

A very noticeable feature of the volume is the large space devoted to the consideration of the Liverworts. It is quite surprising how little attention botanists have been accustomed to pay to this group; many are quite content with acquiring a knowledge of *Marchantia*, which, as it happens, is a very specialised form, and not in the least degree typical of Liverworts taken as a whole. This neglect becomes the less intelligible when one considers the extreme diversity of outward form exhibited even within the limits of a single genus, a diversity only perhaps paralleled by some genera of lichens, or of algæ. And again, their position at the base of the archegoniate series should have sufficed to rescue them from a condition of such unmerited oblivion. Thus the author has done good service by devoting 140 pages to the Hepaticæ alone. It is, perhaps, to be regretted that he did not lay more emphasis on the extent of morphological specialisation which may be observed amongst the species of individual genera; thus, for example, all gradations may be traced in forms included in *Symphlogyna*, from a creeping flat thallus to a complicated branch system, recalling that of the sporophyte of many Hymenophyllaceæ. No doubt this would have entailed an increased number of pages, but, we think, the addition would have been very welcome.

We notice one or two slips here and there. Thus it is stated on page 97 that all the acrogynous Jungermanniaceæ possess a three-sided apical cell, whereas at least one species of *Physotium* is known in which the cell is lenticular in transverse section. But these are but trivial errors, and the general summing-up of the Hepaticæ strikes us as extremely good, even though we may hesitate before accepting all the author's conclusions.

The Vascular Cryptogams are treated very fully, as one would have expected from a writer who has himself contributed so much to our knowledge of the group. But occasionally, if we may venture to say so, the investigator is perhaps a little too much to the front; and whilst individual plants are fully described, and very complete and minute accounts are given of the development of their various organs, one loses, to some extent, the feeling of a wide synthetic grasp of the plants as a whole. It is true, however, that this is atoned for by the suggestive summaries and discussions on phylogeny at the end of the chapters.

Many readers will probably wish that Prof. Campbell had devoted a little more space to a comparative treatment of the external forms, and had also given some account of the numerous biological adaptations which are so abundantly exhibited by many ferns and mosses; and we cannot help feeling that this would have been acceptable, even if it had necessitated the sacrifice of space now devoted to details of development.

The only point about the book which really strikes us unfavourably is the scanty recognition accorded to Palæontology. It is just amongst the vascular cryptogams that the palæophytologist is at his best, and we venture to express the hope that in future editions this most important branch of the subject may be far more fully treated.

If we have seemed to have indulged in a few adverse criticisms, this has not been done with any hostile inten-

tion, but merely by way of attempting to point out how, in our judgment, a really fine work may perhaps be improved. Enough, however, has, at any rate, been said to show that Prof. Campbell has rendered a most important contribution to the literature of botany; and we cannot, finally, suppress an expression of gratification that it should have appeared in the English language.

#### OUR BOOK SHELF.

*A Laboratory Manual of Organic Chemistry.* By Dr. Lassar-Cohn. Translated by Alexander Smith, B.Sc., Ph.D. Pp. vii + 403. (London: Macmillan and Co., 1895.)

It will be readily understood in these days of rapid research that the appearance of a compendium of the latest reagents, processes and apparatus, intended to curtail the labour of the laboratory, would prove acceptable to organic chemists, and it is not surprising that the second German edition of this work should have appeared within a brief period. Its success in Germany may be taken as a guarantee that the English translation will be well received. The original will already have found its way, without doubt, into some of our laboratories. There are many useful laboratory methods scattered through the literature, which are frequently difficult to lay one's hand on at once. The present volume, which has been compiled with much discrimination, gives an account of all the important laboratory operations used in organic research, carefully described and illustrated. In these descriptions either the original account is reproduced, or a sufficiently detailed description is supplied by the author, so that a final appeal to the reference may generally be dispensed with—an important point, since herein lies the essence of the book's utility. The less important methods are briefly mentioned with a reference to the original paper. To try and strike a mean between these two extremes is obviously useless, and occasionally the author falls into this error. For example, under "aluminium chloride" (p. 105), it is stated that this substance is prepared from aluminium and chlorine. Then follows a fairly long account of a method by Gattermann, from aluminium and hydrochloric acid. The description is unaccompanied by any diagram, and is so incomplete that I am confident that any one who attempted to prepare it for the first time from this description would fail. In such a case the author would render a greater service by simply giving the reference.

The book does not aim at teaching the principles of organic chemistry, and it is perhaps not fair to find fault with some of the expressions and definitions used, which are apart from its main purpose. One brief reference may be permitted.

Chapter xii. (p. 10) contains a general account of "condensation." The writer has never met with any good definition of this term, and it is perhaps, as Gmelin said of the relation of organic to inorganic chemistry, more easily felt than defined; but surely we have here a needless confusion of ideas or, at least, of language. In the first paragraph we read: "By condensation we mean the formation of a substance from two others with loss of water, alcohol, hydrochloric acid, ammonia, or a halogen from both components." That the *two others* are not necessarily organic substances we learn from the next paragraph, where an example of condensation is given in the case of hydroxylamine and an aldehyde. Consequently we may include in this term the formation of ethereal salts, nitro-compounds, sulphonic acids, &c. Further down we read: "We include under this heading also the phenomenon of internal condensation, in which a body loses water and forms a new substance." Is the formation of ethylene from alcohol *condensation*? In the next para-

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 "Scandinaviens 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, 1896) 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*,<sup>1</sup> 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  $51^\circ$  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  $55^\circ$  is  $\frac{5}{6}$  A;

and for latitude  $47^\circ$  is  $\frac{6}{5}$  A. Now this difference is found to have

nearly the same value, viz.  $4^\circ$ , 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-

<sup>1</sup> *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. xlviii. 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.