

1672. In 1682, he says that Grew published an enlarged edition of this smaller work under the *same* title. But this is not really the state of the case. The title of the large book is "The Anatomy of Plants, with an Idea of a Philosophical History of Plants." The volume has Sir Christopher Wren's *imprimatur*, which runs as follows:—

"At a meeting of the Royal Society, Feb. 22, 168 $\frac{1}{2}$, Dr. Grew having read several *Lectures* of the *Anatomy of Plants*, some whereof have been already printed at divers times, and some are not printed; with several other *Lectures* of their *Colours, Odours, Tasts*; as also of the *Solution of Salts in Water*; and of *Mixture*; all of them to the satisfaction of the said *Society*: It is therefore Ordered, That He be desired, to cause them to printed (*sic*) together in one Volume.

"CHR. WREN, P.R.S."

The "Anatomy of Plants Begun" is simply reprinted in this volume. "The Anatomy of Leaves, Flowers, Fruits, and Seeds" is, however, printed for the first time. In the second part of this, called "The Anatomy of Flowers prosecuted with the bare eye and with the microscope," which was read before the Royal Society, Nov. 9, 1676, is contained Grew's discussion of the function of the parts of the flower in which the statement about Millington occurs.

Grew's "Anatomy of Plants" can no more be described as a second edition of the "Anatomy of Plants Begun" than Prof. Huxley's "Lay Sermons" can collectively be described as a second edition of any one essay republished in that volume.

The object of the quotation from Sprengel was to show what was his opinion of the claims of Camerarius to be considered the discoverer of sexuality in plants. As Mr. Bennett (vol. xiii. p. 166) makes a point of nothing being cited from Sprengel as regards Millington; here is what Sprengel says on that head. Speaking of Grew:—

"Summam vero meruit et seræ posteritatis gratitudinem, quod primus sexum differentiam in partibus vel fecundantibus vel fecundandis non invenit, sed tamen defendit ac evulgavit. Ipse verecunde satis et candidè Thomam Millingtonium, Savilianum professorem Oxonii nominat, qui sibi dixerit, apparatus eum seminiformem (the anthers) vices partium muscularum probabiliter gerere" ("Hist. rei Herb.," ii. 14).

Next as to Camerarius and Ray, Mr. Bennett says that the observations of the first antedated those of the second by two years. On Mr. Bennett's own showing the date of Camerarius's tract is 1694 (NATURE, vol. xiii. p. 86). The date of the first volume of Ray's "Historia," in which he alludes to the subject, is 1686.

As to Theophrastus it is well known that classical writers on natural history were aware that the unisexual flowers of the date required the "pulvis maris," or pollen, to enable them to set their fruit. But I am not aware that till the time of Grew and Millington the fact that the vast majority of plants contain stamens and ovaries, *i.e.*, both male and female organs, had ever been ascertained. What these persons did for the first time was to point out the function of the essential organs of the flower.

Mr. Bennett, instead of taking his facts secondhand from Prof. Sachs's no doubt excellent "Geschichte," ought to have looked into the authorities himself. He would then avoid the error of quoting non-existent editions and of drawing conclusions which would be inextinguishable if they were not based on erroneous dates.

A. B. C.

Article "Birds" in "Encyclopædia Britannica"

In that portion of the article "Birds," which I have lately written for the "Encyclopædia Britannica," I said (page 729, column 2) that *Odontopteryx* had "jaws armed with true teeth," and in this respect resembled *Ichthyornis*. The mistake has just been pointed out to me, and I shall be greatly obliged by being allowed to correct it, as far as is possible, in NATURE. The sentence should run thus: "jaws armed with tooth-like processes, and in this respect differing from Professor Marsh's *Ichthyornis*."

ALFRED NEWTON

Athenæum Club, Jan. 3

Fertilisation in the Basidiomycetes

IN your review of Dr. Pringsheim's "Jahrbücher" (NATURE, vol. xiii. p. 156) you refer to Dr. Max Reess' paper on the Fertilisation of the Basidiomycetes; this paper you compare with the results recently obtained by Van Tieghem, Dr. Eidam, and my-

self, and you say that the observations of the three former all tend in one direction, which fact should lead botanists to look with very great caution on my results, which are somewhat different.

As I am tolerably well acquainted with the three papers first mentioned, perhaps you will kindly allow me to point out that Dr. Reess' carpoconium, and the carpoconium of Dr. Eidam, are very different bodies, and that the latter author, in the "Botanische Zeitung," even puts a note of interrogation before his own interpretation of the body he figures as a possible carpoconium.

The spermatozoids as described and illustrated by me in the *Gardeners' Chronicle* for Oct. 16 and 23 last, are not essentially different from Dr. Eidam's spermatia; they agree in size, but I maintain that the threads which bear these male bodies come direct from the cystidia, and not from the basidia, and that they are at first spherical. In Dr. Eidam's excellent plate there are sixteen germinating spores shown which do not produce spermatia, and in each instance the spores are shown as ruptured. Three other spores are shown as producing spermatia; now these latter spores are engraved to twice the size of the former, and all three are unruptured. The explanation simply is that the latter threads have not come from the spores at all, but from a cystidium—the spores engraved have not germinated, and have merely been washed against the spermatia-bearing threads.

As for the species experimented upon abroad (except Van Tieghem's plant), one is rare, and the other not British; the plants I have been working upon are common everywhere.

In the January number of the *Popular Science Review* will be found an illustrated paper of mine on the "Reproduction of *Agaricus lacrymabundus*." In this essay will be found not only some new facts as to the reproduction process in the Basidiomycetes, but a *résumé* of the views now generally held on this subject.

WORTHINGTON G. SMITH

The Late Eclipse

I FIND in NATURE, vol. xiii. p. 86, a letter from Dr. Schuster, commenting on some remarks made by me last April respecting the photographic results of the late eclipse. He appears to consider that these remarks related to him personally, which certainly was not my intention. He speaks further of a mathematical solution promised by me, for which he has "had to wait already a considerable time." I remember nothing of such a promise, nor can I conceive how I could have promised, instead of giving at once, the solution of so simple a matter. Dr. Schuster proves very readily that the spectrum of the corona can be photographed in one minute; but I am not aware that anyone has questioned the fact. What I questioned myself was whether the spectral images of the corona can be so photographed that the true extension of the corresponding coronal envelopes can be shown. To quote my own words ("Science Byways," p. 168): "The whole light" [of the corona] "acting at once to form a photograph does not show the full extension of the corona, the outskirts simply losing themselves through excessive faintness. . . . How, then, can a minute portion of that light produce any photographic trace" [of the outskirts]? "How much less can this minute portion show the whole extension of the green solar envelope?" It was the hope that this might be effected which I described as mathematically unsound. I am so busy that I cannot enter further into this matter. But in any case the only justification of controversy respecting it would be the hope that some purpose useful to science might be subserved. This seems unlikely.

RICHD. A. PROCTOR

New York, Dec. 16, 1875

Blowpipe Analysis

THANKING you sincerely for the very well written and not altogether unkind (if rather severe) review of my lately published work on this subject (NATURE, vol. xiii. p. 164), against any part of which I would not at present presume to appeal, I would ask for a corner of your valuable space to explain, with regard to "the production of a precipitate" of sodium sulphide by the addition of a drop of water to a fused mass of soda with a sulphide on aluminium plate, that the term "precipitate" undoubtedly used by me (as the reviewer says so) is obviously a "slip of the pen," for there can be no room to precipitate anything in a drop of water from a fused mass on aluminium plate.