of the younger meteorologists on the Continent. These papers are accompanied by tables giving the mean pressure, temperature, humidity, rainfall, wind, and cloud; and their very great value will be recognised when it is stated that they embrace places whose meteorology was little, if at all known, such as Rio Janeiro, Parana, Mendoza, Monte Video, Buenos-Ayres, Punta-Arenas, Puerto Montt, Santiago, Valdivia, Valparaiso, Serena, Copiapo, and Lima, in South America; Bagdad and Samaua in Mesopotamia; Kuldscha in West China; St. Anna, near Manila, Philippine Islands; and Said, Ismailia, and Suez. Since broad and just views of the atmosphere and its movements can be attained only through the accumulation of such facts and an intelligent discussion of them, our best thanks are due to the Austrian meteorologists for these invaluable contributions. If meteorology were prosecuted more in this spirit than, unhappily, has been the case, it would be marred by fewer crude and hastily-formed theories; and particularly inquiries into local climates and weather over limited portions of the earth's surface would be conducted on sounder principles, and be productive of results which could be accepted as solid contributions to science. We heartily recommend this journal, especially since in this country we have nothing to compare with it,—no periodical which so well puts meteorologists and physicists *au courant* with this rapidly-advancing science.

Das Leben der Erde: Blicke in ihre Geschichte, nebst Darstellung der wichtigsten und interessantesten Fragen ihres Natur- und Kultur-lebens. Ein Volksbuch von A. Hummel. (Leipzig: F. Fleischer; London: Williams and Norgate, 1870.)

It is always a question of doubtful expediency whether it is wise to compress into one work by one writer a complete history of Nature, even in a popular treatise. This has been attempted by Herr Hummel in this volume of 424 pages, and, as far as such an attempt can succeed, not unsuccessfully. We have first a glimpse of the origin of the earth, and of its relations to the solar system. Then follows a chapter on the physical geography of the land, describing the main physical features of the solid crust of the globe. Next we have a treatise on water, and the part it has played in the formation of the existing surface of the earth. To this succeeds a chapter on the atmosphere and its phenomena. In conclusion we have a general sketch of the vegetation of the earth, and of the forms of animal life, in which the author declares against the Darwinian theory of the origin of species. Written occasionally in the inflated language in which continental popular writers too much indulge, the work is, nevertheless, a good one to put in the hands of young people with the double purpose of giving them some knowledge of natural science and of German. It was published on the hundredth birthday of Alexander von Humboldt, as a tribute to the memory of the great naturalist.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Sun's Parallax

Is there nobody who will perform an act of justice, and ask those who seem to have never known or to have forgotten my doings, to be kind enough not to deprive me of my just claims? When, A.D. 1857, my old method of determining the sun's parallax was again publicly proposed, I thought it somewhat strange, and wondered what could be the reason that it should be treated as if it were some new and not a very old acquaintance of Science. When, some time later, a stir was made about what was represented as a new method of investigating the motion of the solar system in space, and, instead of a new, there was brought forward an old acquaintance (known to Science since the times of your grandfathers), only dressed anew, and engaged to perform some truly "astounding" antics, I wondered indeed that no friendly hand should have prevented such an exhibition, but I also comprehended the true state of affairs. And since then I have had to shrug my ghostly shoulders so often when learning further news about your curious knowledge of Science, and your strange opinions, and your queer notions of honour, and justice, and fairness, that I have long ceased to wonder at anything some of you may say or do. However, as it is only right that I should be allowed to retain what belongs to me, and as nobody appears to remember my claims, you will probably raise no objection, if I, myself, enlighten you a little, and remind you how, A.D. 1672, I determined the sun's parallax.

Read in the History of my Life (Baily's Account, &c. p. 32):—

"Whilst I was inquiring for the planets' appulses to the fixed stars by the help of Hecker's ephemerides, I found that, in September 1672, the planet Mars, then newly past his perihelion and opposition to the sun, would pass amongst three contiguous fixed stars in the water of *Aquarius*; and that by reason he was then very near the earth, this would be the most convenient opportunity that would be afforded of many years for determining his, and consequently the sun's, horizontal parallax. I drew up a *monitum* of this appearance, and sent it with a letter to Mr. Oldenburg, who printed it in his *Transactions*, No. 86, August 19th, 1672, having before sent my admonition into France, where the gentlemen of their Academy took care to have it observed in several places. My father's affairs caused me to take a journey into Lancashire the very day I had designed to begin my observations, but God's Providence so ordered it that they gave me an opportunity to visit Townley, where I was kindly received and entertained by Mr. Townley, with whose instruments I saw Mars near the middlemost of the three adjacent fixed stars. My stay in Lancashire was short. At my return from thence I took his distances from two of them at distant times of the night. Whence I determined his parallax then 25″, equal to his visible diameter; which, therefore, must be its constant measure, and, consequently, the sun's horizontal parallax not more than 10″. This I gave notice of in the *Transactions*, No. 96; and the French soon after declared that from their observations they had found the same. Whether they will give you such exactness I leave to those who are skilled in these things to determine."

This extract is, I hope, sufficient, and I will leave it to you to search further. Perhaps you may consider my language a little quaint, but then, remember, I lived two centuries ago.

Now, the planet Mars performs 109 sidereal revolutions in 205 sidereal years and 3¼ days, so that its appearance in the year 1877 will not be very different from what it was in 1672. Accordingly I enjoin you to make then the most of your opportunity, and do your best to prove the goodness of my old method, and I wish you thorough success. And when you watch the planet pass amongst the stars in the water of Aquarius, you will, perhaps, remember with kindly feelings an old astronomer, who in life had to endure great injustice and sore trials, and will bless and honour his memory.

THE GHOST OF JOHN FLAMSTEED, M.R.

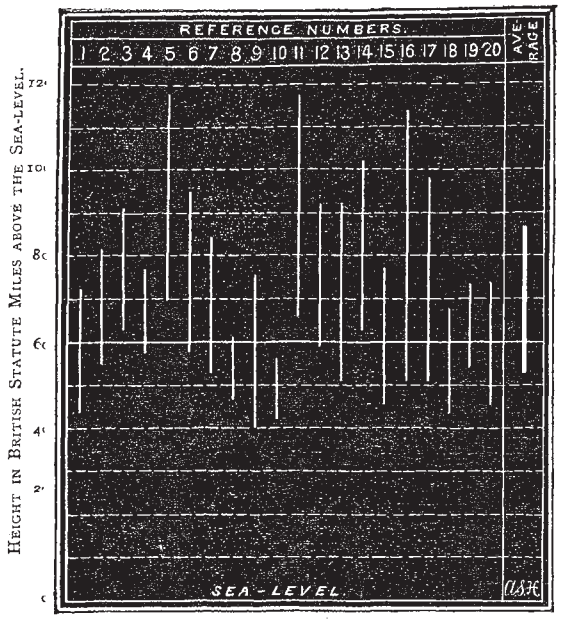
Walhalla

The Marseilles Meteorite

It will probably occur to most of your readers, as it immediately suggested itself to me, on reading in your journal of the 5th inst. a description from *Les Mondes* of a remarkable meteorite observed at Marseilles by M. Coggia, on the 1st of August last, that the bright object having an apparent diameter, at first of about 15′, and at last of a little over 4′, whose uncertain course was noted for eighteen minutes by the stars, was really nothing more extraordinary than a fire-balloon; or it may, possibly, have been some description of brighter signal-light. The planet Saturn, and the other stars named in the description, were all at the low altitude above the horizon, at which a fire-balloon, and other bright signal-lights of ordinary size, floating at an ordinary height in the air, would have about the apparent diameter of the "meteorite." Its apparent diminution in size was, also, perhaps, either the effect of its increasing distance, or of its gradually fading light. After alternately remaining stationary, and changing its apparent course two or three times, it at last fell rapidly in a perpendicular direction. The burning tow, or other inflamed substance with which it was inflated, appears to have detached itself from, or, it may be, to have set fire to the balloon, since it

was remarked that during its perpendicular fall to the horizon it gave out vivid scintillations.

It is difficult, from the exaggerated language of native narratives in the East, to suppose that the destruction of life and property described, from the *Times of India*, as an unprecedented catastrophe in Sind, in the next paragraph of NATURE, was occasioned by an unusual fall of meteorites. In the absence of



HEIGHTS OF TWENTY SHOOTING-STARS DOUBLY OBSERVED AT EIGHT BRITISH ASSOCIATION STATIONS IN ENGLAND ON THE NIGHTS OF THE 9TH TO 12TH OF AUGUST, 1871.

any evidence that a loud report, and other aërolitic phenomena perceived at a great distance, accompanied the occurrence, its unusually disastrous effects may rather, doubtless, be ascribed to devastations produced by lightning of extraordinary violence.

On the accompanying diagram the real heights of some shooting stars are represented which were simultaneously recorded by observers of the annual meteor-shower in August last, at eight British Association stations in England. A. S. HERSCHEL

Newcastle College of Physical Science, Oct. 16

Exogenous Structure in Coal-Plants

PROF. WILLIAMSON criticises my want of certainty with respect to the exogenous mode of growth of extinct Lycopodiaceæ. But surely his reference to the Dixonfold trees does not prove more than that the diameter of their stems was greater near the roots than higher up. The same thing is true of many palms, but I think Prof. Williamson would be the last person to say that it was evidence of *their* being exogenous. Nevertheless, as I have already said, I am inclined to think that Prof. Williamson is right in supposing that the stems of extinct arborescent Lycopodiaceæ increased in thickness, although I do not see my way to asserting off hand that this was the case. Even admitting, with all Prof. Williamson's confidence, that it was so, I can see no classificatory value in the fact to justify overriding reproductive characters in his new classification.

I said in my former letter (and the argument still appears to me a good one) that this increase was in any case "nothing more than an adjustment to an arborescent habit dropped when the arborescent habit was lost." Prof. Williamson finds some difficulty in understanding this, and believes me to imply "that these exogenous conditions were merely adventitious growths assumed for a season and thrown off at the earliest opportunity; that they had no true affinity with the plants in which they were found." He confesses that he sees no ground for so remarkable a conclusion, and I may certainly say that as far as I comprehend it, neither do I.

What I did mean to imply was, that in comparing the stems of existing with those of extinct Lycopodiaceæ, allowance must be made for such adaptations of structure as would be likely to be correlated with enormous size. To make the matter clearer by an illustration:—Suppose we compare a nearly allied woody and herbaceous plant, say a lupin and a laburnum, we shall find in their stems (both "exogens") the same kind of differences as exist between the stem of a herbaceous *Selaginella* and that of the nearly allied arborescent *Lepidodendron*. The lupin may have had arborescent ancestors; if so, it has dropped all such adaptations of the structure of its stem to an arborescent habit as we find existing in laburnum. Assuming (what is of course *only* an assumption) that *Selaginella* is a descendant of *Lepidodendron* or its allies, the parsimony of nature has also suppressed in it all those peculiarities of stem structure which were merely correlated with vast size, and in *Selaginella* and recent *Lycopodiaceæ* we have the residuum. In *Isoetes*, which is only a few inches high, there is a kind of lingering reminiscence of circumferential growth.

Prof. Williamson says that "herbs if they belong to the exogenous group are as truly exogenous in their type as the most gigantic trees of the same class. Size has nothing to do with the matter." With these statements I altogether disagree. I look upon the terms exogen, endogen, and acrogen as altogether obsolete from a classificatory point of view. Mohl pointed this out more than twenty years ago. Compare the following remarks from one of his memoirs with Prof. Williamson's: "The course of the vascular bundles in the palm stem and in the one-year-old shoot of the dicotyledons is exactly similar, and the conception of a different mode of growth, and the division of plants into endogens and exogens formed on it is altogether opposed to nature."

Size, in fact, has everything to do with the matter. It is the persistent growth of the ends of the branches which makes the strengthening of the main stem by circumferential growth a mechanical necessity. Palms not being branched do not require the voluminous stem of an oak, and they exhibit on an enlarged scale only the structure of a one-year-old herbaceous shoot. But in the dragon-tree of Teneriffe an "endogen," which becomes extensively branched, there is a true circumferential growth of the main stem, which increases *pari passu* with the development of the branches. All herbaceous stems, on the contrary, among flowering plants, whether belonging to the exogenous or endogenous group, have practically the same type of structure. Where is the exogenous type in the stem of the common artichoke, or in *Ferula communis*, figured by De Candolle in his "Organographie Végétale," pl. 3, fig. 3, "pour montrer à quel point elle simule les tiges de monocotylèdones" (endogens)?

I think these remarks make it plain that circumferential (which is a preferable expression to exogenous) growth in stems is simply a necessary accompaniment of a branched arborescent habit. As far as the affinities of plants are concerned, it is purely accidental and of no classificatory value. *Lupinus* being herbaceous and *Laburnum* arborescent does not prevent their being placed in the same tribe of a natural family. Since Mohl has shown that one-year-old (herbaceous) stems conform to the endogenous type, while such woody stems as *Laburnum* possesses are of course exogenous, it is clear that Prof. Williamson's views would overthrow all the work of modern systematists, and bring us back, as I pointed out in my former letter, to the primitive division of plants into trees and herbs (not trees and *shrubs* as Prof. Williamson makes me say).

The interpretation of the actual structure of the stems of the extinct Lycopodiaceæ is of course another matter. Prof. Williamson illustrated his views at Edinburgh by referring to *Lepidodendron selaginoides;* every botanist who took part in the discussion, however, objected to his explanation. It may be true that this is only one form of such stems, but of course I can hardly be expected to be acquainted with the unpublished material which Prof. Williamson still has in hand. There is, I think myself, good reason for believing that *Lepidodendron*, *Sigillaria*, and *Ulodendron* all belong to a common type of stem structure; differences in fragments of different age of growth must be expected and allowed for. Of course, as I do not accept the existence of a pith in these plants, the pith or medullary rays must be rejected as well. Mr. Carruthers has shown, I think, conclusive reasons for disagreeing with Dr. Hooker with respect to the spaces which he identified with those structures. I was already familiar with the view of these stems taken by Prof. Williamson in his last paper. Those who are interested in the matter must judge for themselves who is right.