

have been published during the four past years. If, as we suppose, about twenty more parts are required to finish the work, it is manifest that unless the present rate of progress be expedited it will be twenty years before we are able to send our new "History of British Birds" to the binders. The edition was commenced, we believe, in 1871. Now thirty years seems rather long for the execution of a new edition of any work, even with all the improvements which, as we have shown above, the present editor has doubtless bestowed upon it. We would fain ask therefore whether the author and publisher cannot manage to move on a little faster. If this cannot be done it appears to us that the first portion of the work will be almost out of date before the last part is published, and that the subscribers will have good reason to complain.

#### OUR BOOK SHELF

*Jahrbücher für wissenschaftliche Botanik.* Herausgegeben von Dr. N. Pringsheim. Elfter Band, drittes und viertes Heft. With twenty-four plates. (Leipzig: W. Engelmann, 1877 and 1878.)

DR. JAKOB ERIKSSON describes in a lengthened paper the protomeristem of the roots of Dicotyledons, and directs attention to the four great types of structure observable in these roots. In the first type the apex consists of three separate zones of meristem: the plerome, periblem, and dermocalyptrogen. In the second type only two zones are present: the plerome and a common zone for primary cortex, epidermis, and root-cap. In the third type there is a common meristem zone from which all the others develop; while in the fourth there are two zones, the periblem and the plerome. Two additional types are met with in Monocotyledons: (1) in which there are four zones of meristem: calyptrogen, dermatogen, periblem, and plerome; and (2) in which there are three zones: the calyptrogen, the plerome, and a common zone for cortex and epidermis.

The germination of *Equisetum* and *Schizæacææ* forms the subject of two papers, one by Sadebeck and the other by Bauke, whose work was arrested by premature death. Woronin contributes a paper on the *Plasmodiophora Brassicæ*, the remarkable Myxomycete which seems to be the cause of the so-called Hernia of the cabbage plant, which has recently attracted so much attention.

The remaining papers are by Reinke, on *Monostroma bullosum* and *Tetraspora lubrica*. Wydler discusses at great length the morphology of certain forms of inflorescence, chiefly dichotomous; and lastly there is a paper by Pitra on the pressure in stems during the appearance of bleeding in plants. The contents of the parts are, as will be seen, very varied and deal with many different departments of botany, and will be found to sustain the reputation of the "Jahrbücher" so long associated with the name of Pringsheim.

#### 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. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

#### Unconscious Memory—Mr. Samuel Butler

WILL you kindly allow me a portion of your valuable space in order that I may demonstrate the completely groundless character of a series of insinuations which Mr. Samuel Butler

has made not only against myself, but also against Mr. Charles Darwin, in the work which he has recently published, entitled "Unconscious Memory" (Op. 5).

1. Mr. Butler insinuates that Mr. Darwin caused my essay on Dr. Erasmus Darwin to be translated simply in order to throw discredit on his work, "Evolution, Old and New" (Op. 4), which was published in May, 1879. Upon this point I have to observe that Mr. Darwin informed me of his desire to have my essay published in English more than two months before the appearance of Mr. Butler's book; that the translation did not appear earlier is due to the fact that I asked for a delay in order that I might be able to revise it.

2. The assumption of Mr. Butler that Mr. Darwin had urged me to insert an underhand attack upon him (Mr. Butler) in my sketch, is not only absolutely unfounded, but, on the contrary, I have to state that Mr. Darwin specially solicited me to take no notice whatever of Mr. Butler's book, which had in the meantime appeared. Since however I thought it desirable to point out that Dr. Erasmus Darwin's views concerning the evolution of animated Nature still satisfy certain thinkers, even in our own day (a fact which must add greatly to Dr. Darwin's reputation), I have made some remarks upon the subject in a concluding paragraph, without however naming Mr. Butler. And I may here emphatically assert, that although Mr. Darwin recommended me to omit one or two passages from my work, he neither made nor suggested additions of any kind.

3. Mr. Butler's assertion that the revision of my translation was made "by the light" of his book is only in so far justifiable that I looked over the latter before sending off my work, and that my attention was thereby called to a remark of Buffon's. From Mr. Butler's book I have neither taken nor was I able to take the slightest information that was new to me concerning Dr. Erasmus Darwin's scientific work and views, since in it practically only one portion of the "Zoonomia" is discussed at any length, and this portion I had already quoted and analysed, while Mr. Butler only refers to one comparatively unimportant part of the "Botanic Garden," and absolutely ignores the "Phytologia" and the "Temple of Nature." So that no single line of Mr. Butler's far from profound work was of the slightest use to me.

Mr. Butler's contention that I have quoted from his book a remark from Coleridge is entirely without foundation. I have been acquainted with this remark for years, and from the source quoted. It is also quoted in Zoeckler's work (vol. ii. p. 256), mentioned by me on p. 151, which appeared prior to Mr. Butler's book (Op. 4). The whole of my indebtedness to Mr. Butler reduces itself therefore to a single quotation from Buffon.

4. Finally, as concerns the main accusation that no mention is made in the preface of the fact that my essay had been revised previously to publication, it is clear, as even a child could not fail to see, that this is not due to design, but is simply the result of an oversight. It would be simply absurd for a writer intentionally to attack a publication which appeared subsequently to the date indicated on his title-page; and the so-called falsification, so far from injuring Mr. Butler, could only be most agreeable to him, because it might induce the careless reader to fancy that no reference whatever was intended to Mr. Butler in the closing sentence. Should however such a reference be clearly intended—and to every reader posted up in the subject this could not be doubtful—every man of common sense would recognise this terrible falsehood to be a simple oversight.

Be'lin, January 12

ERNST KRAUSE

#### Hot Ice

I VENTURE, in referring to Dr. Lodge's letter of this week, to put before your readers the meaning of the remarks made on Dr. Carnelley's experiment at the Chemical Society by Prof. Ayrton, who is now away from England. I understood him to say that as Dr. Carnelley's hot ice is obviously in a condition which cannot be represented within the as yet known fundamental water surfaces, it is necessary to produce these surfaces beyond the places at which, hitherto, abrupt changes have been supposed to take place in them. He took as an instance the ice-water surface which has hitherto been assumed to stop at Prof. James Thomson's "triple point," and showed that although Sir Wm. Thomson's experiments have proved that it is nearly plane for the stable state of water and ice, yet in the imaginary district beyond the triple point a change of latent heat might give such a change of curvature as to bring this surface into the hot-ice region.