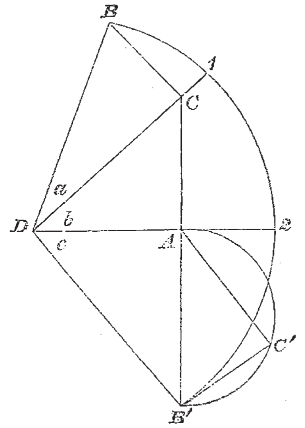
arc B1, which is in the same plane with it, will be at right angles to the arc 12. The third arc 2B′ will therefore be the hypothenuse of a right-angled spherical triangle, of which B1, 12, are the two sides. Calling these arcs or the angles of the faces resented by them, a, b, c, and the angles opposite to them in the spherical triangle, A, B, C, the proof of Napier's Rules, with this solid figure, proceeds by the same direct steps as those already described, with a special example of the figure in my former letter. As the construction there described is confined to the representation of a particular kind of right-angled spherical triangle, and is therefore inapplicable to illustrate the proof of



Napier's Rules experimentally in every given case, the general construction supplied by "J. J. W.," which is limited by no such restrictions, and which is at least equally convenient, will evidently serve more effectively the same practically useful and instructive purpose.

Instead of "accessible," as applied to the difficulties of the geometrical proofs produced by Mr. Cooley in his letter on "Elementary Geometry" (in NATURE, No. 103), which are indeed there obviously overcome, I would have used the word "surmountable" as more descriptive of geometrical difficulties, properly treated and discussed, had the word immediately presented itself to me; but having often found an easily executed model extremely useful and convenient in practical applications of Napier's Rules, with whose design, as a general resource to facilitate their study, I was not, however, so fully satisfied, I applied, perhaps unconsciously, to Mr. Cooley's demonstrations a term expressing strictly only the diffidence with which I ventured to present to readers of NATURE my own very imperfect geometrical contrivance. In thus making my difficulties accessible to "J. J. W.," I very gratefully acknowledge the assistance which I have derived from his remarks on my letter in NATURE, No. iii., and I cheerfully admit the merit and superiority of the general rule for constructing a proper model in cardboard, to illustrate the proofs of Napier's Rules, and to facilitate their study, which he has kindly consented to describe.

Newcastle-on-Tyne, Dec. 16 A. S. HERSCHEL

Alternation of Generations in Fungi

I AM sure that the Rev. M. J. Berkeley will exonerate me from any deliberate intention to misrepresent him; nor do I think that there is, after all, much difference of opinion between us regarding the present subject, unless, perhaps, that I am more sceptical. I alluded to the paper cited by him from the "Journal of the Horticultural Society," on propagation of bunt spores, and not to his communications on the hop or vine mildew. I was under the impression that he regarded the "four consecutive forms of reproductive cells in the bunt" as an instance of alternation of generations. On reference to the original paper, I find that he did not go so far then as to indicate four consecutive forms of reproductive cells; but that Tulasne followed on his track in 1854, and in 1857 Mr. Berkeley seemed to have accepted the results of Tulasne's observations, since, in his "Introduction," he gives figures at page 318, in the description of which the following phrases occur:—"spores of the second order," "spores of the third order," "spores of the fourth order." Here are the "four consecutive forms of reproductive cells" to which I alluded. At page 321 he writes concerning the bunt:—"The spores, however, are not immediate means of propagation; they are, in fact, only a sort of prothallus, from which the mycelium grows, producing at the tips, or on lateral branchlets, bodies of various forms, which are themselves capable of germination, and immediately reproduce the species." The real issue between us seems to lie in the phrase, "alternation of generations." If the bunt spores, on germination, produce fusiform bodies, which, after conjugation, produce short cylindrical spores, and thus intermediate reproductive cells unlike the parent cell come between that and the ultimate reproduction of the species, I am induced to call it an "alternation of generations." It would be waste of time to discuss phrases, or I might take exception to the application of this phrase to the *Erysiphei*. The conidia and pycnidia of the hop mildew may be developed without sporangial conceptacles, and the parasite reproduced without sporangial fruit, but I cannot recognise alternation of generations in the reproduction of a species by means of conidia, stylospores, or sporidia, or by one of these alone. If such may be construed into an alternation of generations, it must be by permitting greater elasticity to the phrase. Conidia germinating and producing pycnidia, the stylospores of the pycnidia germinating and producing sporangial conceptacles, containing the sporidia which, upon germination, will produce the mycelium and conidia again, returning to the original form after two or three consecutive departures from it, appears to me a perfect type of alternation of generations. I fully admit that "if it is once established that a Puccinia produces an Æcidium, or an Æcidium a Puccinia, we should have a clear case, especially when the third form reverts to the first again." Without the slightest desire to "depreciate the labours of Oersted and De Bary," I cannot admit that they have established facts until their observations are confirmed, especially when there is an evident possibility of their having been deceived. I shall have no hesitation in accepting the facts when they are confirmed by independent and equally trustworthy observers, although I may be unable to account for some of the phenomena. At present I must confess that I am not so sanguine as Mr. Berkeley appears to be.

The correspondent signing himself "Mycelium" wishes to know if "the liability to produce parasitic fungi is communicated from the seed to the mature plant." In some instances we know such to be the case, in others perhaps only suspect it. The "bunt" is an instance, or why the steeping of seed corn? or how did the Rev. M. J. Berkeley succeed in producing bunted wheat plants from seed corn inoculated with bunt spores? Two or three years since I published particulars of a similar instance of celery seed and *Puccinia Apii*. It would be as rash to affirm that this is always the case as to deny its occurring at all.

M. C. COOKE

In Re Fungi

THE letters in your last two numbers have reminded me how ill this subject is studied by some botanists in this country. I will give two recent instances: 1. In the last number of the *Journal of Botany*, p. 383, it is positively stated that *Agaricus cartilagineus* (a rare and very critical species by the way) was determined by a growth which is there described a mere *mass of mycelium*. He must have been a bold man who ventured to name an agaric (above all things) from a *mass of mycelium*. 2. In the first number, October 1871, of the *new edition* of "Paxton's Botanical Dictionary"—"enlarged and revised"—under the article *Agaricus* there is to be found such a collection of obsolete names and absurd errors as to make the article simply ridiculous.

W. G. S.

Mr. Lowne and Darwinian Difficulties

MR. LOWNE (NATURE, December 7) sees no difficulty whatever in explaining by what natural process an insect with a suctorial mouth is developed from one having the mandibular type of mouth, but still he does not explain. He affirms there is no doubt that "the pupa state is a modification (!) of the ordinary process of skin shedding," and that this is "proved" by so many facts that he cannot understand how it could be "denied," &c., but he does not prove it.

For aught I can tell, every internal tissue and every external scale of the butterfly may be represented in the larva; but I do not know and cannot prove that this is so, nor do I believe any one can prove it. That the changes which take place during the pupa state are very different from those that occur during any portion of the larva period, will be admitted by every one who