said M. Tisserand, "and gave it a permanent place. Before him, astronomers concerned themselves chiefly with the movements of stars and members of our planetary system, seeking to explain them in their minutest details by the law of gravitation. Arago studied the nature of heavenly bodies, and the character of the phenomena continually exhibited by them. The polariscope showed him that the glaring surface of the sun is gaseous, and gave him important information as to the light of comets. Another application of physical methods furnished him with a precise means for measuring the diameters of planets or determining their magnitude. Nothing is more ingenious than his explanation of the scintillation of stars, founded upon the remarkable properties Fresnel found to be possessed by rays of light. Arago ought truly to be considered as the founder of a branch of astronomy—physical astronomy—that has since been remarkably extended, for it was he who pointed out the importance that would accrue from the application of photography to the study of celestial bodies. He was not able to foresee the day, however, when chemistry would enter into the domain of astronomy, and we should be able to discover their constitution; spectrum analysis has only been discovered, in fact, since the death of Arago."

"An example will give an idea of the perspicacity of Arago. It is generally known that about the end of last century France took the initiative of the metrical system and made it an international thing by connecting the metre with the size of the earth. But our globe is cooling and contracts, little by little, in the course of centuries, so that the unit of length is rendered liable to slight changes. Arago thought that a minute study of the light-rays that come to us from the sun and stars might furnish a rigorously constant unit of length, connected not with the earth, but with the stars—a sidereal metre of some kind. Well, this beautiful idea was realised a few months ago by Mr. Michelson, at the American Bureau of Weights and Measures." M. Tisserand also dwelt upon the influence that Arago exercised upon his pupils and the comprehensive character of his literary works. M. Cornu followed with an account of Arago's investigations in experimental physics, and after stating his work in connection with the *experimentum crucis* of the emission and wave theories of light, said, "If we come to terrestrial physics, meteorology, or industrial applications of steam and electricity, we always find Arago in the front rank with new ideas. Of an indefatigable activity, in science as in government, he was present with all the resources of his powerful spirit, with the ardour of his generous heart, especially where there was a great work to direct, a just cause to defend, a social evil to fight, and, at the call of duty, a peril to face."

The character of the statue, which is in bronze, is shown in the accompanying illustration from *La Nature*. Arago has his face turned towards the observatory. The pedestal on which the figure stands is also in bronze, and bears the simple inscription "FRANÇOIS ARAGO, 1786–1853. SOUSCRIPTION NATIONALE." Men of science throughout the world respect the name, and their French *confrères* revere it. Those who have done homage to the man by thus assisting to perpetuate his memory are themselves honoured in the act.

MODERN MYCOLOGY.[1]

IT is not often that a great and industrious investigator lives to see his chief work so far completed as Prof. Brefeld has done; and still more rarely to find an enthusiastic exponent of all his views so willing and so capable of putting them before the public as Dr. von Tavel here proves himself to be.

It is hardly thirty years ago since the late Prof. de Bary of Strasburg showed that the study of the fungi, up to that time a chaos of statements in which the student usually lost himself hopelessly, was capable of being made not only a very scientific and important branch of Botany, but also a very interesting one, and that there were already workers in the field—especially the Tulasnes—who were showing how to do this, by patient and thorough investigations of each species that could be properly studied.

De Bary himself founded a school of exact inquirers,

[1] "Vergleichende Mor. hologie der Pilze." By Dr. F. Von Tavel (Jena: Fischer, 1892.)

whose brilliant discoveries on the nature of parasitism and the development of the fungi, and above all, the propounding and testing of intelligent theories to explain the facts observed, will never be forgotten.

Brefeld was driven, at an early period of his investigations, to differ entirely from De Bary regarding a fundamental point in the morphology of the fungi. Certain organs discovered by De Bary in the simpler ascomycetes were regarded by him as morphologically sexual organs, and in later years the doctrine of the sexuality of the fungi became a central pivot around which the whole question of the morphology and evolution of these remarkable cryptogams turned.

As is well known, De Bary showed that if his interpretation of the facts was right, we have the principal groups of the fungi ascending along one main path of development. Starting with the *Phycomycetes*, which include the mucors and the fungi of the potato-disease and vine-disease (and which are so obviously allied to certain green algæ that it was impossible to doubt that these lower fungi are derived from green algæ), the main path of evolution was traced through the lower ascomycetes, such as the fungi of the hop-disease, and the higher members of the same series—*e.g.* ergot of rye, the larch-disease, &c., and found to end in the Uredineæ or "rusts," and basidiomycetes—the mushrooms, toadstools, and puff-balls, &c.

From this main series, branches were regarded as given out at various points, as the *Chytridiaceæ*, *Ustilagineæ*—the "smuts" and "bunts" of our cereals, &c.—and so on.

De Bary pointed out very clearly that the most astonishing morphological phenomenon observable in the fungi is the gradual loss of first functional sexuality, and then of even the last traces of sexual organs, as we ascend from the lower to the higher fungi.

Brefeld—and it should be stated that the book before us is almost entirely an admirable short edition of Brefeld's ten large volumes—maintains that De Bary and his pupils were wrong in interpreting the organs of the ascomycetes in question as sexual, and that the loss of sexuality among fungi occurs much sooner than was supposed. The sexual organs, in fact, disappear within the limits of the *Phycomycetes* themselves, and De Bary's ascocarp and pollinodium lose all the significance his hypothesis assigned to them.

But De Bary's chief mistake—into which he was led by the above interpretation of his own observations—was in deriving his ascomycetous series from *the wrong branch of the Phycomycetes.* Instead of their origin being from the Peronosporeæ (*Oomycetes*) the ascomycetes are derived from the *Zygomycetes*, their line of descent passing through a group containing *Protomyces* and *Thelebolus*, and which group Brefeld terms the Hemiasci.

The oomycetes, indeed, are regarded as leading nowhere, except to the richly branched genera which compose it.

While the ascomycetes represent an enormously branched and successful series of forms which have specialized the type of the *sporangium* more and more, the basidiomycetes (in which the author includes uredineæ) have come off from another group of zygomycetes, and have specialized the *conidium* as their type of reproductive organ. The half-way group along this line is the Ustilagineæ, and Brefeld terms them Hemibasidii, accordingly.

The grounds for these revolutionary views cannot of course be explained in a review. They depend upon the numerous new facts brought to light by the untiring devotion and industry displayed in the Münster laboratory, and which are very clearly described and illustrated in the book before us.

A number of new forms have been discovered, of which the simple *Ascoidea rubescens* is an interesting example.

Very instructive types, as yet unknown in text-books, are *Thelebolus*, *Pilacre*, *Tomentella*, *Tylostoma*, and some others; owing partly to the new facts brought out regarding them, and partly to the suggestiveness of the new views as to their morphology, such forms bid fair to become as well known in future hand-books as *Mucor*, *Podosphæra*, and *Agaricus*, are in those of to-day.

The generalizations regarding the comparative morphology of the reproductive organs of fungi as a whole are distinctly an advance, and show a delightful gleam of light leading to freedom from the chaos of terms the subject has laboured under: "pycnidia" and "spermogonia" disappear as such—they are merely chambers in which conidia are developed (*conidien-früchte*), the germination of numerous spermatia by Brefeld and others having established the conidial character of those mysterious particles, the spermatia.

The author's views regarding *Chlamydospores* will probably cause surprise to many who have not followed the progress of Brefeld's work during the last few years. If these views are accepted, the principal "spores" of *Protomyces*, the *Ustilagineæ*, and the *Uredineæ* are all to be interpreted as *Chlamydospores*, homologous with the resting spores of Mucor; even more startling is the discovery of such Chlamydospores (including "oidia" and "gemmæ") among the higher ascomycetes and basidiomycetes, novelties which are only equalled perhaps by the rich series of true conidial spores found in the latter group.

Zopf's work on fungi had already prepared us, in 1890, for some of the changes which these discoveries entail, but Zopf was not prepared for anything like the revolution which Von Tavel has accepted—and, indeed, so great a change of front was impossible before the publication of Brefeld's ninth and tenth volumes.

The new "comparative morphology of the fungi" certainly offers many advantages in the simplification of our views as to the nature of the spore, and promises to remove the bone of contention which this item has always offered to mycologists. We are asked now to accept the following view. There are four types of sporogenous organs in fungi:—(1) The *sexual spore*, only met with in the lower fungi (*Phycomycetes*), as the zygospore and the oospore, and gradually losing the sexual character within the group. (2) The *endospore*, formed asexually in a sporangium, and occurring as zoospores (Peronosporeæ, &c.), sporangiospores (*Mucor*, *Thelebolus*, &c.), or ascospores (all ascomycetes), the ascus being merely a sporangium of *definite* shape and size, and containing a *definite* number of endospores. (3) The *conidium*, which starts as a one-spored sporangium where *the sporangial wall and that of the contained spore fuse*, illustrated by the "yeasts" of Ustilagineæ and many ascomycetes, the "sporida" of Uredineæ, and the "conidia," "stylospores," "spermatia," &c., of Uredineæ and ascomycetes. The "*Basidium*" is merely a specialised conidiophore where *the position and number of the conidia* ("basidiospores") *are constant*, and true conidia occur in the group in addition—*e.g.* *Heterobasidion*, *Tomentella*, &c. Indeed, it is the play on this type leading to gradual specialisation which characterises the whole Basidiomycetous series. (4) The *Chlamydospore*, which, in the form of the type or of so-called "oidia," "gemmæ" occurs generally in all the series, and becomes specialised into "fructifications" in the *Uredineæ*.

This is perhaps a fair sketch of the central ideas of the new school, though it by no means summarizes or even mentions dozens of other interesting points brought out in the book under review, such as the remarkable evolutions of the germination—*e.g.* in *Nectria*—of the conidiophore—*e.g.* in *Peziza*—of the conidium, the basidium, the ascus, &c.

In conclusion, it may be pointed out that although Von

Tavel writes more fairly with regard to the work done by other schools, and has wisely avoided the bitter methods adopted by Brefeld towards De Bary's pupils in some of his volumes, there still seems to persist a tone of under-valuation of the work of the Strasburg school. After all, it should never be forgotten that unless De Bary and his pupils had followed up the clue—however false it may prove—of the "sexuality" of the ascomycetes, the matter would have had to be investigated, and the fact that the Münster school is enabled to explain the phenomena seen in a new sense proves how valuable De Bary's careful observations were. Moreover, however probable Brefeld's view of the origin of the ascomycetous series is—and it is now the clearest story yet put forward—many of his own facts show that the *impossibility* of De Bary's view of a sexual origin, now lost, of the ascocarp, is by no means proved. Brefeld insists that the simplest ascocarp (*e.g. Thelebolus*) may be derived by suppressing the stalk and withdrawing the sporangium of a form like *Mortierella* into the investing barren hyphæ at its base; but the *zygote* of *Mortierella* also has investing hyphæ, and it would not be going much further to suppose the sporangium of the germinated zygospore of such a form to be similarly withdrawn into the invested capsule. This "wild hypothesis" would not alter Brefeld's view as to the homology of the ascus, or the derivation of the ascomycetes from the zygomycetes, but it would, and very materially, alter the attitude adopted towards the sexual hypothesis. We have termed the suggestion "wild," but it is possibly not more so than Brefeld's own hypothesis as to the nature and evolution of the chlamydospore, and we imagine that the last word has not yet been said on either matter. However that may be, Brefeld's laurels of results are such as are won by very few investigators and Von Tavel is to be congratulated not only for his own discoveries, but also on his book, which is by far the best exposition of the subject in existence.

H. MARSHALL WARD.

DAUBRÉE ON THE GEOLOGICAL WORK OF HIGH PRESSURE GAS.

A SERIES of experimental researches which promise to lead to important results, and which have already been applied by their author to the explanation of some difficult geological problems, have during the last few years been carried on by M. Daubrée. These experiments are concerned with the action of rapidly moving and high-pressure gas on rock masses, and lead to the conclusion that such high-pressure gas is a geological agent of no small importance. To carry out such experiments is no easy matter, but M. Daubrée has been fortunate enough to obtain the use of the apparatus used in the testing of explosives in the *Laboratoire Centrale des Poudres et Saltpêtres*. The high-pressure gas has been obtained by the explosion of gun-cotton and dynamite, the explosions being made in a steel cylinder with very thick walls, and closed at both ends with steel plugs. One of these plugs is fitted with a platinum wire, by the heating of which the charge can be exploded. The other, which under ordinary circumstances contains the manometer for measuring the force of an explosion, is modified so as to contain a block of the rock to be experimented on. A circular hole, moreover, is made at one end so that the gas, after traversing the rock, is allowed to escape. The rock, cut in the form of a cylinder, is supported between a steel stopper and the head of a piston. The charge of gun-cotton or dynamite usually filled a tenth part of the interior, and the pressures obtained were from 1100 to 1700 atmospheres. In one experiment the pressure was increased to 2300, and in another the still greater pressure of 2400 atmospheres was obtained. Many different kinds of rock were used, such as limestone, gypsum, slate, and granite, and each cylindrical block experimented on was cut through by a diametrical plane. In some of the experiments an additional very fine perforation was made along this plane.

As a result of the sudden shock of the explosions most of the rocks were fractured. In the case of the slate this resulted in faulting. The limestone and granite were broken up and crushed, but under the influence of the pressure the small fragments were quickly consolidated so as to resemble the original rock. This property of reconsolidating under pressure, thus shown to be possessed by rocks, seems analogous to the plasticity of ice observed by Tyndall.

All the rocks experimented upon, even the most tenacious, have undergone more or less erosion. The gases have disintegrated and pulverised them, and carried out the fragments. When their action was concentrated along certain lines, true perforations—that is to say, rounded channels more or less regular—were eroded through the blocks. In the case of a granite block the original perforation of 1·2 mm. was increased to a channel of 11 mm. The walls of these perforations after the explosions were found to be striated and polished. Sometimes the striations are parallel, like those produced by ice. At other times they spread in fan-form, and sometimes they are slightly curved.

The products of erosion are thrown out into the atmosphere, and an examination of the powder thus produced shows that a portion of the same possesses an interesting resemblance to the dust usually held to be of cosmic origin.

M. Daubrée applies the results of his experiments to explain the remarkable "diamond pipes" of South Africa. These diamond deposits are described by M. Mouelle in the *Annales des Mines* (tome vii. p. 193, 1885) as filling in cylindrical cavities of unknown depths in the rocks. These cavities appear to be cut out of the subjacent sedimentary or eruptive rocks, their upper parts are filled with a soft yellow decomposed rock matter, while below they contain hard volcanic conglomerate. They vary in size from a diameter of 20 to one of 450 m., and are originally surmounted by slight eminences, known as *kopyes* (little heads).

An interesting point about the general arrangement of the "pipes" is their occurrence along a straight line of 200 kilometres in length. Their walls, again, are smoothed and finely striated. These striations are often parallel, and indicate a powerful thrust from below upwards. No alteration is observable in the beds of shale forming the walls, except a slight elevation of their edges.

Thus in their general form, as long, narrow, cylindrical perforations in the earth's crust, they resemble the artificially produced perforations in the rocks experimented on. Their arrangement along a straight line suggests that they may have been opened along a line of fracture as were the perforations in the experiments. In the latter, the line of the eroded channel was determined by a very narrow perforation, and M. Daubrée suggests that in the former the positions of the "pipes" may have been determined in some cases by cross-fractures. The polishing and striation of the walls of the diamond pipes, again, is reproduced in the polishing and striation of the perforations in the experiments.

Another application of his experimental results made by M. Daubrée is to explain the opening out of the channels by which volcanic products reach the surface. Here, again, the linear arrangement of volcanoes, which has been so frequently pointed out, is noted as connecting volcanic vents with the experimental results. These are supposed to lie along lines of fracture, and each