of the Benedictine monks of Fulda, the trustworthiness of the lunar and solar tables, and the accuracy of the computer who brought out so marvellously correct a result without knowing that it agreed exactly with the true meaning of the record.

No doubt equal credit may be given to the computer's statement that this eclipse was total in London, totality continuing at St. Paul's from 1h. 16m. 20s. to 1h. 18m. 10s. local mean time.

C. S. TAYLOR.

Height of T'ai Shan.

A FORMER student of mine, Mr. S. Couling, has recently ascended T'ai Shan, the loftiest of the sacred mountains of China, and one of the most ancient and popular places of pilgrimage. He believes that the height of it above the surrounding plain has never before been measured, and has sent me his observations to reduce. The elevation from the plain to the summit comes out at 4780 feet; whilst a temple vaguely stated to be about 400 feet below the summit is, as ascertained by barometer, 4485 feet above the plain.

SILVANUS P. THOMPSON.

The Shadow of a Mist.

LIVING on the Blue Mountains at an elevation of 5000 feet, I am frequently astonished at the ever varying beauty of the mists and clouds. But a short time ago it was my good fortune to see the shadow of a mist, itself not visible.

On the evening of November 16, shortly after 7 o'clock in the evening, I was watching the electric light with which the military authorities were experimenting at Port Royal, 15 miles distant in a straight line. The light at times was so brilliant that the shadow of a person standing 20 feet from the house was distinct on the white-painted front, even when he held a lamp partially turned down close to his body on the side next the house. Rain was falling, but so slightly that there was no need for an umbrella. No mist or cloud was visible in the direct line to Port Royal, and yet a net-work of shadow was thrown on the house, the meshes of which were 3 or 4 inches in width. The shadows were all in motion, moving from east to west, in the direction of the scarcely noticeable breeze; individual portions of the meshes disappearing and re-forming as they moved, so that it was quite dazzling to look at the shadow, reminding me of the ripple on water as seen against a strong light. A puff of tobacco smoke had a shadow only when an inch or two from the house, so that the mist must have been much denser, and yet it cannot have been of any great breadth, or the shadow would have been uniform instead of reticulated. No doubt many of your readers can explain this appearance, which to me seemed so singular.

W. FAWCETT,
Director of Public Gardens and Plantations.

Cinchona, Gordon Town P.O., Jamaica, December 1, 1887.

The Ffynnon Beuno and Cae Gwyn Caves.

IT would seem that so long as the controversy with regard to the contents of these caves is confined to Dr. Hicks, Prof. Hughes, and Mr. W. G. Smith, the points at issue will never be decided. Dr. Hicks argues most needlessly for the *pre*-Glacial age of the cave deposits; Prof. Hughes calmly assumes that the outside deposits are *post*-Glacial; and many geologists must be heartily tired of hearing these two gentlemen contradict one another without defining what they mean by the terms Glacial and post-Glacial.

The fact is that the St. Asaph drift (to which Prof. Hughes now admits the outside deposits belong) is part of the later Glacial series of Northern England; and Prof. Hughes has no right to call it post-Glacial without defining what he means by that term. Most people call them Glacial deposits. If therefore the cave-deposits are older than this drift, they are not necessarily *pre*-Glacial, as Dr. Hicks maintains, but only anterior to what Mr. Mellard Reade terms the marine low-level boulder-clays. Now many think that these clays and their associated sands are coæval with, or newer than, the so-called post-Glacial river-gravels of Southern England. It is not surprising therefore that the cave fauna should be the same as that of the river-gravels, and it is perfectly needless to compare it with the fauna of the Cromer Forest bed.

In Lincolnshire the same marine shells occur in sands and gravels beneath the latest sheet of boulder-clay, and a gravel beneath the same clay at Burgh has yielded teeth and bones of *Elephas antiquus*, *Rhinoceros leptorhinus*, and *Bos primigenius*. These beds are on the same line of latitude as St. Asaph, and are probably of the same age as that drift; but it may be that neither of them are older than the oldest river-gravels of the Cam or Thames valleys.

It has been repeatedly pointed out that the terms Glacial and post-Glacial cannot be used as conveying any idea of relative age except along one and the same parallel of latitude, and it is rather surprising that the Woodwardian Professor of Geology should seem to be unaware of this. If by post-Glacial Prof. Hughes means later Glacial or newer Pleistocene, everyone will probably agree with him, but he confuses the issue by his bad choice of terms.

The palæontological evidence is really of no value—the argument leads nowhere; what we want is an expression of opinion by some geologist who has seen the locality and the recent excavations, regarding the explanation proposed by Prof. Hughes, viz. that the present position of the bones beneath the marine drift is due to the falling in of the roof of the cave near one entrance, while the animals may have got into the cave by another opening. Many geologists have visited the locality—will some of them communicate their views on this point?

A. J. JUKES BROWNE.

Southampton, December 28, 1887.

THE OLD MOUTH AND THE NEW: A STUDY IN VERTEBRATE MORPHOLOGY.

"THE question of the nature of the mouth," says Prof. Dohrn in one of the first of his celebrated "Studien zur Urgeschichte," "is the point about which the whole morphological problem of the Vertebrate body revolves." According to Dohrn, the present mouth of Vertebrates arose from the coalescence of a pair of gill-clefts. In this we have an example of Dohrn's principle of change of function, and also, as I hope soon to demonstrate, of Kleinenberg's law of the substitution of organs. I do not now wish or intend to give an account of the researches by which Dohrn showed that the mouth in some cases first arises as a pair of lateral invaginations of epiblast, still less of my own small contribution to this question, which consisted in recording the facts that the mouth also resembles a gill-cleft in some other particulars.

It suffices here to say that these researches have not yet been refuted, and that the view that the present mouth of Vertebrates is, so to speak, a new structure, rests on a very sound foundation.

With the blastopore as the foundation of mouth and anus I have here no concern, nor have I any sort of sympathy with the upholders of a theory which has been condemned and rejected by embryologists such as Lankester, Kleinenberg, and Salensky.

The problem I have to discuss is, granted that the present Vertebrate mouth is a new[1] structure, what traces, if any, are to be found of the old mouth? It is conceivable, and I strongly emphasize the point, that the old mouth might have disappeared, even from the development, without leaving a trace behind.

We seem to be gradually getting out of the idea that ontogeny is even a fair repetition, much less a perfect one, of phylogeny, for absolutely rudimentary organs (organs performing no function at all) are only retained as larval or embryonic organs, as the basis or *Anlage* of other organs, or, finally, because they are inseparably connected with the development of other organs. Of the latter a fair case, it seems to me, is to be seen in the rudiment of the parietal eye in the higher Vertebrates. This organ, functionless except in a few fishes and reptiles, possibly only reappears in the development because it is intimately connected in some way or other with the paired eyes.

A still better example is, I think, to be met with in the

[1] It is rather paradoxical to speak of a thing as new which has existed in its present form for untold millions of years.

rudiments of the gill sense-organs and ganglia described by Prof. Froriep in Mammalia. (Of these I hope to give a fuller account in connection with other work.) I find them in lizards, crocodiles, and birds; and there can be little doubt that they exist as rudiments in all animals above fishes and amphibia. Their recurrence has its explanation in that they probably form the *Anlage* for certain portions of the cranial ganglia.

It was Dohrn who first hinted, in his work on "Der Ursprung der Wirbeltiere," published in 1875, that the hypophysis cerebri represented the last remains of the old mouth, and that it must have opened on the dorsal surface, after passing between the crura cerebri.

This idea he soon gave up, and indeed, in the work above mentioned, he inclined to the view that the opening lay somewhere in the region of the medulla oblongata. Since then he has relinquished, for the time, the search for the old mouth, and has advised others to do the same.

His first hypothesis has more recently been advanced as new by Prof. Owen and Mr. J. T. Cunningham. Both of these writers hold very slightly different views from those originally suggested by Dohrn.

Some of the statements which I am about to make appear on the surface to bear slight resemblance to Cunningham's views, but, as I hope will be seen, nothing could be further from the truth. Cunningham, starting from Balfour's well-known and now universally accepted, belief that the spinal cord and brain were once an open plate, advocated, as the latest discovery of Vertebrate morphology, the view that the infundibulum, whose walls consist of nervous matter and nothing else, is the vestige of the old mouth which pierced the brain.

One cannot but marvel at the rashness of an hypothesis which annexes, without more ado, a portion of the nervous system, and proclaims it to all the world as the remnant of a former passage from the exterior to the stomach of the animal!

Cunningham overlooks entirely the nature and exceedingly complicated development of the processus infundibuli, or nervous portion of the hypophysis.

Although, thanks to Rabl-Rückhard and others, we have obtained a certain amount of light on the nature of the pineal gland or epiphysis, the body (hypophysis), at the opposite end of the third ventricle still remains one of those organs on which all sorts of speculations may be made, with impunity. Some of the explanations offered are in accordance with certain facts of its development. Others, on the contrary, accord with no known fact of embryology.

The nervous part—or, as I shall call it, the *neural hypophysis*—has been considered by Rabl-Rückhard as a gland secreting cerebro-spinal fluid. I must, however, express a strong opinion that such a glandular function is extremely improbable, for the conversion of a piece of nervous tissue into a gland is absolutely without parallel.

Goette and Wiedersheim both regard the nervous part as a remnant of a sense-organ; against which view *a priori* little or nothing can be said. The mouth part or *oral hypophysis* was finally classed by Dohrn as the rudiment of a pair of gill-clefts—a supposition not wholly unsupported by its developmental history. It has also, not unnaturally, been looked upon as a remnant of a mouth-gland.

Prof. Hubrecht made it the basis of his comparisons of Nemertea and Vertebrata, and saw in it the remains of the Nemertean proboscis, the Vertebrate notochord being the homologue of the proboscis sheath—comparisons which appear to me to be as little capable of support as those of the same investigator between the Vertebrate and Nemertean nervous systems.

And so, after all, on turning to Wiedersheim's latest book, "Der Bau des Menschen," we read: "The hour of the release of the hypophysis cerebri from its obscure position has not yet struck, and the problems it presents are rendered more difficult in that it develops from two different points—from the brain (infundibulum) and from the epiblast of the primitive pharyngeal involution."

For what we know of the facts of its anatomy and development we are mainly indebted to five distinguished morphologists: Profs. W. Müller, Goette, Mihálkovics, Kölliker, and Dohrn. In the following very brief summary I partly follow Kölliker's account (in his valuable "Entwickelungsgeschichte des Menschen," 1879), which, for the time it was written, is by far the most complete we possess.

My own researches on Sharks, Ganoids, Dipnoi, Cyclostomata, Amphibia, Lizards, Snakes, Crocodiles, Birds, and Mammals, mainly confirm Kölliker, who, in his turn, has taken the greater portion of his account from the beautiful classic of Mihálkovics.

The hypophysis cerebri is composed of two parts: the one, *neural hypophysis*, derived from the nervous system; the other, *oral hypophysis*, from the epiblast in the region of the mouth.

The oral hypophysis is formed early in development as an epiblastic involution towards the end of the notochord, *i.e.* towards the hypoblast, and in the direction of the base of the brain. In some cases it may even grow in the direction of a process of hypoblast immediately below the anterior end of the notochord. But, except in Myxine, it never fuses with the hypoblast. It afterwards becomes pinched off from the pharynx, and gets thus to lie on the floor of the skull, becoming finally converted into a compound gland-like organ.

The neural hypophysis, or hinder lappet of the hypophysis, on the other hand, develops ventrally as a process of the basal portion of the thalamencephalon, or hinder part of the fore-brain. At first composed of tissue of exactly the same character as the rest of the thalamencephalon, it becomes solid below and converted into indifferent tissue; the portion of the process which remains hollow, and forms the base of the infundibulum, alone retains a nervous structure. Kölliker records that in pig embryos of 3 centimetres in length longitudinal bundles of nerve-fibres pass into the developing neural hypophysis, or processus infundibuli as it is called, from the base of the thalamencephalon.

In most cases, especially in Mammalia and also in Dipnoi, the neural hypophysis becomes closely and almost inseparably connected with the oral hypophysis. Usually the *Anlage* of the oral hypophysis lies in the region of the mouth epiblast; in Petromyzon and Myxine it lies in front of and outside the mouth. The process by which it got into the mouth involution cannot be explained without numerous figures.

According to Dohrn, the oral hypophysis arises in Petromyzon as an invagination of epiblast in front of the mouth between the oral and nasal depressions. It grows towards the base of the infundibulum, and comes into close relationship with the end of the notochord, *i.e.* with a structure derived from hypoblast, while it approaches a special process of hypoblast itself, with which, however, in Petromyzon it does not fuse. In Myxine, although the development is unfortunately not yet known, we may assume that this fusion is effected, for in that animal it opens throughout life into the gut (see figure, *O.M.*).

In Ammocœtes it gives off a certain number of gland-follicles, which, according to Dohrn, become pinched off in the Petromyzon. While I am not yet quite convinced of the certainty of this latter point, I find, in Myxine, numerous small glandular follicles opening into the oral hypophysis. In Petromyzon and Myxine the neural hypophysis is present, and, as I believe, not rudimentary. It appears to supply nerve-fibres to the oral hypophysis.

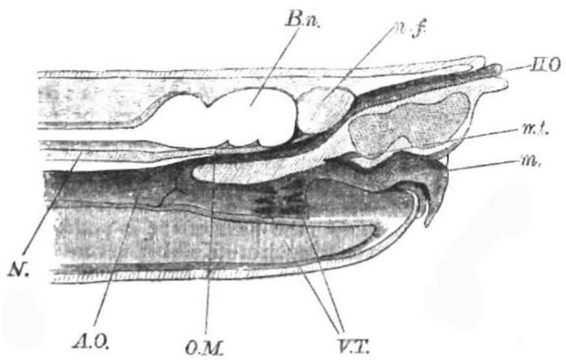
Dohrn finds in Hippocampus traces of a paired origin of the oral hypophysis. This is important.

I propose to divide the oral or glandular hypophysis into two parts, viz. a duct or main oral hypophysis and a glandular part or glandular hypophysis. The whole structure is without doubt in nearly all cases rudimentary, and of little or no functional importance. A mass of information bearing upon it has recently come to light in the study of the developmental history of Annelids, chiefly at the hands of Kleinenberg and Salensky.

From the results of Kleinenberg's work, more especially, we are placed in a position to compare the structure and development of the hypophysis with those of certain organs in the worms. To my mind, the comparison which follows is one of the neatest in the whole range of comparative morphology; I would therefore, before proceeding further, give a brief *résumé* of Kleinenberg's results so far as they here concern us.

In the first place, he records how the larval stomodæum or mouth is replaced in a very complicated manner by the Annelid permanent mouth or *Schlund.* The latter is formed as a paired involution of the stomodæum, *i.e.* of the epiblast, and this he considers to have originally represented stomodæal glands. It encroaches upon and swallows up the old mouth, and, finally fusing with the hypoblast, it opens into the gut.

The replacement of the larval mouth in Annelids by a new structure was already known, but Kleinenberg describes the steps of the process in great detail, and he



Myxine glutinosa. Head in longitudinal section × 2. *H.O*, opening of hypophysis; *m.*, mouth opening; *m.t.*, median tooth; *V.T.*, ventral teeth; *n.f.*, one of the folds of the nasal sensory membrane; *Bn*, brain; *O.M.*, opening of hypophysis into gut; *A.O.*, œsophagus; *N.*, notochord.

states that this mode of mouth substitution by means of a paired involution is of very wide occurrence in the Chætopods.

In it we have a direct parallel to the substitution of the old Vertebrate mouth by a pair of gill-clefts, but, in truth, we have something more.

Another phenomenon of extreme interest is the formation of the special mouth (or *Schlund*) nervous system. *This apparatus is only concerned with the innervation of the permanent* Schlund, *and takes no share in the innervation of the hypoblastic alimentary canal.* It arises as a special process of the hinder part of the subœsophageal ganglion: this grows towards the developing Schlund, becomes closely attached to the latter, fuses with it and gets pinched off from the larger portion of the subœsophageal ganglion, which is left as the first ganglion of the ventral chain.

I must here digress in order to discuss the question of the presence or absence of any representative of the supraœsophageal ganglion of Annelids in Vertebrates,[1] and here again Kleinenberg comes to our assistance.

I have myself devoted a good deal of attention to this point, and have arrived at the conclusion (held also, I believe, by Prof. Dohrn) that there is no likelihood at all of our finding an area in the Vertebrate brain which was ever pierced by the œsophagus—pierced so as to divide the brain into a supraœsophageal and a subœsophageal portion, which might be compared respectively to such divisions of the Annelidan nervous system. At first sight, this appears like an admission that the Annelidan theory of the origin of Vertebrates is untenable. But such is not the case.

From a large number of researches, including those of Bergh, Salensky, and Kleinenberg, we know that the supraœsophageal ganglion of Annelids certainly arises independently of the ventral chain, and that it only later becomes connected with the latter by the development of the circumœsophageal collar.

Kleinenberg's brilliant researches also teach us that the permanent Annelidan nervous system arises through substitution, and partial or entire disappearance of whole larval nervous apparatuses and sense-organs. And, indeed, after reading his beautiful work, one is fully prepared for one of the closing statements in it—that possibly the supraœsophageal ganglion is entirely absent in Vertebrates.

Personally, I have no hesitation at all in accepting this as probably true; but the grounds for my belief, or some of them, I can only hint at here. They arise out of as yet unpublished developmental researches. Briefly stated, I see in the development of the gill-clefts, with their special sense-organs and ganglia[1]—all of which lie in the region which is under the control of a system comparable to the ventral nerve-cord of Annelids—a probable cause of the disappearance of the supraœsophageal ganglion in the ancestors of Vertebrates,—in a similar way to that in which, according to Kleinenberg, the dislodging and destruction of the special larval ganglionic centres takes place in the Annelid.

I believe that in the ancestors of the Vertebrates, by the development of the eyes, and of the important gill sense-organs and ganglia, the ventral chain came to obtain control over a very extensive system of ganglia, sense-organs, and muscles; and, having already a control over the mouth or *Schlund*, it entirely deposed the supraœsophageal ganglion (and its sense-organs). The entire *raison d'être* of the latter being thus disposed of, it naturally degenerated and finally disappeared.

If it be admitted that the supraœsophageal ganglion of Annelids is absent in Vertebrates, and that the brain and spinal cord of the latter may be compared directly with the ventral cord of Annelids, then a whole host of direct structural relationships between Annelida and Vertebrates may be established. Kleinenberg expresses his opinion that the spinal ganglia of Vertebrates have their parallel in the parapodial ganglia of Annelids,—a comparison which, as I shall elsewhere show, is entirely justifiable for the spinal ganglia and for certain portions of the cranial ganglia also.

Let me now briefly review the conditions demanded of any structures in the Vertebrate which are to be homologized with the permanent mouth of Annelids. Such ought to arise as a paired involution of epiblast (though it is conceivable *a priori* that the paired character might be lost). This involution must fuse with, and open into, the cavity of the hypoblast. It must also give rise to certain glands, and it must have a special nervous system of its own derived from the hinder part of the first ventral ganglion or its homologue—which nervous system must supply it alone, and no other part of the alimentary canal.

All these conditions are fulfilled by the complex called hypophysis cerebri.

In at least one case (Hippocampus) the oral hypophy-

[1] I postpone the consideration of Prof. Semper's views on this point, and on the nature of the mouth in Annelids and Vertebrates.

[1] The cranial ganglia of Vertebrates are far more complicated morphologically than has hitherto been recognized. In addition to parts which appear to correspond morphologically to the posterior root ganglia of the spinal nerves plus the sympathetic ganglia, they also contain the special ganglia which are formed in connection with the gill sense-organs.

sis[1] is known to arise as a paired epiblastic involution (Dohrn). In the Cyclostomata it is formed as an epiblastic involution (possibly paired) at the extreme anterior end of the body. In one Vertebrate alone, Myxine (*vide* figure), it still opens into the hypoblast; in all others it approaches the hypoblast in development, but does not fuse with that layer. It always lies in very close relationship with the extreme end of the notochord—that is, with the end of a structure derived from the hypoblast.

In adult Petromyzon, in which the tube of the oral hypophysis has the same relationships as in Myxine, except that the posterior opening into the hypoblastic sac is absent, it nevertheless has an astonishing length, and ends blindly very close to the gut. In Myxine and Petromyzon, tubular glands are developed in connection with it. In all the higher Vertebrates, in which the oral part is very rudimentary, it always has a distinct glandular character.

And now, what of the last condition? This also is satisfactorily met. In all cases the oral hypophysis has a special, and indeed large, process of nervous matter (the processus infundibuli, or neural hypophysis), which is derived from the posterior part of the fore-brain, from the base of the infundibulum. This process is concerned with the innervation of the oral hypophysis alone. In Myxine and Petromyzon alone, so far as my researches extend (possibly also in Protopterus), this nervous system is not rudimentary. In most Vertebrates the neural hypophysis, which, as Kölliker aptly remarks, is at first composed of the same cell elements and fibres as the rest of the brain, degenerates, and in very many full-grown animals forms a mass of tissue, the structure of which many observers have compared to that of the suprarenal bodies (known to be masses of degenerated tissue).

The neural hypophysis is thus the most remarkable structure in the whole of the Vertebrate central nervous system. Though degenerated, it still clings to the traditions of its ancestry, for even, as it were, in its death it is closely and almost inseparably connected with the rest of the hypophysis, especially in Mammalia and in Dipnoi. In Myxine alone, of all Vertebrates, the old mouth still retains some of its functions as a mouth; it conducts the water of respiration to the gills. In this case, even, changes have occurred, for the nose[2] (see figure, *n.f.*) has got partly involved in the passage of the old mouth. If it be true that the nose was originally a branchial sense-organ—which view, in spite of Gegenbaur, I still maintain—its assumption of a position in the passage of the old mouth in Myxine is, on purely physiological grounds, intelligible.

It is well known that that which I call the old mouth in Myxine is purely respiratory, conducting water into the gills; and what then could be more likely than that one of the branchial sense-organs should be, as it were, told off to do duty at its entrance. It is certain, from Goette's and Dohrn's researches, that these passages in Myxine and Petromyzon are the representatives of the oral hypophysis. I have gone over and extended these observations, and can fully confirm Dohrn in nearly every point, and all I claim here is the identification of the hypoblastic opening in Myxine as the (modified) opening of the old mouth into the gut.

If the above morphological comparison can be maintained (and I believe it can), the importance of its bearing on the morphology of Vertebrates can hardly be over-estimated.

A number of other problems and conclusions arise out of all this, but I reserve the consideration of these for a much more exhaustive work, in which the literature of the subject will receive full attention. J. BEARD.

Anatomisches Institut, Freiburg i/B., November 16.

[1] I believe it is very frequently paired, though not at its point of origin.
[2] In Petromyzon, Dohrn finds that the nose is at first a special depression apart from the hypophysis invagination. The latter lies between the nose and mouth.

TIMBER, AND SOME OF ITS DISEASES.[1]

III.

HAVING now obtained some idea of the principal points in the structure and varieties of normal healthy timber, we may pass to the consideration of some of the diseases which affect it. The subject seems to fall very naturally into two convenient divisions, if we agree to treat of (1) those diseases which make their appearance in the living trees, and (2) those which are only found to affect dead timber after it is felled and sawn up. In reality, however, this mode of dividing the subject is purely arbitrary, and the two categories of diseases are linked together by all possible gradations.

Confining our attention for the present to the diseases of standing timber—*i.e.* which affect undoubtedly living trees—it can soon be shown that they are very numerous and varied in kind; hence it will be necessary to make some choice of what can best be described in this article. I shall therefore propose for the present to leave out of account those diseases which do injury to timber indirectly, such as leaf-diseases, the diseases of buds, growing roots, and so forth, as well as those which do harm in anticipation by injuring or destroying seedlings and young plants. The present article will thus be devoted to some of the diseases which attack the timber in the trees which are still standing; and as those caused by fungus parasites are the most interesting, we will for the present confine our attention to them.

It has long been known to planters and foresters that trees become rotten at the core, and even hollow, at all ages and in all kinds of situations, and that in many cases the first obvious signs that anything is the matter with the timber make their appearance when, after a high gale, a large limb snaps off, and the wood is found to be decayed internally. Now it is by no means implied that this rotting at the core—"wet-rot," "red-rot," &c., are other names generally applied to what is really a class of diseases—is *always* referable to a single cause; but it is certain that in a large number of cases it is due to the ravages of fungus parasites. The chief reason for popular misconceptions regarding these points is want of accurate knowledge of the structure and functions of wood on the one hand, and of the nature and biology of fungi on the other. The words disease, parasitism, decomposition, &c., convey very little meaning unless the student has had opportunities of obtaining some such knowledge of the biology of plants as can only be got in a modern laboratory: under this disadvantage the reader may not always grasp the full significance of what follows, but it will be at least clear that such fungi demand attention as serious enemies of our timber.

It will be advantageous to join the remarks I have to make to a part description of some of the contents of what is perhaps one of the most instructive and remarkable museums in the world—the Museum of Forest Botany in Münich, which I have lately had the good fortune to examine under the guidance of Prof. Robert Hartig, the distinguished botanist to whose energy the Museum is due, and to whose brilliant investigations we owe nearly all that has been discovered of the diseases of trees caused by the Hymenomycetes. Not only is Prof. Hartig's collection unique in itself, but the objects are classical, and illustrate facts which are as yet hardly known outside the small circle of specialists who have devoted themselves to such studies as are here referred to.

One of the most disastrous of the fungi which attack living trees is *Trametes radiciperda* (Hartig), the *Polyporus annosus* of Fries, and it is especially destructive to the Coniferæ. Almost everyone is familiar with some of our common Polyporei, especially those the fructifications of which project like irregular brackets of various colours from dead stumps, or from the stems of moribund trees;

[1] Continued from p. 207.