whilst the upper is formed of a confused mass of matter which belongs chiefly to the third element, but is interspersed with the round balls of the second. The passages in the intermediate region he conceives to be so grooved that the fluted columns entering from one side cannot return again by the same passages, but when opposed in their straight course are forced back through the air or upper portions of the earth to those openings by which they entered, whilst those from the other side make similar circuits. He considers that magnets contain passages the same as those first mentioned, and such is the inclination of the fluted columns to enter these passages, that even if the poles are not turned to receive them they will push aside all opposing particles, till, if not restrained by still stronger bodies, the magnets are forced to assume those positions in which their poles point oppositely to those of the earth.[1] Such is the hypothesis of Descartes, ingenious rather than plausible, and interesting chiefly as exhibiting the speculative mind of its author.

In 1683 the celebrated Halley presented a paper of great importance to the Royal Society of London, entitled "A Theory of the Variation of the Magnetical Compass." In this communication he states that the "deflection of the magnetical needle from the true meridian is of that great concernment in the art of navigation, that the neglect thereof does little less than render useless one of the noblest inventions mankind ever yet attained to," and gives as the result of "many close thoughts" the following explanation of the variation of the compass. "The whole globe of the earth is one great magnet, having four magnetical poles or points of attraction, near each pole of the equator two; and in those parts of the world which lie near adjacent to any one of those magnetical poles, the needle is governed thereby, the nearest pole being always predominant over the more remote." He remarks that the positions of these poles cannot as yet be exactly determined from want of sufficient data, but conjectures that the magnetic pole which principally governs the variations in Europe, Tartary, and the North Sea is about 7° from the north pole of the earth, and in the meridian of the Land's End, whilst the magnetic pole which influences the needle in North America, and in the Atlantic and Pacific Oceans, from the Azores westward to Japan, is 15° from the north pole, and in a meridian passing through the middle of California. The variation in the south of Africa, in Arabia, Persia, India, and from the Cape of Good Hope, over the Indian Ocean to the middle of the South Pacific, is ruled by the most powerful of all these magnetic poles, which is situated 20° from the south pole of the earth, and in a meridian passing through the island of Celebes; in the remainder of the South Pacific Ocean, in South America and the greater part of the South Atlantic Ocean, it is governed by a magnetic pole 16° from the south pole, in a meridian 20° west of the Straits of Magellan. On this hypothesis Halley explains the variation observed in different places, and among others cites the two following instances. On the coast of America, about Virginia, New England, and Newfoundland, the variation was found to be west, being above 20° in Newfoundland, 30° in Hudson Strait, and 57° in Baffin's Bay. On the coast of Brazil, on the contrary, it was found to be east, being 12° at Cape Frio, and increasing to 20½° at the Rio de la Plata, thence decreasing towards the Straits of Magellan. Thus, almost in the same geographical meridian, we find the needle at one place pointing nearly 30° west, at another 20½° east; this is explained by the north end of the needle in Hudson Strait being chiefly attracted by the North American magnetic pole, whilst at the mouth of the Rio de la Plata the south end is attracted by the south magnetic pole, situated west of the Straits of Magellan.

[1] Descartes designates the south pole of the magnet that which turns to the north pole of the earth, and the north pole of the magnet that which turns to the south pole of the earth.

Sailing north-west from St. Helena to the equator, the variation is always in the same direction, and slightly east. Here the South American is the chief governing pole, but its power is opposed by the attraction of the North American and Asian south poles; the balance as you recede from the latter being maintained by approach to the former.

Nine years later Halley made another communication to the Royal Soceity, in which he endeavoured to meet two difficulties he had always felt in his former explanation; one, that no magnet he had ever seen or heard of had more than two opposite poles; the other, that these poles were not, at least all of them, fixed in the earth, but slowly changed their positions. The following observations are cited by Halley in proof of the motion of the magnetic system. At London, in 1580, the variation was 11° 15′, east; in 1622 it was 6° east, in 1634 it was 4° 5′ east, and in 1657 there was no variation; whilst in 1672, it was 2° 30′ west; and in 1692, 6° west. At Paris the variation was 8° or 9° east in 1550, 3° east in 1640, 0° in 1666, and 2° 30′ west in 1681. At Cape Comorin it was 14° 20′ west in 1620, 8° 48′ west in 1680, and 7° 30′ west in 1688. Halley considered the external parts of our earth as a shell, separated by a fluid medium from a nucleus or inner globe, which had its centre of gravity fixed and immovable in the common centre of the earth, but which rotated round its axis a little slower than the superficial portions of the earth. The nucleus and exterior shell he regarded as two distinct magnets, having magnetic poles not coincident with the geographical poles of the earth. The change observed in Hudson's Bay being much less than that observed in Europe, Halley concluded that the North American pole was fixed, while the European one was movable; and, from a similar observation on the coast of Java, he considered the Asian south pole as fixed, and the pole west of the Straits of Magellan to be in motion. The fixed poles he regarded as those of the external shell, and the movable those of the inner nucleus. Of these latter, the one placed by him in the meridian of the Land's End was ascertained, in the present century, to have moved to Siberia, in 120° east long., and that placed by him 20° from the Straits of Magellan to have moved between 30° and 40° west of this position; while those poles regarded by Halley as fixed were found but slightly altered in position since his time. It is extremely interesting to find that not only modern observations of declination, but also those of dip and magnetic intensity, have received their best explanation on the assumption of four magnetic poles. Much, however, that is mysterious remains unsolved, and Halley's remarkable words may even now with truth be quoted: "Whether these magnetical poles move altogether with one motion or with several; whether equally or unequally; whether circular or libratory; if circular about what centre, if libratory after what manner, are secrets as yet utterly unknown to mankind, and are reserved for the industry of future ages."

K.

THE POTATO DISEASE

IN the *Journal of the Royal Agricultural Society of England*, Second Series, vol. xii., Part I., No. xxiii., 1876, Prof. A. De Bary of the University of Strasburg has published a paper entitled "Researches into the Nature of the Potato Fungus."

De Bary's essay treats of the Peronosporeæ, Artotrogus (in its plain and echinulate forms) and Pythium. These fungi are described by De Bary as four distinct plants, whilst I, in common with several other observers, believe the first three (if indeed not all four) to be mere conditions of one and the same fungus, viz., the *Peronospora infestans* of Dr. Montagne. In replying to De Bary's remarks it will be convenient (especially as the potato-fungus appears to be somewhat imperfectly understood), to refer to each of the above forms separately, and to illustrate each to an uniform scale. Therefore in the present paper I will confine myself to the description of the two forms of Artotrogus, and leave the consideration of Peronospora and Pythium with De Bary's criticisms of my observations for another paper.

I.—ARTOTROGUS, *Mont.*

To make my description of Artotrogus quite plain, it is necessary to briefly recapitulate the early history of the potato-fungus. Mdlle. Libert was one of the first to describe this in 1844. In the same year Mr. Berkeley writes he first saw diseased potatoes.

In 1845 Dr. Montagne described and illustrated the potato fungus. In his illustrations he included certain spherical bodies found in spent potatoes by Dr. Rayer; these bodies were attached to threads, some of the bodies being terminal (Fig. 1, A), and others within the threads

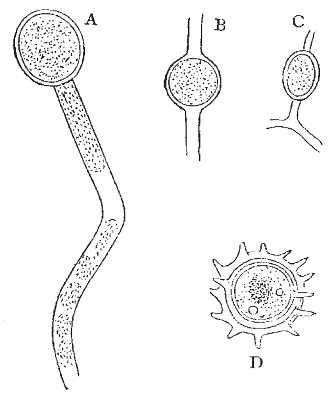

FIG. 1.—*Artotrogus hydnosporus*, Mont. A, B, C. Oogonia from Montagne's original camera-lucida drawing. D. Echinulate oogonium from De Bary ("Researches," p. 256, Fig. 3), × 400 dia.

(Fig. 1, BC), just as true oogonia are now known to occur in Cystopus and other of the Peronosporeæ. A third form (Fig. 1, D), was more or less echinulate, and this was assumed to be the mature spore. Dr. Montagne, in 1845, did not thoroughly comprehend the meaning of these bodies, so he named them provisionally Artotrogus.

From 1845 till 1875, when I rediscovered the entire series of these bodies in direct connection with *Peronospora infestans*, in the Chiswick tubers, no record of their rediscovery had ever been published. Botanists had sought for Artotrogus in vain.

In 1846, in the first vol. of the *Journal of the Royal Horticultural Society*, the Rev. M. J. Berkeley published his famous paper on the potato-murrain (the essay is dated Nov. 22, 1845), and in this paper Mr. Berkeley reproduces the description and illustration of Montagne's Artotrogus. Mr. Berkeley's published belief has for many years been that the spherical and echinulate forms of Artotrogus belong to no other than the secondary condition of the potato fungus.

In 1849, Mr. C. Edmund Broome, of Batheaston, discovered a second species of Artotrogus; this was found in decayed turnip. A copy of the original camera-lucida drawing made by Montagne is here reproduced. It shows the mycelial threads and the mature resting-spore at E.

Fourteen years afterwards (1863), Prof. A. De Bary published a paper on the development of parasitic fungi in the *Annales des Sciences Naturelles*, vol. xx. In this paper the author illustrated, amongst other things, the resting-spore of *Peronospora parasitica*, Corda, a plant common upon turnips, &c. Part of De Bary's illustration is here reproduced to show the probability of the second described species of Artotrogus being identical with the Peronospora. Some allowance must of course be made for the sketch of the turnip Artotrogus (Fig. 2), as it was made in 1849 when resting-spores were little understood, but it is clear that Mr. Broome detected

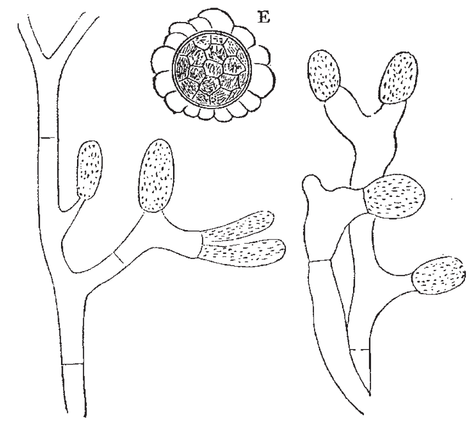

FIG. 2.—Artotrogus on decayed turnip = *Peronospora parasitica*, Corda. × 400 dia.

not only the resting-spore with its collapsed oogonium, but probably also a group of antheridia (see the detached antheridium in De Bary's illustration, Fig. 3).

This probable identity of the Artotrogus and Peronospora of the turnip, points in the direction of the probable correctness of Mr. Berkeley's views as to the Artotrogus of the potato. The oogonia of the turnip parasite are similar in size and form with the oogonia found in potatoes, and to these latter I shall now return.

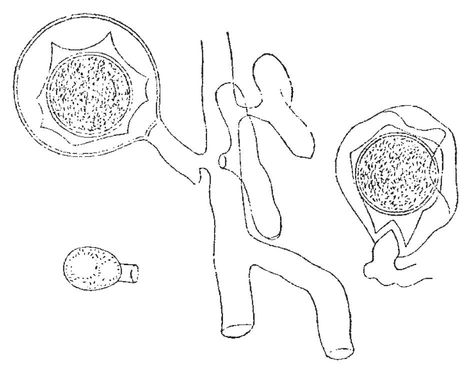

FIG. 3.—*Peronospora parasitica*, Corda. Resting-spores and detached antheridium (De Bary, "Ann. des Sc. Nat.," 4th series, vol. xx., Pl. X., Fig. 5–7), × 400 dia.

The bodies found by me in the Chiswick tubers I have from the first identified with Montagne's Artotrogus, though De Bary in his criticisms has found it convenient to omit all mention of this fact. But De Bary himself now illustrates the same bodies "from Montagne's original specimen;" this illustration is here reproduced (Fig. 4), and I can certify as to its general correctness with the reservations (1) that the oogonia shown are larger than any I have seen in the "original specimens;" (2) larger than any Montagne has figured; and (3) that De Bary has omitted all the terminal oogonia. In Montagne's original camera lucida drawing there are three terminal oogonia, one is reproduced at A (Fig. 1), with two similar bodies intercalated at B C.

De Bary (as well as Montagne, Berkeley, and myself) has also met with the more or less echinulate bodies, and one of De Bary's illustrations is reproduced, at D (Fig. 1). I agree as to its general accuracy.

Now whilst De Bary contends that all these forms are distinct species of fungi, and not belonging to the potato fungus, I maintain with Berkeley, and other competent observers, that they are *one, including Peronospora.* I have seen them all growing from the same threads, and I will now review the grounds on which De Bary refers them to *different* fungi.

First, from De Bary's own words, I will show how extremely near the connection was, even with him. The italics are mine. He says ("Researches," p. 256), "In the tissues of potatoes *penetrated with the mycelium of Phytophthora* (De Bary's new name for the potato fungus) there sometimes appear other bodies which might be regarded as oogonia or oospores of the potato fungus. I have several times found them with Pythium vexans in old collapsed tubers which had sprouted in the ground, *and once without Pythium* in a living stalk which

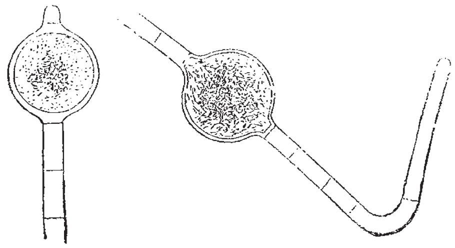

FIG. 4.—*Artotrogus hydnosporus*, Mont. De Bary's illustration ("Researches," p. 258, Fig. 8), × 400 dia.

had been on the ground. But they were *always restricted to those regions which were occupied by the Phytophthora mycelium.*" And again, "It was certainly remarkable that they were *often* situated *close* to the inner surface of the cell-walls in places where externally the mycelium of Phytophthora *undoubtedly ran in the intercellular spaces*, or even where a short branch of it *penetrated the interior of the cell.*"

In his desire, however, to dissociate these bodies from Peronospora, De Bary (p. 257) first says they were found by Montagne in a "sprouted but *not diseased potato.*" But this statement becomes of no value when De Bary himself confesses (as he does) that with him the oogonia were always restricted to those regions which "were occupied" by the *Peronospora mycelium.* I, too, have found them in similar "regions" and *upon* the *Peronospora* mycelium. De Bary says (p. 256), "In most cases I found these bodies complete, mature, and without any distinct indication of their being attached to mycelium," and he says the same of Montagne's material and afterwards of mine. But this statement (like the former one) becomes of no value when we find the author writing on the very next page that after "long searching in vain he found them to grow on the extremities of the branches of a mycelium which is very like that of *Pythium vexans.*" And now what is the mycelium in *P. vexans* like? De Bary tells us, on page 253, that it is "scarcely possible to draw a positive distinction" between it and the mycelium of *Peronospora infestans!* The conclusion is obvious.

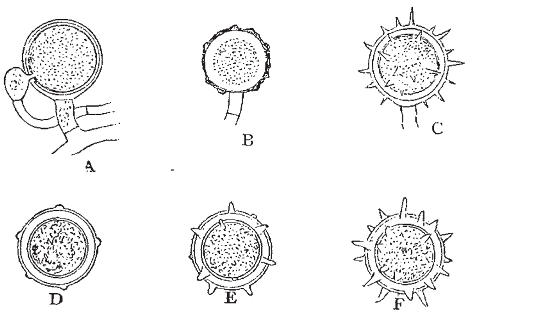

FIG. 5.—Oogonia of *Peronospora infestans*, Mont. A, B, C. Different forms from Chiswick potatoes. D. From De Bary's slide, No. IX. (common in Montagne's preparations). E. From De Bary's slide, No. XI. F. From De Bary's slide, No. X., as sent to the Royal Agricultural Society.

It is, perhaps, difficult to explain why a smooth oospore should become a rough or echinulate one, but the fact remains that the phenomenon is perfectly well known in fungi, notably in the spores of some of the Gasteromycetes. Plain and echinulate oospores are also produced on the same plant in some Saprolegnieæ. Max Cornu also maintains that *Saprolegnia asterophora* of De Bary is the same as the warted form of *Achlya racemosa* of Hildebrand. Dictyuchus also occurs with warted oogons. The three upper figures on the accompanying illustration (Fig. 5) are exact reproductions from different forms of oogonia found by me growing on the same threads with *Peronospora infestans.* The lower three are from De Bary's own specimens sent to the Royal Agricultural Society; D is from slide ix. (this is also very common on Montagne's preparations); E is from De Bary's slide xi., and F is from slide x. A glance at the actual preparations will show that every intermediate form is to be found.

But De Bary will not see the association, and in his concluding paragraph on Artotrogus (p. 258), he criticises Montagne and Berkeley, and says these authors "have explained the globular cells of both kinds as exhibiting progressive steps in their development, the smooth ones being the younger. For this *no reason is given,*" says De Bary, "nor have I found any in the renewed examination of the specimens. Anyone can scarcely conceive, from the known phenomena of development how the smooth

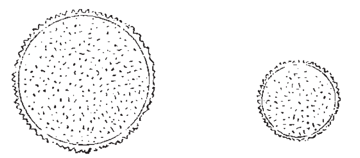

FIG. 6.—Resting-spores of *Peronospora Holostei*, Casparry. × 400 dia. From De Bary's preparation sent to the Royal Agricultural Society.

thick walled cells (the cell-walls are exactly the same in both) could become the smaller star-shaped ones."

Now, the accompanying illustrations (especially Fig. 1) show De Bary to be quite wrong. The three upper oogonia on Fig. 1 are from Montagne's own camera-lucida tracing, the left-hand figure, A, is the *largest* oogonium he has shown, and the right hand oogonium, B, is the smallest. I can testify as to their correctness from an examination of the original material. The starry figure at D is one of the (presumedly) mature bodies, and is reproduced from *De Bary's own figure* ("Researches," p. 256, Fig. 3). I reproduce this figure because De Bary calls it the "common form." Now, instead of being smaller, a glance will show this echinulate body to be *larger than the largest plain oogonium found by Montagne.* On turning now to Fig. 5 it will be seen there is no "getting smaller" in the case, for all the bodies are as a rule very uniform in size, and if they vary at all they get somewhat larger instead of smaller at maturity. This is the rule, but all botanists know well that the oogonia of the Peronosporeæ are liable to vary. A good illustration of this is afforded by De Bary's own preparation of *P. Holostei*, as furnished by him to the Royal Agricultural Society. The accompanying illustration (Fig. 6) is a camera-lucida reproduction of two oogonia belonging to this species from his own slide, and it shows well how oogonia may vary in size.

Thus De Bary's notes and criticisms (taken in connection with the observations of other competent observers) on this, *the first point* (Artotrogus), completely fall to the ground. De Bary confesses to having found both forms of Artotrogus in those restricted regions *only* of diseased potatoes where *Peronospora mycelium* was *undoubtedly* and *always* present. He moreover found the bodies attached to a mycelium so like that of Peronospora that it was almost impossible to distinguish it from Peronospora.

As regards the unfortunate criticism of Montagne and Berkeley as to the size of the oospores De Bary effectually refutes himself. WORTHINGTON G. SMITH

SCIENCE IN LEEDS

THE Annual Meeting of donors and subscribers to the Yorkshire College of Science was held in Leeds last Friday. Financially the College seems to be fairly prosperous. The subscriptions promised prior to the inauguration amounted to 28,000*l.*, and of the special fund of 10,000*l.* started by Sir Andrew Fairbairn's conditional offer of a second donation of 1,000*l.*, about 8,000*l.* have been raised. The Council are most anxious that this should be completed without delay. The College will also participate yearly in the proceeds of William Akroyd's Foundation. Of the present condition of the Yorkshire College as regards efficiency, the following communication from Mr. G. T. Bettany will afford a fair idea:—

A recent visit to Leeds enabled me to inquire into the working of the Yorkshire College of Science, which has now been in existence for nearly two years. In the building which has been temporarily adapted to the purposes of the College, I found abundant evidence of labour and study on the part of both professors and students. The attendance of seventy-five day and more than two hundred afternoon and evening students during the present session shows that the advantages offered by the College are becoming widely appreciated. Although youths are admitted at the age of fourteen, most of the students are much older; I was informed that the average age was a year and a half greater than at Owens College. The chemical department has the lion's share of accommodation; the lecture-room is large and good, and the laboratory would allow of forty students working at the same time. There have been few vacant benches during the past term. Prof. Thorpe has a room fitted up as a museum and reference library, and has also a private laboratory. A considerable amount of work is done in the department of mathematics and physics, but physical teaching suffers from want of space. Practical work can only be carried on in Prof. Rücker's private room. Geology has been fairly attended, though the day class is but small at present. Prof. Green is forming most instructive series of rock-specimens, illustrating stratigraphical geology, volcanic phenomena, and transitions in metamorphism. Mr. Miall's biological lectures have resulted in some very good work. He has prepared a large number of dissections for demonstration, including a series illustrating Prof. Rolleston's "Forms of Animal Life;" practical work has been undertaken by several students, including ladies, one of whom gained the highest place in an examination at the end of last term. Finally, the instruction on textile industries, under Mr. Beaumont, has been made scientific in many respects, especially in relation to the theory of colouring.

It can hardly be considered a misfortune that the College has been started in temporary buildings; for by means of its present effort science will become more widely appreciated, and much larger donations will come to hand than those already received; and the construction of the permanent college buildings cannot fail to be advantaged by the experience now being gained by the professors. It is to be hoped that many wealthy Yorkshire manufacturers who have at present given little or nothing to the College will be induced to follow the example of men in other localities, and liberally support a system of teaching which will be of great intellectual and material benefit to Yorkshire. I was struck with the large amount of work undertaken by the professors. When more prosperous times come, it will be for the good of the College not to exact so much work from them as their zeal is now leading them to perform.

The Leeds Philosophical Museum is becoming yet more interesting under the care of Mr. Miall, who has worthily succeeded the late Mr. Denny. The whole of the Museum is gradually being arranged in the most educative manner, and very great progress has been made. The casual visitor cannot fail to be instructed as well as interested, which can hardly be said of many more pretentious museums. Brief and clear printed descriptions or explanations abound, showing the particular interest of a specimen, or giving the general characters of a class of animals or of a geological formation. If an additional skilled curator could be appointed, who should relieve Mr. Miall from the care of several departments, the Leeds Museum would advance still more rapidly than at present, and would soon be worthy of any provincial college.

THE LATE SIR WILLIAM WILDE

SIR WILLIAM ROBERT WILLS WILDE, M.D., &c., was born in Castlerea, county of Roscommon in Ireland, in the year 1814, and he died at his residence in Dublin on the 19th instant. He was educated at the Royal School at Banagher and at the Diocesan School at Elphin; when scarcely eighteen years of age he was bound apprentice, according to the practice of those days, to the well-known surgeon, Abraham Colles, and he acquired his professional knowledge from such men as the Cramptons, Marsh, Wilmot, and Cusack. Early in 1837 he became a Licentiate of the Royal College of Surgeons in Ireland, and shortly after he resolved to devote himself to ophthalmic surgery, in which he attained a position of the highest eminence.

However distinguished as an oculist, however renowned as a writer on statistics, in these columns we lament in his decease the departure from among us of one who, as an earnest, devoted, and painstaking student of the early history of the Irish races, has left in his writings on this subject a great and an enduring monument.

Sir W. Wilde was elected a member of the Royal Irish Academy in June, 1839, having previously read two papers before the Academy, which were published in abstract in their *Proceedings*, and exhibited a collection of ancient spear-heads found in his native country. At this time the Academy had no museum (the Underwood purchase was not arranged), but in the same month that Wilde was elected, Prof. M'Cullagh munificently presented them with the Cross of Cong, "in order that he might contribute to the formation of a national collection, the want of which was regarded by Sir Walter Scott as a disgrace to a country which, like Ireland, so abounded in valuable remains." This noble gift bore speedy fruit, and meeting after meeting witnessed the presentation of donations, many of which were from time to time described by Wilde.

In 1855 Wilde was elected member of the Council of the Academy, and Secretary of Foreign Correspondence in 1857. In 1852 the Academy had moved to the premises that they at present occupy, and the Council took steps to have a catalogue of their museum made. The task was entrusted to Dr. Petrie. The resolution of the Council would seem not to have been carried into effect, and after some years of anxiety the Council and the Academy were but too happy, in March, 1857, to accept Wilde's liberal proposal to arrange and catalogue their museum. The energy that he brought to bear on this task may be judged from the fact that Part I. was ready in the month of August in the same year, when the British Association met for a second time in Dublin. Part II. was published in 1860. Part III., concluding Vol. I., in 1863. Part I. of Vol. II. had been published in the previous year. Part II. of this volume, although in great part ready, was never printed; let us add that the best