show is how that of the one body may increase while that of the other decreases, and why the increase in the one case is equal to the decrease in the other. For example, it may be that in a perfect fluid such transference of kinetic energy actually takes place; but the question is, has Prof. Lodge explained this as a case of "contact action" or "action at constant distance"? What are the things or "bodies" which in this case are actually in contact, and which move over equal distances while the action is going on? Or between what points is the "constant distance" to be measured? Prof. Lodge has not shown in his last paper or in those in the *Phil. Mag.* how "potential energy" can be explained by contact action, nor how kinetic energy can be transferred by contact action alone. But perhaps the answers to these questions are included in the "something more definite" which Prof. Lodge now realises that he has "to say concerning the functions of the ether as regards stress"?

The third paragraph of Prof. Lodge's letter is evidently a joke. I certainly suppose that the denial of action at a distance means that material particles are without *direct* influence upon one another until they touch; *i.e.* that any influence they do exert is *indirect*, and takes place through their both touching something else. Indeed I indicated this in my last letter; but Prof. Lodge apparently hoped I would overlook his omission of the word "direct," and that so the joke would go against me!

EDWARD T. DIXON.

Trinity College, Cambridge, June 10.

Chemical Change.

IN the current number of the Proceedings of the Chemical Society, Prof. H. E. Armstrong publishes two articles on (1) the conditions determinative of chemical change, and (2) the nature of depolarisers. The former deals mainly with the presence of water as a necessary condition of chemical change, the latter with the question of the solution of metals in acids. For some time past I have been engaged with work on the former subject, upon terms of mutual understanding, with my friend Mr. H. B. Baker, whose experiments, following upon those of Prof. H. B. Dixon, have revolutionised our conceptions of chemical change. In the last four years I have also carried on investigations upon the reactions of metals with acids, especially nitric and sulphuric. I should, therefore, propose to deal more fully in a separate publication with the interesting speculations raised by Prof. Armstrong in the articles quoted above. For it has become apparent that after a century of work in chemical science we have no answer to the questions, (1) What is the nature of chemical change? and (2) What is the cause of its commencement? It is probable that both questions resolve themselves, in the long run, into the first. Of facts there is no end, but no interpretation thereof.

The subject is, therefore, ripe for discussion, not only for chemists among themselves, but also, as Prof. Armstrong aptly remarks, for physicists.

Such a discussion might be brought forward at the Chemical Section of the British Association, at Nottingham, in the current year, or, more appropriately, next year in Oxford, the home of Robert Boyle, Mayow, and other earlier chemists.

V. H. VELEY.

The University Museum, Oxford.