

graph we are told that loss of water is not essential; but that polymerisation is a form of condensation. In the next we read: "By means of condensation (*i.e.* the formation of a substance from two others) chemists have been able to prepare far more new bodies and entire classes of bodies than by any other process."

With the exception of a few mistakes in spelling, especially of proper names, and a few omissions in the index, the work of the translator leaves nothing to be desired.
J. B. COHEN.

Physikalisch-chemische Propädeutik, Erste Hälfte. Von H. Griesbach. (Leipzig: Wilhelm Engelmann, 1895.)

THIS work is designed mainly for the use of the chemist and the doctor; for, according to its author, the former, when engaged on certain legal inquiries or on questions relating to hygiene, must know something of medical science, and the latter, in order to follow his calling to advantage, must be familiar with much that is chemical and physical.

The present volume is the first half of the work, and deals with physico-chemical science and logic, the origin, nature, methods, and aim of physico-chemical science, measurement and systems of measurement, time, space, matter, energy, motion, velocity, the divisibility and constitution of matter, hypotheses regarding the ether, the atomic hypothesis, living and dead matter, organised matter as producing fermentation and disease, &c.

The reader requires no special scientific knowledge to follow the information supplied, which differs essentially from that given by most of the text-books, as a detailed historical account, containing short biographical sketches of leading investigators, is given in the case of each of the subjects dealt with. References are also given to original papers, and although the material discussed is mainly theoretical, apparatus and methods are also treated to some extent.

For a book which deals with subjects so widely apart as, say, the genesis of the elements and the karyokinesis of a living cell, the information is remarkably accurate, up to date, and well arranged; and the historical method adopted in the case of subjects which are but seldom handled in this way, makes the book specially interesting and valuable.
J. W. R.

The Pterophorina of Britain. By J. W. Tutt, F.E.S. Pp. 161. (Hartlepool: John E. Robson.)

ALTHOUGH dignified with the title of a monograph, this work, reprinted from the *British Naturalist*, is a carelessly compiled reproduction of almost every statement which has ever been published upon the British species of Plume-moths. Mr. Tutt has not attempted to condense into a useful or readable form this mass of crude material, which, however, may prove attractive to a certain type of collector. The generic diagnoses, unaccompanied by synoptic tables or figures, are taken mainly from Jordan's abstract of Wallengren's "Scandnaviens Fjädermott," and the specific characters are given mostly in the words of other writers, two or three descriptions being sometimes quoted for a single species. The book is roughly printed, and contains several misspellings of names; it will bring little credit to author or publisher, though as a compilation it may prove useful to those who care to search its pages.

Submarine Telegraphy and other papers. By James Bell, A.Inst.E.E., and S. Wilson. Pp. 63. (London: Electricity Office, 1895.)

A COLLECTION of papers, originally published in the columns of *Electricity*, dealing with matters belonging to technical telegraphy. Will be especially serviceable to persons engaged in the postal telegraph service, but appeals to all practical electricians.

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LETTERS TO THE EDITOR.

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The Astronomical Theory of the Glacial Period.

TWO letters have recently appeared in NATURE (October 17, p. 594, and November 29, 1895, p. 29), in which Sir Henry Howorth attacks Sir Robert Ball as the author of a work entitled "The Ice Age," on the ground that the supposed astronomical cause of glaciation is totally inadequate to produce the alleged effect.

I do not now write because I have any new contribution to make to the discussion, but because the author of the review in NATURE (January 28, 1892) of "The Ice Age" might perhaps be expected to express an opinion on the subject in the columns of NATURE.

I still think that the book has the merit of laying down the simpler issue as to the direct effect of the variation in the eccentricity of the earth's orbit on climate, and of setting aside the many collateral causes with which Croll has somewhat clouded the subject.

I wish, however, to reiterate that Sir Robert Ball has, as I think, emphasised the wrong numbers, when he lays so much stress on the ratio 63 to 37, which expresses the ratio of the heat received by a whole hemisphere in its summer to that received in its winter. The really important point to consider is what change that ratio undergoes when the eccentricity of the orbit varies.

In my review it was shown that, with maximum eccentricity of the earth's orbit, and with summer in perihelion, the ratio of the daily supply of heat in summer to that in winter must be augmented by the factor $\frac{199}{166}$; whilst with summer in aphelion

the same ratio must be reduced by the factor $\frac{166}{199}$. Thus the con-

trast between the two configurations is best represented by the ratio of 199^2 to 166^2 , or of nearly 6^2 to 5^2 , or say as 3 to 2. These are the numbers which deserve emphasis.

The astronomical theory has, however, been recently subjected to a powerful criticism by Mr. Culverwell in some papers in the *Geological and Philosophical Magazines*,¹ and the criticism is, I understand, adopted by Sir Henry Howorth. A concrete case (using only round numbers) will express very shortly Mr. Culverwell's argument. At present, with practically zero eccentricity of the earth's orbit, in latitude 51° the ratio of the daily supply of heat in summer to that in winter has a certain magnitude, say A. Then the corresponding ratio for latitude 55° is $\frac{5}{6}$ A; and for latitude 47° is $\frac{6}{5}$ A. Now this difference is found to have

nearly the same value, viz. 4° , for all the middle latitudes, so that it may be concluded that the alleged cause for glaciation would give London a climate something like that of Yorkshire; and the converse would produce a climate something like that of mid-France. The parallelism of the two cases is by no means perfect; but with allowance of the widest margin of uncertainty, it seems that neither a polar nor a tropical climate could be produced by the astronomical cause.

Is there any great flaw in Mr. Culverwell's argument? I do not at present see one; and great as are the uncertainties of the case, they seem insignificant as compared with those involved in calculations founded on the temperature of space, as used by Croll and Ball. Mr. Culverwell has independently carried to its logical conclusion the same line of argument as that of my review, and I can now only confess with regret that I did not perceive whither it tended.

The astronomical theory of the great changes of climate or which geology affords evidence is so alluring, that I cannot sur-

¹ *Phil. Mag.*, December 1894, p. 541; *Geolog. Mag.*, decade iv. vol. ii. No. 367, p. 3, January 1895, and No. 368, p. 55, February 1895. Since this letter has been in type, I have read a valuable paper by Mr. G. F. Becker (*Amer. Journ. Sci.*, vol. xlvi. August 1894), in which he concludes that zero eccentricity of the earth's orbit will present the condition most favourable to glaciation. I have to thank Sir H. Howorth for reminding me of this paper.