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. Velccities, 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 vouchasfes 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 $\nabla^2 v$ 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

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. NO. 1233, VOL. 48] C. G. KNOTT.

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 *logether*; 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 "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 kir.etic 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 strees"?

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 (I) 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, (I) 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.

The University Museum, Oxford.

V. H. VELEY.