

which should follow Experiment 112 (Preparation of Ethyl Nitrite) have been moved on by a whole page, and made to follow Experiment 115 (Preparation of Nitro-ethane). The utterly bewildering effect of this jumble, which is enhanced by the unexpected re-entrance of the subject of nitro-ethane in the middle of a paragraph a little later on, cannot be realized without reading the passage.

The work is intended to combine practical with theoretical instruction. The selection of experiments is, on the whole, judicious, and the practical directions are generally good. This is not to be wondered at, as the author has evidently, in these points, followed pretty closely Prof. Emerson Reynolds's "Experimental Chemistry," even to such details as the substitution of a tin oil-can for a distilling flask (p. 99), or a peculiarity in the bending of a tube (p. 74), and to the reproduction of some of the illustrations—in every case without acknowledgment. Prof. Reynolds is not, however, responsible for the illustration on p. 17, in which the distillate from a Liebig's condenser is represented as falling from a considerable height into a flask placed below.

It is not true that (p. 12) "every organic compound containing nitrogen will, when fused with metallic sodium, convert the latter into sodium cyanide." Diazo-compounds do not yield any cyanide; and compounds containing sulphur as well as nitrogen form thiocyanate. Nor is heating a cyanide with excess of concentrated sulphuric acid (p. 76) a method of distinguishing it from a formate.

The author's style is occasionally slovenly, and sometimes worse: "Observe how the fact that oxalic acid so readily split up into CO , CO_2 and H_2O support (*sic*) this graphic formula for it" (p. 117).

On the whole, we suspect that teachers will prefer a text-book which calls for fewer marginal corrections.

OUR BOOK SHELF.

An Introduction to the Study of Botany, with a special chapter on some Australian Natural Orders. By Arthur Dendy, D.Sc., and A. H. S. Lucas, M.A. Small 8vo, 272 pages with about 30 pages of woodcuts. (Melbourne and London: Melville, Mullen and Slade, 1892.)

THE authors of this little work are both teachers of Natural Science in the University of Melbourne and it is specially intended for the use of students in Australia. With this object in view it would have been better perhaps to have selected common Australian types to illustrate the life history of the great divisions of the vegetable kingdom; but *Pinus* is taken as a representative of gymnosperms and *Vicia* of angiosperms. Whether these plants are both easily procurable in Australia we are unable to say, but even in that case it would have been better to have taken native plants. Possibly the preparation of illustrations may have influenced the authors, for they are largely, in the first part, "modified," "simplified," or "adapted" figures from well-known books, or they are simply copied. Taken as a whole, we do not doubt that this primer will prove useful to students, but it needs much revision to make it what it ought to be. Here and there, where we have tested it, we have found serious shortcomings. Take for example the account of the divisions of the vascular cryptogams.

"1. *Filicinae*.—These are the ferns which constitute a very large and interesting subdivision. The full account already given of the common bracken renders a detailed

description unnecessary in this place. There are two principal subdivisions of the *Filicinae*; the homosporous, which produce only one kind of spore, and the heterosporous, which produce large megaspores and small microspores. The former include all the ordinary ferns and are again subdivided into six 'families,' of which the Polypodiaceæ are the best known and most abundant, including most of the common ferns, such as *Pteris*."

One would have expected a word or two respecting the heterosporous group—the *Rhizocarpeæ*, with some mention of *Marsilea*, so memorable in the history of Australian exploration; but the authors seem to have come to grief between the older and newer classifications of vascular cryptogams, for in another place (p. 90) we read of "heterosporous ferns." The definition of the *Equisetineæ* contains no reference to the spores; and the description of the *Lycopodineæ* contains no information at all. It runs thus: "This group includes the club-mosses (*Lycopodium*) and the beautiful *Selaginella*, a plant frequently grown in conservatories for decorative purposes. They are all of rather small size, and are popularly spoken of as "mosses" owing to the general appearance of the plant with its numerous very small leaves."

Comment on such a description would be superfluous. In the classification of the cellular cryptogams, lichens are altogether left out, and are apparently not mentioned anywhere. In fact the same incompleteness and inexactness pervades the book, which opens with a eulogistic preface by W. Baldwin Spencer, Professor of Biology in the University of Melbourne. W. B. H.

A German Science Reader. (Modern German Series.) Compiled by Francis Jones, F.R.S.E. (London: Percival and Co., 1892.)

THE idea of introducing to English readers extracts from the works of many well-known German scientific authorities will be thoroughly welcomed. The author has brought together sixteen very interesting articles on several branches of science, supplemented with notes, in which difficult passages are translated, and a glossary of the technical terms not usually found in dictionaries. Among the articles we may mention, Electric Telegraphs by Bernstein; Ice and Snow by Kantz; Air by Müller; Aniline Dyes by Kekulé; Spectrum Analysis by Kirchhoff and Bunsen, &c. W.

More About Wild Nature. By Mrs. Brightwen. (London: T. Fisher Unwin, 1892.)

MRS. BRIGHTWEN's book on "Wild Nature Won by Kindness" was so widely appreciated that she has been encouraged to prepare a second volume of the same general character. It speaks well for her knowledge of animals, and for her interest in their habits, that the new sketches are written in as fresh and bright a style as if she had never before occupied herself with the mass of subjects with which she deals. She is a careful and accurate observer, and all readers who care for natural history will find much to please them in the facts and impressions she records. The author's illustrations add greatly to the charm of the text.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended or this for any other part of NATURE. No notice is taken of anonymous communications.]

Arborescent Frost Patterns.

ON Sunday last, December 4, I observed a curious phenomenon, which I do not remember having ever seen before in the streets of London. Along the Euston Road, the Marylebone

Road, and other thoroughfares having an east and west direction the paving flags were all covered with a striking, vegetable-like pattern which might be most appropriately described as an arborescent tracery. The pattern was not formed of the usual small and delicate frost figures such as we are familiar with on window panes, but was made up of large and boldly-fronded designs such as shown in the sketch, which I hurriedly made on the spot:—



The "fronds" were from one to two feet in length, and often most gracefully curved. A keen wind was blowing at the time from a few degrees north of west and the flags had evidently been coated with a thin layer of mud from the previous night's rain. I attribute the pattern to the rapid freezing and evaporation of the water in this surface layer of mud which was going on during the morning. I only noticed the tracery along east and west thoroughfares; in sheltered streets not swept by the cold wind no design was visible. The phenomenon may be known well enough to others, but by many, like myself, it may have hitherto been passed over unnoticed. My chief object in sending this description is to call attention to the very vegetable-like appearance of the pattern. If allowed to dry in a calm atmosphere and then buried under a fine alluvial or other deposit a record would be preserved which the future geologist might at first sight be tempted to read as "vegetable remains." I have seen very similar tracery in the London clay about Clacton-on-Sea and elsewhere.

R. MELDOLA.

Ice Crystallites.

THE interesting facts recorded by your correspondent C. M. Irvine on p. 31 recall some unrecorded observations of my own. On several occasions during recent winters I have observed these crystallographic forms of ice on a gravel walk by the side of my lawn, in places where, owing to faulty gradients, the water does not completely drain away at the surface, and the ground just below the surface is in consequence more saturated with water than at other spots. The acicular ice-forms have appeared in bundles standing up between the pebbles and capped by earthy material, just as described by Mr. Irvine, and in previous communications to NATURE by Mr. B. Woodd Smith (see his letter on p. 79). The nature of the soil agrees with that described by these two observers, so far as permeability to water is concerned; and I think they appeared on the occurrence of clear frosty weather after a thaw and melting of previous snow. My observations, however, extended further than theirs appear to have done. I was at the time pursuing the study of the glassy acicular *crystallites* of sulphur (which are erroneously described as "crystals" in most textbooks on chemistry). These, on examination with polarized light (as I have described elsewhere) are found to be destitute of any crystalline internal structure (in fact truly vitreous or isotropic masses in spite of their crystallographic outlines); such structure developing, as devitrification proceeds, by crystallization in the orthorhombic system, to which the outlines of the crystallites do not conform.

In NATURE (vol. xxxvii. p. 104) is a letter from myself, recording some observations on the vitreosity of ice, as exhibited under certain suitable conditions by hailstones, and referring to a previous letter (*Ibid.* vol. xxxvi. p. 77), wherein the vitrification and devitrification of water was suggested as the possible

cause of certain structural phenomena observed in them from time to time. It was with those ideas present to my mind that during recent winters I have made an examination of the acicular ice-forms referred to, which struck me as made up of unusually clear and transparent ice. On taking my microscope out of doors, fitted with a polarizing apparatus, when the temperature was a few degrees below freezing, with a thick overcoat on to prevent the heat of one's body from affecting the ice-needles, I found that, on taking them from the ground and placing them at once on the stage between crossed "Nicols," they appeared to be *completely isotropic*, as they had no reaction on polarized light. I have concluded, therefore, that these ice-needles are strictly analogous (physically) to the prismatic crystallites of sulphur; and they resemble precisely the microscopic lathe-shaped forms, into which I have seen a perfectly clear minute plate of sulphur-glass break up in the first stage of devitrification. The explanation suggested by Mr. Woodd Smith, that they may have been formed by a slow growth of ice at their base, the molecular movement of water in the soil keeping up the supply so long as refrigeration continued, has seemed to me the most natural one; their isotropic molecular structure is no doubt due to the rapidity of freezing owing to a sudden fall of temperature at the spot.

A. IRVING.

Wellington College, Berks, November 27.

The *Volucella* as Alleged Examples of Variation "almost Unique among Animals."

IT is barren work for the parties in a controversy merely to deny each other's statements without adducing further evidence. Mr. Bateson first stated that var. *mystacea* did not mimic *Bombus muscorum*. I replied that it did, and the statement in my letter in no way depended on the case at the Royal College of Surgeons, but on a careful comparison of the insects in the Oxford Museum. It is useless for me to repeat that I regard it as an example of mimicry, not indeed equal to that afforded by the same fly and *Bombus hortorum*, but far better than many others which are generally believed to be instances of this principle (such as the resemblance of *Clytus arictis*, or even the resemblance—admitted by Mr. Bateson in his first letter—of *Volucella inanis*, to a wasp). I therefore propose to furnish the Editor of NATURE with photographs of the *Volucella* and humble-bees for reproduction, so that readers can judge of the matter for themselves. I will do my best to obtain a negative which shows the coloured bands.

Although I believed that the two London Museums supported my view, it will be obvious to any one who reads the letter that I did not rely on such support, but on my own comparison of the insects.

Mr. Bateson has offered no further evidence in support of his remarkable assertion that the variation of the *Volucella* is unique. I am not surprised that he should pass over this part of my letter, for I felt sure that there was no further evidence to offer. It will be remembered that this evidence was contained in the "brief statement of facts" given in his first letter, and is practically summed up in the sentence "This fly exhibits the rare condition of existing in two distinct forms in both sexes." In assuming this rarity to be so excessive that the words "almost unique" may be applied to it, and in evidently considering that we must proceed as far as the peach and nectarine in order to find a parallel, Mr. Bateson exhibits a want of acquaintance with the facts of variation which is very surprising in one who is believed to have spent some years in their study. For there is no essential biological difference between this variation and many others, examples of which I gave in my last letter, and which could easily be multiplied. In fact, many a "showcase" would have corrected such a mistake. Compared with the magnitude of this erroneous statement in Mr. Bateson's first letter, the details under discussion assume very small proportions. In considering that "no speculation is needed to enhance the exceptionally interesting facts of the variation and the resemblances of the *Volucella*," it would appear that Mr. Bateson seeks to replace that most invaluable servant of science, speculation, by far-reaching and unsupported assertion.

In his last letter Mr. Bateson says "it is admitted that in making this statement Mr. Poulton relied not on original authorities, but on the general impression of others." So far from this being the case I stated my belief that the impression is prevalent among those who are original authorities on the Hymenoptera and their parasites, and I also showed that nothing