is to be hoped that some English lithographic printer will see the American triumph in this particular, and will forthwith mend his ways. P. M. D.

LETTERS TO THE EDITOR

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

Alternation of Generations in Fungi

In Mr. Cooke's article on this subject, it is stated that I have shown that there are at least four consecutive forms of reproductive cells in the bunt (*Tilletia caries*). I imagine that by a slip of the pen he must have substituted this for hop mildew; but, be this as it may, what I really did say at a time (1847) when the formation of secondary fruit was not ascertained in Ustilago, Puccinia, and allied parasites, was as follows, after describing the curious anastomosing threads which are produced on the germinating processes of the bunt spores:—"I was at first inclined to think that it had something to do with the reproduction of the bunt, and it is quite possible that in plants, as well as in the lower animals, there may be an alternation of generations. This is, however, merely thrown out as a hint which may be followed out by those who have fewer avocations than myself. Many anomalous appearances, amongst Algæ especially, seem to indicate something of the kind." * This growth can only be regarded as an intermediate state, which is probably necessary for the propagation of the parasite, and the same must be said of other cases in which the anomalous form does not produce organisms similar to itself. In such cases as the hop and vine mildew, the Oidium forms may be propagated almost indefinitely with only an occasional production of another form, and this, perhaps, may safely be regarded as an alternation of generations, while mere conidia-bearing forms can scarcely be so regarded. In such cases as that of the Uredos, which accompany or precede Puccinia, though both are fertile, we can scarcely recognise such an alternation; but if it is once established that a Puccinia produces an Æcidium, or an Æcidium a Puccinia, we should have a clear case. The usual argument about wheat being subject to mildew where there are no berberry plants, or Rœstelia where there are no savines, does not seem to me to be good. It appears quite clear that wheat mildew may be produced, either from the germination of *U. rubigo vera*, or from its own secondary spores, and that almost indefinitely, where there is no berberry; but this does not show that the spores of Puccinia, when sown on the berberry leaf, may not produce the Æcidium, or the spores of the Æcidium the mildew. I quite agree with Mr. Cooke, that the observations of Oersted and De Bary are not absolutely conclusive, though I may be inclined to give them more weight than he does. The observations should certainly be repeated; but, if the results should be the same, I should certainly feel inclined to accede to their views, indisposed as I always am either to jump hastily to conclusions myself, or to accede at once to the crude observations of others. M. J. Berkeley

Whether Mr. Cooke has sufficiently appreciated the labours of De Bary and Oersted, in his article published in your columns of last week under the above title, I leave for others to determine. I wish now merely to call attention to one sentence in his article, as follows:—"It is manifest that no amount of care in cultivation, under bell glasses or other exclusion from foreign influences, is sufficient against a contingency *which dates back to the seed of the nurse-plant.*" Does Mr. Cooke mean that the spores of the fungi themselves deposited in the seed of the nurse-plant are carried up, so to speak, in the process of growth, into the leaves, where they germinate; or that the *liability* to produce parasitic fungi is communicated from the seed to the mature plant by some process which combines the Pangenesis of Darwin with the spontaneous generation of Bastian? I see no other explanation of the sentence than one or other of these alternatives.

Mycelium

Leibnitz and the Calculus

Prof. Tait need not wonder if an attack that is "totally unexpected" should seem "appallingly sudden." In the absence of a statute of limitations restricting to two years and a half the right to take up a gage, there can be no reason why an attack should not be made, save its personal bearings; and the circumstances of the challenge might be cited in bar of any exception taken on that ground. I thank the Professor for his explanations. I could not have guessed that under cover of his challenge to produce a metaphysician who was also a mathematician, lurked the assumptions, that every mathematician was a metaphysician, and that every metaphysician was either a mathematician or (in the old sense) a physician. Well, he has a perfect right, for his own private convenience or pleasure, to identify two names which he had from the first asserted to be eternally distinct. Accepting his classification, then, for the sake of argument—certainly not for fruitless controversy—to wit, that everyone is either a mathematician or a non-mathematician, and that every *true* metaphysician must be either mathematician or physician (Faraday did not hate the term "physicist" worse than I do) we are confronted with some surprising results. Leibnitz, the author of the *Monadologie* and the *Théodicée*, works that are known to contain the germs of the *Kritik der reinen Vernunft*, was a *spurious* metaphysician. Why, in the name of common sense? "Because," says Prof. Tait, "he was a non-mathematician; there is no medium, you know; he must have been either a non-mathematician or a mathematician, and a mathematician he was *not*." What! Leibnitz not a mathematician? "Not a bit of it," says Prof. Tait; "for he was, I fear, simply a thief as regards mathematics, and in physics he did not allow the truth of Newton's discoveries." I do not object to the Professor calling a spade a spade; but I assure him that this charge is made just twenty years too late. It is exactly that time since the last vestige of presumption against the fair fame of the great German was obliterated. If Prof. Tait does not understand me, or, understanding me, disputes the unqualified truth of my statement, I promise to be more explicit in a future letter. But I incline to think the question is not susceptible of *proof* until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do the simple act of justice and reparation required of them, viz., publish the letters and papers relating to this controversy, which since that date have slumbered in the secret archives. I advise Prof. Tait to utilise the meantime by reconsidering some of his utterances on the *Principia*, lib. ii. lem. 2.

It appears, too, that Descartes, notwithstanding his physics, which are very sad, *was* a mathematician, and therefore a *true* metaphysician, and this, I suppose, despite the *spurious* metaphysics of his *Discours* and his *Méditations*. By the way, when Prof. Tait parenthetically and admiratively corrects me for calling him *Cartes*, he surely overlooked the fact that *Cartes* is his English name, the name by which he was known to the readers of Dr. Samuel Clarke, &c., and is therefore preferable to the dog-latin alternative.

Such, then are some of the surprising results of adopting Pro Tait's classification of mathematicians and metaphysicians. But he objects to my classification of the former, that the greatest mathematicians of our own day—among which Prof. Tait will allow me to count himself—would fall into my second class, since they are not inventors of a calculus, and yet they are not mere experts. Among the names he adduces are Cayley and Sylvester, the co-inventors of a new calculus, viz., that which has been so fertile in its application to Linear Transformations; I mean, of course, the Higher Algebra. Accordingly, both would, of course, fall into my first class; and I will add, that I should assuredly think that "something is rotten in the state of Denmark" if I found the true mathematical ποιητής had ever contented himself with the improvement and application of other men's productions. C. M. Ingleby

Highgate, Dec. 4

The Science and Art Department

I have been expecting, but in vain, to see Mr. Uhlgren's reply to the request made to him a few weeks since, to produce the Department's letter of which he spoke, and in which it was stated that the rumoured reduction of the number of certificates awarded had actually taken place through the examination papers having been returned for revision. I quite agree with your correspondent who challenged its production, that such a document ought to be made widely known if it exists; whereas if Mr. Uhlgren's statement is founded on any misapprehension, it ought to be corrected without delay.

If such a statement were unfounded, such complaints as those Mr. U. made are, I think, more likely to damage the cause of

* "Journal of Horticultural Society of London," vol. ii. p. 112.