illustrations, the strains of lively music which we are told accompany every movement, and, above all, the repeated assurance that the ladies need do no more than they like, will all tend to persuade parents and daughters that gymnastics are very pleasant and desirable.

On Eozoön Canadense. By Professors King and Rowney. 8vo. (Dublin, 1870.)

THIS reprint from the Proceedings of the Royal Irish Academy, treats of a controverted subject of considerable interest to geologists and zoologists, namely, the nature of certain Canadian and other serpentinous limestones in which Logan, Dawson, Sterry Hunt, Carpenter, Jones, Gümbel, and others believe they find definite traces of a foraminifer known as *Eozoön*. Great difference of opinion on the subject under notice has been expressed during discussions before learned societies and in memoirs written by geologists, some seeing under the microscope good proofs of the presence of foraminiferal structure; and these observers are mainly rhizopodists well acquainted with the peculiar structures of shelled protozoa, others finding nothing but inorganic fibres, globules, flocculi, &c., of mineral matter in both the Canadian and any other similar serpentinous marbles. Among the latter disputants are Doctors King and Rowney; and in the paper before us there are some new descriptions and figures of specimens illustrative of the structure of certain ophitic rocks from different countries, and likely to be of use to "eozoönal" students, enlarging their field of observation, and aiding them, perhaps, in arriving at definite conclusions. The figures, however, are little better than diagrams, and cannot help the student much. The paper is largely composed of criticisms on the researches and remarks of others, in a highly disputatious form, and not enriched with anything new to those who have thoroughly studied the matter, either mineralogically or from a zoological point of view. The following important facts do not appear to be recognised by the authors: first, that ophites, on the one hand, may not be really "eozoönal" and yet have mineral structure resembling in one point or another what occurs in *Eozoön;* secondly, that true eozoönal rock is often so greatly crumpled up in its metamorphic state, that patches only of the organic structure are found here and there amongst the somewhat similar ophitic mass of granules and fibres.

Die Ophthalmologische Physik, und ihre Anwendung auf die Praxis. Von Dr. Hugo Gerold, of Giessen. Part II. (Vienna, 1870. London: Williams and Norgate.)

THE advances in the department of Ophthalmology have of late years been so rapid and important, that either thoroughly-revised editions of the standard works or altogether new books have become a sheer necessity. The volume before us comes under the latter category, and is the work of a gentleman well known as an able physicist. The present part is occupied with the Dioptrics of the Eye; the defects in it that are due to spherical and chromatic aberration; the terminology employed to indicate the different functional relations of the several parts to one another and to light, as æquatorial, median, and sagittal planes, axes, visual lines, field of vision, angle of elevation, &c.; the principles of perspective and of the construction of the microscope, the ophthalmoscopic investigation of the eye, and the adaptation of convex and concave lenses for hypermetropia or myopia, and lastly, a section on light and colour. The parts we have read appear to be clearly and intelligibly given, and with something like French method and order. The mathematical formulæ introduced are not beyond the comprehension of an ordinary well-instructed reader, and the diagrams are numerous (123 in number) and instructive.

H. P.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Prof. Pritchard and Mr. Proctor

IT has been pointed out to me that Prof. Pritchard, engaged as he is in many important avocations, may quite unwittingly have misjudged my treatise on the Plurality of Worlds. I readily (eagerly) admit this, and also that, in this case, I owe the esteemed Savilian professor an apology for suggesting that he has intentionally wronged me.

The matter is now reduced to a simple issue. I have submitted considerations which are sufficient to convince Prof. Pritchard that his critique is not just. If he withdraws his unfavourable comments, as resulting from accidental misconception, I shall be bound to apologise for too hastily charging him with deliberate unfairness. If he will not, I cannot truthfully withdraw my objections. I will not endure to be represented as speaking severely (and by inference unfairly) of men for whom I have (and have expressed) a most sincere and unqualified admiration—of such men, to wit, as the Herschels, Tyndall, Lassell, Balfour Stewart, and Sir W. Thomson.

RICHARD A. PROCTOR

Whence Come Meteorites?

I HAVE read, with great interest, in the number of June 2nd of your journal the article which Mr. N. S. Maskelyne has devoted to the examination of my theory on the Origin of Meteorites. I request permission to offer some observations on the criticisms of that learned mineralogist.

Although Mr. Maskelyne concludes by saying that, in his opinion, I have not attained the end which I had proposed to myself, I will attempt to show that my system has, in fact, perfectly resisted his attacks.

In truth, the views I have been led to take on the subject of meteorites are not by any means a simple fruit of my imagination. I have been led to them by the observations of material facts easy of verification; and it is only in the background, so to speak, that I have brought under consideration different consequences, which may certainly be matter for discussion. Now, in Mr. Maskelyne's argument, he has given the place of honour to these secondary considerations, whilst he has left the real substance of the question completely in the shade. A few lines will suffice to justify my assertion.

The chemical and mineralogical study of the specimens which compose the rich collections of meteorites at the Museum of the Jardin des Plantes has made me acquainted with *polygenic* masses—that is to say, masses formed of angular fragments soldered together, but possessing each one such decidedly separate characters that it is impossible to suppose that they were originally produced in the forms and in the relative positions which they present at the present day. These clastic meteorites had been previously studied; but not, as far as I am aware, from the point of view at which I have placed myself.

From the studies and experiments I have made on this subject results the indubitable fact that the fragments, the union of which constitutes various clastic meteorites are, each one, completely identical with well-known monogenic meteorites. It is thus, that the clastic meteorite of St. Mesmin (May 30, 1866) contains angular fragments rigorously the same in every respect as those which would be produced by breaking up the meteorite of Lucé (Sept. 30, 1768); fragments soldered together by a dark coloured cement exactly similar to the substance which forms the principal mass of the stone of Limerick (Sept. 30, 1813). It is thus also that in the same cement, the meteorite of Canellas (May 14, 1861), contains fragments of a rock impossible to distinguish from that of which the mass of Montrejeau (Dec. 9, 1858) is a specimen.

How is it possible to understand these positive facts without having recourse to the explanation, so evidently true of terrestrial fragments? For fragments of two distinct rocks to be found associated in one clastic mass, it is absolutely necessary that these two rocks should come from a region where they were in connection. Thus, on one hand, the rocks of Lucé and of Limerick were in connection; thus, on the other hand, the rocks of Montrejeau and Limerick were in connection; then, in conclusion, the rocks of Lucé and of Montrejeau were in connection.

By the side of this first assemblage of facts, of which the meaning seems to me not doubtful, I find another of at least equal importance—that of meteoric rocks evidently eruptive.

The meteoric iron recently discovered in the cordillera of Deesa, in Chili, having been submitted by me to a careful analysis, both chemical and mineralogical, appeared to me clearly to be formed from the mixture of two meteoric rocks, known, each of them, by masses of which they are entirely constituted. The one, stony and black, fell at Sétif, Algeria (June 9, 1867); the other, metallic, constitutes the mass of iron found in 1828 at Caille, in the south of France. Besides this, the metallic portion of the iron of Deesa, in which the black angular fragments are encrusted, has manifestly preserved the character assumed by the iron of Caille when it is subjected to fusion, so that the mode of formation of the Chilian mass cannot be considered doubtful. We must believe that on a globe, large enough to have been the seat of considerable pressure, masses of iron from Caille, still melted, were injected into superposed layers of Sétif rock so as to give birth to dykes, identical, except in their mineralogical nature, with those which the crust of the earth everywhere presents to our view.

These two orders of facts, which seem to me indisputable, being admitted, there remains to explain how fragments of polygenic conglomerates, or of dykes, can wander through space, and here only it is that the hypothetic part of my work begins.

From what precedes the meteorites in question are, by definition, planetary fragments. It remains to learn how the rupture of the planet whence they come can have taken place. On this it is evidently impossible to argue with any certainty.

Nevertheless, it appears to me that several considerations may greatly facilitate a choice among the different explanations which present themselves to the mind.

In the first place the *unity of composition* of the solar system, mentioned by Mr. Maskelyne, is evident.

Secondly, it is manifest that in the same system there exists a perfect *unity of geological phenomena.*

Lastly, but this, perhaps, has less weight, it appears to me that we should have recourse to accidental causes to explain natural phenomena only when every other means is forbidden.

This said, I observe that without making any other hypothesis than that of Laplace, we arrive at the conclusion that the stars tend of themselves to become broken. The earth is cracked in all directions; these fissures, designated as *faults*, are known to everyone. Little by little, as they form, they become reunited by the injection of an internal melted cement. But if the supply of this cement failed, the molecular operation which has opened the faults would still continue its action to enlarge them; we observe this in the moon, which, far more advanced in refrigeration, manifests by its fissures a phenomenon hitherto unknown in our earth. Evidently if we suppose to have been formed at the same time as the moon, a much smaller globe, that globe will have arrived actually at a state of cold far more advanced than that of the moon; and the fissures, excessively multiplied, and increased in depth and in width, may have finished by reducing the globe into separate fragments.

We have no positive proofs that such events have really happened, but is it not a very simple hypothesis to admit that meteorites, which bear so evidently the impress of a detritic character, may have had such an origin?

It is very probable that once parted from one another, the fragments are scattered along the orbit, and it is evident that they will tend progressively to approach the central star, so as to finish by falling on its surface under the form of meteorites.

Now, whether these fragments have been sorted or whether they have not, whether this sorting, if it exists, be or be not in accordance with that which the facts of observation have seemed to point out to me; I consider the question as entirely secondary as regards the general theory, and I request permission, in order to keep within the limits of the present discussion, to lay it absolutely aside for the present. I will simply repeat, in concluding this note, already somewhat long, that positive facts alone have served as the basis of my theory, and that the different circumstances on which my opponent has so learnedly insisted, possess for me but a secondary importance.

At the same time, I sincerely congratulate myself in the fact that my work has had the good fortune to fix the attention of a scientific observer so well placed as Mr. Maskelyne for submitting the mineralogical and lithological part of it to a severe verification.

DR. STANISLAS MEUNIER, Aide Naturaliste au Muséum
23, rue de Vaugirard, à Paris

Monographs of M. Michel Chasles

PAR une lettre insérée dans le No. 36 de NATURE, page 190, M. C. Ingleby fait appel aux lecteurs de votre Revue pour obtenir quelques renseignements au sujet de "l'Aperçu historique" de M. Chasles, imprimé à Bruxelles en 1837. Le travail, qui porte pour titre exact: "Aperçu historique sur l'origine et le développement des méthodes en géométrie, particulièrement de celles qui se rapportent a la géométrie moderne," a été publié par l'Académie royale des sciences de Belgique dans le tome xi. de ses "Mémoires couronnés et des savants étrangers" (in 4to.), et il est très-difficile aujourd'hui de s'en procurer des exemplaires. Toutefois, M. Ingleby pourra s'adresser, pour consulter ce mémoire, à la Société royale de Londres, qui doit certainement le posséder dans sa Bibliothéque. Voici d'ailleurs la liste des établissements scientifiques de Londres qui ont reçu cet ouvrage a l'époque de sa publication: Société royale, Société astronomique, Société royale de littérature, et Société linnéenne.

J'espère que ces details pourront être utiles à votre honorable correspondant.

Bruxelles, le 8 Juillet A. LANCASTER,
Attaché au Secrétariat de l'Académie royale des Sciences de Belgique

IN reply to Dr. Ingleby's note I may state that many papers by M. Chasles on various subjects in the history of Mathematics, are to be found in the volumes of the *Comptes Rendus* for 1837, onwards. His "Aperçu Historique" &c., originally appeared as a special volume of the Transactions of the Brussels Academy, but was sold as an independent work. It appeared in quarto, and was published in 1837. Like his "Traité de Géométrie Supérieure," it is very rare, and fetches an enormous price. Mr. Quaritch is, perhaps, the most likely bookseller in London to be able to procure it. The German translation by Sohncke is comparatively cheap, and may be readily obtained through Messrs. Williams and Norgate.

Torquay, July 9 G. E. DAY

The Specific Heat of Mixtures of Alcohol and Water

IN the report of the papers read at the Academy of Sciences, Paris, June 13, which appears in NATURE for June 30, it is stated that MM. Jamin and Amaury presented a note on the above subject, in which they point out, apparently as if it were something new, that the specific heat of some of these mixtures rises even above that of water.

Now, more than two years ago, March 26, 1868, we communicated a paper to the Royal Society giving the specific heat of various mixtures of alcohol and water, and drawing special attention to the remarkable fact that the specific heat of these mixtures is not only above the calculated mean specific heat, but that in all those of less strength than 36 per cent. of alcohol, it is higher than the specific heat of water itself. A knowledge of this fact should therefore be old by this time.

An abstract of our paper is printed in Proc. R. S., vol. xvi., p. 337. Subsequently we examined this and various other properties of similar mixtures more in detail, and communicated our results to the Royal Society in a second paper, an abstract of which is printed in Proc. R.S., vol. xvii., p. 333, and the paper in full in Phil. Trans. for 1869, Part II., p. 591.

The insertion of the above in the next number of your valuable journal will greatly oblige A. DUPRE & F. T. M. PAGE

Westminster Hospital, July 2

Geographical Prizes

HAVING been chiefly instrumental in causing prize medals to be offered by the Geographical Society for competition among the chief public schools, I do not like Mr. Wilson's letter in your last number to pass without comment.

Geography may be, to use his words, a subordinate branch of education, but I maintain that it is so only in the sense that it underlies a large part of liberal knowledge. It underlies the study of history. For example, I do not see how a boy could thoroughly understand Bible history without having acquired a very vivid conception of the geography of Palestine, and the same is true for all other histories, ancient and modern. It follows, as a matter of fact, that geography is incidentally taught to a considerable extent in schools, and I am sorry to say it is sometimes very ill-taught, as we learn from the reports of our examiners, but