OUR BOOKSHELF.

The Inter-Relationships of the Bryophyta. By Dr. Frank Cavers. Reprinted from the *New Phytologist*. Pp. vi+203. Cambridge: At the Botany School, 1911. Price 4*s*.; postage 4*d*.

WE are a little late in announcing that Dr. F. Cavers's series of articles which appeared on the inter-relationships of the Bryophyta in the *New Phytologist,* vols. ix. and x., 1910–11, has been issued separately. It is a great convenience to have the work in this form, and it certainly deserves this distinction. The classification is mainly that adopted in Engler and Prantl's "Natürlichen Pflanzenfamilien," but as a result of his investigations the author introduces some modifications. His proposed divisions are: (1) Sphærocarpales, (2) Marchantiales, (3) Jungermanniales, (4) Anthocerotales, (5) Sphagnales, (6) Andreæales, (7) Tetraphidales, (8) Polytrichales, (9) Buxbaumiales, and (10) Eu-Bryales.

Dr. Cavers discusses more particularly the question of the old primary division of the Bryophyta into two classes, Hepaticæ and Musci, especially in relation to the Anthocerotales and the Sphagnales. He argues: "If the Anthocerotales are to be made a separate class apart from the Hepaticæ, either Sphagnales should also be considered a separate class apart from the Musci, thus making four primary divisions of Bryophyta—Hepaticæ proper, Anthocerotes, Sphagna, and Musci proper—or the Anthocerotales and Sphagnales might be united to form a class between the Eu-Hepaticæ and the Eu-Musci, thus giving three classes of Bryophyta." But he prefers dividing the Bryophyta into ten groups as designated above.

The account of *Riella capensis* is of special interest, and it is to be followed by a more detailed paper on the genus generally. Until 1902 this singular aquatic genus was only known to inhabit the Mediterranean region and the Lake of Geneva. Since then a species has been discovered in the Grand Canary; another in Texas; a third in Turkestan; and a fourth in South Africa.

LETTERS TO THE EDITOR.

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Determination of the Epicentre of an Earthquake.

IT has been proved by observation with the Galitzin seismographs, both at Pulkowa and Eskdalemuir, that when the first phase P of an earthquake is sharp, the azimuth of the epicentre from the station is uniquely determined by the observations at that station. It follows that if the azimuth of the epicentre is determined at two independent stations suitably situated, the epicentre can be determined from these two azimuths alone.

We have to-day, as an example, verified by construction and by computation that this principle gives accurately the epicentre of the earthquake that occurred in Monastir on February 18, 1911.

The azimuth observed at Pulkowa was 22° 53′ west of south, while the azimuth observed at Eskdalemuir was 55° 56′ east of south. The resulting epicentre we find to be 40·5° N., 20·3° E.

The epicentre deduced by the Pulkowa observations of azimuth and epicentral distance was 40·5° N., 20·1° E.; while the similar deduction from the Eskdalemuir results was 40·3° N., 20·4° E.

It is clear that in this case the accuracy of determination from the azimuths alone equals that of the determinations from the separate stations, and it is known that the earthquake did occur in the region indicated.

The advantages of this new method based on azimuths alone are:—

(1) That it is quite independent of any time reckoning whatever at the two stations.

(2) That it is independent of the determination of the second phase S on a seismogram (which is frequently difficult to fix with certainty).

(3) That it is independent of any empirical tables for epicentral distance, which are admittedly only approximate.

(4) Although only two stations are used, the determination is unique.

We may observe that for a given case the accuracy of determination depends on a suitable choice of the two stations.

B. GALITZIN.
GEORGE W. WALKER.
The Observatory, Eskdalemuir, Langholm, Dumfriesshire, August 29.

Implements of Man in the Chalky Boulder Clay.

IN NATURE of August 15 Mr. Reid Moir has given us certain interesting facts observed by him in connection with the scratching of flints.

(1) He notes the occasional scratching of what remains of the "cortex" of the original nodule. It does not seem to have occurred to him that such a result may have been produced while the flint was still enclosed in its original chalk matrix. Topley (in his "Geology of the Weald") showed long ago that the chalk strata had in many cases undergone considerable differential movement concomitant with crustal movements; and I have myself seen crushed flints still *in situ* in the chalk cliffs at Ventnor, where there is evidence of intense crustal movement of the strata. So far back as 1880 I noted this, also the extremely fractured and unworn condition of the flints left as a residuum from the solution of the chalk by carbonated rain-water on the top of St. Boniface Downs (see P.G.A., vol. viii., No. 3), and my interpretation of the phenomena there observable has since been confirmed by Dr. A. Strahan, F.R.S., of the Geological Survey. Here we have a sufficient mechanical cause totally independent of anything that may be connoted by the term "glaciation." There seemed, moreover, to be just that slight amount of surface-staining of the fractured surfaces which might be due to meteoric iron-dust.

(2) One fails to see that there is any mystery about the non-striated condition generally of the fracture-surfaces of the flint fragments from the Boulder Clay. How could the soft matrix of the Boulder Clay scratch a flint, or even hold a harder stone with sufficient grip to give it effect as a graving-tool, however great the volume-pressure may have been? When the "glazier" wants to cut glass he does not use putty to hold his "diamond." So much for the talk of "intense glaciation" of hypothetical pre-Crag flints, on which I hope to have shortly more to say.

On the other hand, boulders of the Chalk itself, if

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