

spinulosa, and have concluded that whilst their Diploxyloid organisation differs from that of the *Lepidodendra*, it justifies the conclusion that *Sigillariæ* were not lycopodiaceous but gymnospermous plants. But I have already shown that several indisputable *Lepidodendra* have precisely the same organisation. Hence I contend that Brongniart's reasons for separating these plants have no existence, and consequently his conclusions must be abandoned; M. Grand' Eury, forgetting this part of my work, and only remembering that I have also described the bark of a true *Syringodendroid* *Sigillaria*, and shown that it is identical in every feature with the corresponding tissue in *Lepidodendra*, says that I have arrived at my conclusion "par des faits isolés d'après l'analogie de l'écorce, et non par des exemples complets réunissant les caractères extérieurs aux caractères intérieurs." How, in the face of my published memoirs, my friend could make so erroneous a statement, I am at a loss to conceive.

I should have felt it necessary to have subjected the volumes under consideration to an incisive criticism on these and some similar points, were it not that he kindly allows me to quote from some letters which I have received from him. In these communications he says:—"Les points sur lesquels nous différons sont précisément ceux que je n'ai pas étudiés." Referring to facts which I have observed, he adds: "Comme je n'avais pas ces faits pour me guider, j'ai conclu d'après ce que je connaissais bien, et je ne suis pas fain de conclure que je me suis trompé; dans ce cas nous aurions dans les *Sigillaires* et les *Lepidodendrons* des cryptogames excessivement élevées en organisation; si élevées qu'ils formaient, en quelque façon, une classe intermédiaire entre ces plantes et les *Gymnospermes*." "L'Association presque constante des macrospores avec le débris des *Sigillaires* est en faveur de vos conclusions." Quite in accordance with the above remarks are the following observations which the author makes in his volumes: "Il est au moins curieux que, à part le corps vasculaire, les autres parties des *Sigillaires* soient semblables aux parties correspondantes des *Lepidodendrons*." This is perfectly true with the exception that the vascular portions are less exceptional than M. Grand' Eury's remarks imply. The *Sigillarian* stem is merely that of *Lepidodendron* *Harcourtii*, with an additional exogenous vascular zone interposed between that of the *Lepidodendron* and its investing cortex; and which I find in other true *Lepidodendra*. If all the plants of the coal-measures which possess a similar exogenous zone are to be transferred from the cryptogamic to the phanerogamic group, there will be few cryptogams left in the carboniferous rocks beyond *Lepidodendron* *Harcourtii* and the ferns. M. Grand' Eury concludes his notice of the *Sigillariæ* by a remark which I fully endorse: "Seulement je crains d'avoir tiré des conséquences trop complètes de ces indices insuffisants, dont je n'aurais peut-être alors même dû parler que pour éveiller l'attention des observateurs sur une solution possible du plus important problème de la paléontologie végétale."

Having thus indicated some very important points respecting which I am compelled to differ from M. Grand' Eury, I can with the sincerest truth again express my sense of the value of this new contribution to the study of the carboniferous flora, and of the praiseworthy perseverance with which the author

has laboured for many years in collecting his materials. The most prominent fact which the work reveals is the remarkable abundance of the *Cordaites* in the coal-measures of Central France, compared with what we see in England. In some districts, as M. Grand' Eury informs us, the coal is almost entirely composed of their *débris*. I have met with nothing like this in Great Britain, but it is in strict accordance with what we know of the distribution of living plants, that whilst similar types may be expected to be met with over wide geographical areas, some forms will predominate in one region, whilst in other localities different types will prevail; hence the materials out of which coal has been found must have been widely different at these various spots.

The plates with which the above work is illustrated are extremely beautiful, as is usually the case with the productions of the French lithographers.

W. C. WILLIAMSON

OUR BOOK SHELF

Annaes do Observatorio do Infante D. Luiz. Magnetismo Terrestre. Lisboa, 1876.

THIS part of the *Annals* of the Lisbon Observatory is a continuation of those noticed in *NATURE*, vol. xiii. p. 301. The results for the magnetic declination are carried forward from 1867 to 1871, while some include the means from 1858 to 1875. This is the case for the secular change and annual variation. Mr. Capello found previously that the north end of the declination magnet approached the north at the rate of 5'91 yearly (1858-1868). The results he now divides into two series, 1858 to 1866, with a rate of 5'46, and 1866 to 1875, with a rate of 7'64 yearly.

The yearly means are deduced from observations at 8 A.M. and 2 P.M. Mr. Capello has also shown that the diurnal law of disturbance appears to be different at Lisbon in different years of the decennial period.¹ In this case, even if two observations daily were otherwise sufficient to give accurate means, or means strictly comparable from year to year, the varying effect of the disturbance on the observations at the two hours mentioned would of itself interfere with this comparability. It is probably for these reasons that the yearly means at Lisbon do not appear to show the small decennial inequality in the secular movement first indicated by me in 1857, and afterwards discovered by Hansteen and Lloyd.

Mr. Capello has repeated discussions for the magnetic disturbances with the increased materials in his possession. He had observed in a preceding number of the *Annals*, that many observations which were considered disturbed (that is to say, which differed from the means for the hours by 2'26 or more) really belonged to diurnal variations which were regular, only larger than usual; and it was pointed out in *NATURE* (in the notice cited above) that one cause of these excessive deviations would be found in the superposed lunar actions. Mr. Capello now finds that a great majority of these quasi-solar disturbances are rather to be considered due to the moon. This conclusion induces me to believe that if Mr. Capello had the necessary aid to perform the calculations for the lunar diurnal variations for each month, and for different positions of the moon, as well as for other investigations, the Lisbon observations could not fail to add many important scientific results to those already published.

JOHN ALLAN BROWN

Incidents in the Biography of Dust. By H. P. Malet. (London: Trübner and Co.)

THE first impression one gets of this book is that of a