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INTRODUCTORY REMARKS BY THE CHAIRMAN*

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A personal impression is given of the progress in the study of Discomycetes during the last half century. Boudier's fundamental ideas still form the main basis for a classification of the Discomycetes. Some of the recent trends in the taxonomy of the Operculates are considered.

I feel it a great honour to preside at this meeting and take the opportunity to say some introductory words.

It would have been natural to try to give a short historical sketch of the progress in "discomycetology" during the nigh 50—or more precisely 48—years I have been working in this branch. In recent times, however, Dr. Kimbrough in his excellent paper entitled "Current trends in the classification of Discomycetes" has already covered the historical background, so that I shall restrict myself to some personal impressions.

As we know, the French mycologist Boudier as early as 1879 had already suggested the high taxonomic value of the mode of dehiscence of the ascus, which opens either by means of a hinged operculum or a simple pore. Six years later he published a rather elaborate scheme of the classification of the Discomycetes, the main divisions of which were the Operculates and Inoperculates. In view of the optical equipment of his time it is surprising that he was able to see these subtle structures.

It would take a long time for Boudier's ideas to become generally known and accepted. Perhaps the time was not yet ripe, perhaps there was some other important reason.

Taxonomists, of course, are to a certain degree influenced by geographical boundaries, both natural and political, as most of their field work is necessarily restricted to a certain area or certain areas. Most publications, too, cover geographical areas of limited extent. While this is quite natural, it should nevertheless be borne in mind that the tendency of too many students to neglect more or less completely the investigations carried out in even closely neighbouring areas has slowed down progress considerably and become the source of a great deal of unnecessary confusion.

When I began studying Discos the standard works were first of all Rehm's magnificent volume in Rabenhorst's "Kryptogamenflora von Deutschland, Österreich und der Schweiz" but also Karsten's "Mycologia Fennica," Schroeter's Flora of

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Silesia, and Phillips' and Massee's Floras of Britain, all of the 19th century. Three of these had been published so late that their authors must have had ample time to seem to have been influenced, however, while Rehm only cursorily mentioned get acquainted with the ideas of the great French mycologist. The authors do not Boudier's work.

From the very beginning I had the privilege of having access to Boudier's "Icones Mycologicae". I studied this work carefully, admired the plates, read his other publications, compared Discos of various groups, and soon became convinced of the soundness of his ideas. Other contemporary students arrived at the same conclusion, and particularly after the publication in 1928 of Seaver's "North American cup-fungi (Operculates)" every serious student seems to have accepted the two main groups proposed by Boudier.

Up till now no new facts have been discovered to disturb the picture. It is true that there are still a number of Discos whose position is doubtful. Most annoying perhaps is the genus *Cyttaria*, which is most peculiar in almost every respect. After the recent studies by Dr. Kimbrough I am personally convinced that it is a true Operculate, the aberrant features of which may be explained by its ecology. To give one example, thick-walled cells and relative longevity of tissues form a combination of features which is known to have evolved independently in various groups of fungi. Suffice only to mention the lichenized fungi.

The Operculates constitute a much smaller group than the Inoperculates and one which is much more homogeneous and far less diversified. To my mind the Operculates form a natural monophyletic taxon, the origin of which dates far back in time. The group seems to have split up rather early into different evolutionary lines, of which at least the surviving members are not too numerous. The partial unveiling of these lines is perhaps the most important advance in our field during the last half-century. But here also Nature has not cared, of course, to mark the evolutionary lines with arrows indicating in which direction they run.

There are, in my opinion, clear indications that certain aberrant small groups or single species will eventually be found to constitute evolutionary lines of their own.

Fifty years ago developmental and cytological studies on the stages of ascocarp formation and related phenomena were very much in vogue. In most groups of plants and animals similar studies on early stages had given results of utmost phylogenetic interest, but in Ascomycetes almost every species studied showed a number of peculiar or even unique features, which made it impossible to discern a pattern of phylogenetic lines. Would it be possible to explain this phenomenon in the following way? The Ascomycetes, after having lost their normal sexuality, which entails the loss at least of the motility of ciliate male gametes, have tried to develop a substitute in various ways, and it is these ways which are still flexible and open to further experimentation.

Even if or, rather, perhaps because studies such as mentioned above have failed to elucidate the origin of the Ascomycetes and the main lines of their evolution, there is every reason to believe that similar investigations in smaller and well circum-

scribed groups will be very fruitful in tracing what I would like to call micro-evolution. Thus it is with great expectation that van Brummelen's paper is awaited.

There is another character, a cytological one of rather simple nature, which in recent times has proved to be very important taxonomically, viz. the number of nuclei in the mature spore. Four-nucleate spores were one of my motives in 1937 for emending the scope of the genus *Helvella*, and some 25 years later Berthet was able to demonstrate that the number of nuclei is of high value generally in characterizing the larger groups within the Operculates.

Further new approaches to a better understanding of the relationships are the studies on the apical apparatus of the ascus by Chadefaud and his collaborators, on the spore ornamentation by Mme Le Gal, on the carotinoids by Arpin, on various chemical reagents, on conidial stages, etc., etc.

It is always tempting to believe that a new approach promises to become the thread of Ariadne, with the help of which it should be possible infallibly to find the way out of the labyrinth, and so the significance of the results is sometimes grossly overemphasized.

A case in point is in my opinion the transference by Arpin of *Sepultaria*, *Tricharia*, *Mycolachnea*, and *Trichophaea* to the Otideaceae, because they were found devoid of carotinoids. Morphologically they deviate considerably from the typical members of this family, but show good agreement with *Scutellinia* and several other carotinoid-possessing genera, in the neighbourhood of which the genera under discussion have usually been placed. It may be remembered that albinistic mutants, e.g. in *Sarcoscypha coccinea*, are known to occur. Why would it not be possible for such a mutant to become genetically stable and give rise to a new species or a group of species? Moreover, can we be sure that the genera mentioned earlier are really devoid of carotinoids? Is it not possible to assume that these are present in the shape of colourless precursors or colourless derivatives? To my mind the situation is much the same as in the period of the first bold attempts at employing lichen-substances in lichen taxonomy.

Sound taxonomy should make use of all the characters available and weigh them against one another. Besides it should be kept in mind that in certain cases any character may fail to show up or appear in disguised form.

Our present knowledge of the classification of the Operculates has recently been excellently summarized, independently of each other, by Rifai and Eckblad. It is interesting to see how they arrived at similar conclusions in most respects. Their work also shows how numerous and big are the gaps in our knowledge. It is to be hoped that this meeting will contribute to at least some of the gaps being filled up, although on the other hand new gaps are likely to be uncovered. In this connection it should be pointed out that in almost all larger genera the species are badly in need of a careful and critical revision.