FIFTY YEARS OF FUN WITH THE DISCOMYCETES, AND WHAT'S LEFT TO DO

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Mycology: Past, Present, and Future
to Honor the Career of
Dr. Richard P. Korf, Professor Emeritus
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Fontispiece: Richard P. Korf
in the rôle of Elias Magnus Fries
on the occasion of the celebration of Fries' 200th birthday
at the Mycological Society of America meetings held in conjunction
with the V International Mycological Congress in
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I wish to express my deepest thanks to those who came up with the idea for such a symposium as this, a wholly unexpected surprize, and an honor I most deeply appreciate even as I doubt that I deserve such a singling out, for as you will see later in this talk, the dent I have made in the mountain of ignorance is very small indeed; I must express my thanks to those who have so strongly contributed to my career: first to my students from whom I learned a great deal, and whose continued progress after they left Cornell has been an exciting and totally rewarding confirmation that students is what a university is all about, second to the teachers I had who inspired me and also convinced me to ask unanswerable questions, third to colleagues worldwide—many of whom I know only from the literature both current and long past—and from whom I learned nearly everything else, fourth to the university administrators and granting agencies who have employed me and supported[1] my students and technical helpers, or provided me funds to travel and to collect fungi from Japan to above the arctic circle to my favorite haunts in the tropics, and last but by no means least to my family, parents, wife, and children, who have borne with me and given me leave to pursue my career even when it took me away from them for extensive periods.

I was asked by Bill Fry, the Chair of my Department, some time after this symposium was thought up whether a) I objected to such an occasion—obviously I didn't, and b) whether I would be willing to present a talk at this symposium on my research. The talk that I am giving touches on a few aspects of my research during the last 6 weeks, couched—as is my wont—in an historical framework, giving you some notion of how little progress this scientist made over a 50-year span, and concluding with some thoughts on how mycologists might attack the rest of that mountain of ignorance I mentioned earlier. I shall not dwell further on the fact that much of my career has been spent in producing monographs, submonographs, and keys, and that I have even attempted to study the whole discomycete "flora" (let me be up to date, and say "mycota") of the Atlantic islands that comprize Macaronesia. It was just over 50 years ago that I had my first in-depth encounter with some of the Discomycetes or "cup-fungi," that group of organisms that was to intrigue me for the rest of my professional life. I was then a first-term sophomore at Cornell University, and had been convinced by my brilliant freshman Botany instructor, Professor Loren Petry, that I "must take the course in Plant Pathology given upstairs by Professor Whetzel." I dutifully followed his advice, and had the opportunity to meet and be inspired by Herbert Hice Whetzel, the most dynamic teacher I have ever encountered. It was the last time he was ever to offer the course, for he died soon thereafter. It was during his course that I met and was examined on my knowledge of a fungal disease by a great teacher, Watt Dimock, another Plant Pathology professor, and in that course also learned of the highly successful "plant doctor" and her books, Cynthia Wescott. That these three names appear as sponsors of this symposium is, for me, like coming home again.

In Prof Whetzel's course I learned about diseases caused by members of the genus *Sclerotinia* Fuckel, almost all of the species of which produced apothecia arising

[1] In particular I must acknowledge the generosity of Anna E. Jenkins, a long-time personal friend and graduate of this Department, whose bequest has provided stipends and other support for a whole generation of mycologists in my laboratory.

from overwintering sclerotia and formed ascospores as dispersal agents. Some species produced in addition *Botrytis* Persoon or *Monilia* Bonorden conidial states which also functioned in asexual reproductive dissemination. Prof Whetzel's own research in mycological aspects of plant pathology had, by 1943, centered on the taxonomy of *Botrytis,* and as an offshoot that of *Sclerotinia.*

In the spring term of my sophomore year I took two more plant pathology courses, one on plant disease control, and one on mycology. I quickly discovered that what had so enchanted me with plant pathology as a discipline was not really the diseases, many caused by fungi but others by bacteria, viruses, nematodes, etc., but the fungi that cause disease. Thus 1944 marked my first recognition that I was to be a mycologist, and during that year I was introduced by my mycology professor, Harry Morton Fitzpatrick, to what I still consider to be the most beautifully-illustrated volumes on fungi ever printed, Émile Boudier's *Icones Mycologicae.* Two of its three volumes of folio plates are devoted to the Discomycetes. It is no exaggeration to say that I began my life-long love affair with the Discomycetes then and there. I proceeded to take Fitzpatrick's full-year Advanced Mycology course, normally open only to graduate students, in my junior year, and took two terms of independent research on Discomycetes during my senior year. Immediately thereafter I enrolled in the graduate school in 1946, when I embarked on a monographic study of a tribe of the inoperculate discomycetes, the Arachnopezizeae, a thesis study which resulted in the award of my Ph.D. in 1950.

Fitzpatrick was convinced that field work is an essential for understanding fungi, and during both my undergraduate and graduate career I was fortunate to have as the teaching assistant assistant in mycology a consummate field-worker for all sorts of fungi, Clark T. Rogerson, who was to become a faithful friend. He has had an exceptionally distinguished career in mycology, particularly during his long tenure at the New York Botanical Garden. As a student he shared with me his collections, and we frequently collected together, not just Discomycetes but all groups of fungi, and from him I learned more perhaps than from any other mycologist.

With this preamble out of the way, I have prepared you for my talk. Where and when did I become bold enough to assert my professional involvement? My very first mycological paper[2] was a brief note in 1947 reporting a new locality for a very rare discomycete, *Seaverinia geranii* (Seaver & Horne) Whetzel, which had originally been called *Sclerotinia geranii* Seaver & Horne, and which had been transferred by Prof Whetzel to his new genus *Seaverinia* Whetzel, which lacks sclerotia and whose *Botrytis* state has sculptured instead of smooth conidia. To this date I have been exceptionally interested in the conidial genus *Botrytis* and in other, *Botrytis*-like, conidial fungi, since some, but not all of which, also have Discomycete apothecial states.

The following year, 1948, at the age of 23, was when I presented my first talk before the national meetings of the Mycological Society of America. It was not on my thesis work, which was on a group of inoperculate discomycetes, but rather on four particularly intriguing operculate discomycetes, members of the other large grouping within the cup-fungi. One of the fungi I talked about at that meeting was

[2] Korf, R. P. 1947. *Seaverinia geranii.* Mycologia 39: 743.

an apparently undescribed species of *Peziza* Linnaeus found in the greenhouses at Cornell, which produced a *Botrytis*-like conidial state. In preparing this lecture I discovered the original 3x4-inch glass lantern slides I used for that talk still on my shelves after all these years, and show you these here (Fig. 1). This is a picture of the

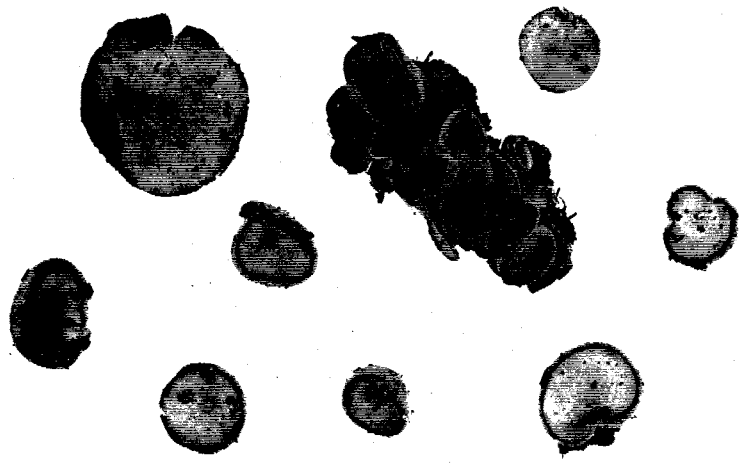

Fig. 1 *Galactinia* n. sp., now *Peziza ostracoderma,* approx. natural size (CUP *37284*)

cup-shaped to discoid apothecia from the Cornell greenhouses, which are about 1-inch in diameter, and Fig. 2 is a set of my drawings showing some of the features of

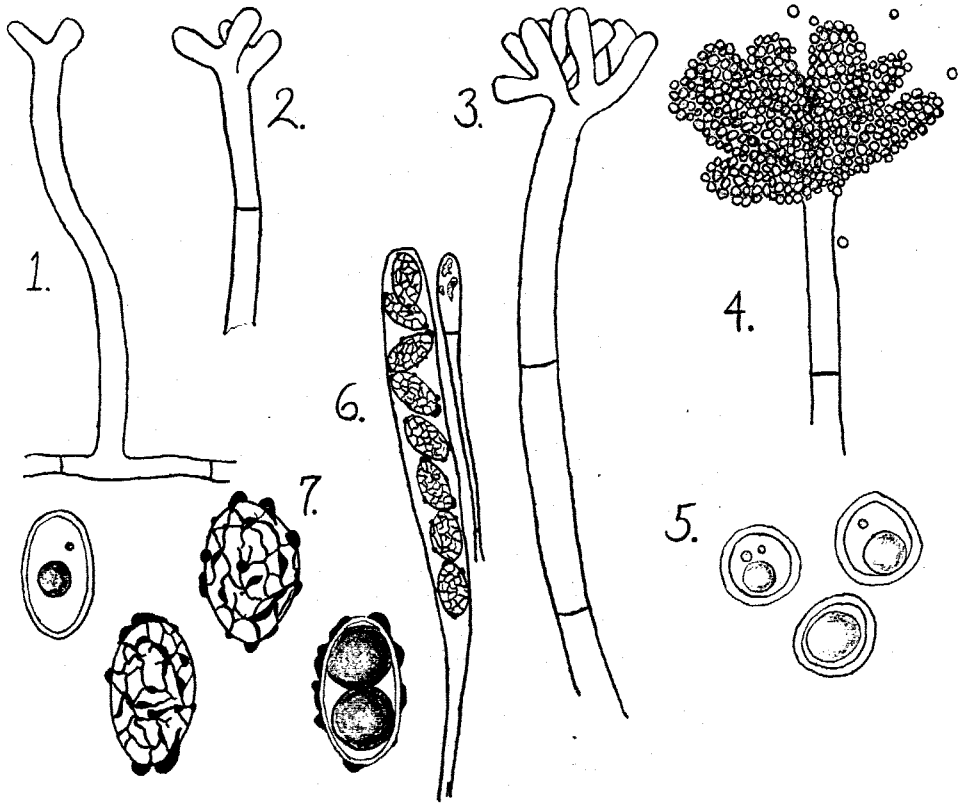

Fig. 2. Drawings of *Galactinia* n. sp., now *Peziza ostracoderma*

the asci and reticulately sculptured ascospores as well as developmental stages of the apically dichotomously branching conidial state. At maturity the conidia are produced on tiny sterigma-like pegs simultaneously over the ends of the finger-like clavulae of the final dichotomies. At that point in time I was already well-aware that conidial states had been reported for only a few operculate discomycetes, both in *Peziza* and in what is now called *Iodophanus* Korf, where the conidial state was referred to the genus *Oedocephalum* Preuss, as well as in the discomycete genus *Trichophaea* Boudier, where the conidial state was referred to the genus *Botrytis*. Fig. 3 is a copy of Vuillemin's plate[3] showing the apothecia of *Peziza asterigma* Vuill. and its conidial *Oedocephalum* state. Unlike *Botrytis*, which has a branched conidiophore bearing small, subglobose clavulae arranged in "botryose clusters" rather like grapes, each with a simultaneously produced set of conidia, here we see an apically globosely swollen, unbranched end that similarly produces simultaneous conidia on pegs. The difference between the two genera thus appears to lie in the way the conidiophore branches or fails to branch, and the degree of swelling of the spore-bearing cells.

Let me return to my undescribed *Peziza*, and to my inability to assign it to any recognizable genus of conidial fungi, since it clearly differed from both *Oedocephalum* and *Botrytis*. Six years later, in 1954, a German worker, Dr. Roswitha Schneider, described[4] this species as new from greenhouses in Berlin, giving it the name *Plicaria fulva* Schneider (Fig. 4). Though she overlooked the characteristic reticulum on the ascospores, easily demonstrated in her type material, she faithfully illustrated the conidial state. She also declined to assign that state to any existing genus of conidial fungi. I had meanwhile consulted several eminent mycologists of the day who worked with conidial fungi, in particular Dr. S. C. Damon and Dr. Stanley J. Hughes. Possible names for it that I had to consider were *Hyphelia* Fries, *Rhinotrichum* Corda, and *Ostracoderma* Fries. One year later, in 1955, Wolf described[5] the identical conidial state from greenhouses at Duke University, North Carolina, as *Mycotypha dichotoma* F. A. Wolf, but *Mycotypha* Fenner is a genus of the Zygomycetes and surely cannot be used for this Discomycete state. I accepted Dr. Hughes' suggestion that *Ostracoderma* was probably the best generic name for this conidial fungus. In 1960 I proposed that Dr. Schneider's discomycete be transferred to the genus *Peziza*, since *Plicaria* Fuckel was inappropriate for a species with ovoid spores. It was impossible however to transfer her epithet, "fulva," because there already exists a very different fungus called *Peziza fulva* Persoon. The new name I gave it,[6] still in use, was *Peziza ostracoderma* Korf to call attention to its peculiar conidial state.

Now let me introduce yet another player in this drama, Dr. Grégoire L. Hennebert, a mycologist about my own age in Belgium, who had been working on the genus *Botrytis* for his 1960 doctorate thesis at the Université Catholique de Louvain. Because of Whetzel's immensely important work on *Botrytis* and on *Sclerotinia*, I was several years earlier than that in frequent correspondence with him, sending him Whetzel's notes and specimens from the Cornell Plant Pathology herbarium,

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[3] Vuillemin, P. 1887. Sur le polymorphism des Pézizes. Compt. Rend. Assoc. Franç. Avancem. Sci. 15: 491–497, pl. 10.
[4] Schneider, R. 1954. *Plicaria fulva* n. sp., ein bisher nicht bekannter Gewächshausbewohner. Zentralbl. Bakteriol. Parasitenk. Infectionskrankh. Hyg., 2 Abt., Allg. Landwirtschaftliche Techn. Mikrobiol. 108: 147-153.
[5] Wolf, F. A. 1955. Another *Mycotypha*. J. Elisha Mitchell Sci. Soc. 71: 213-217.
[6] Korf, R. P. 1960. Nomenclatural notes. IV. The generic name *Plicaria*. Mycologia 52: 648-651.

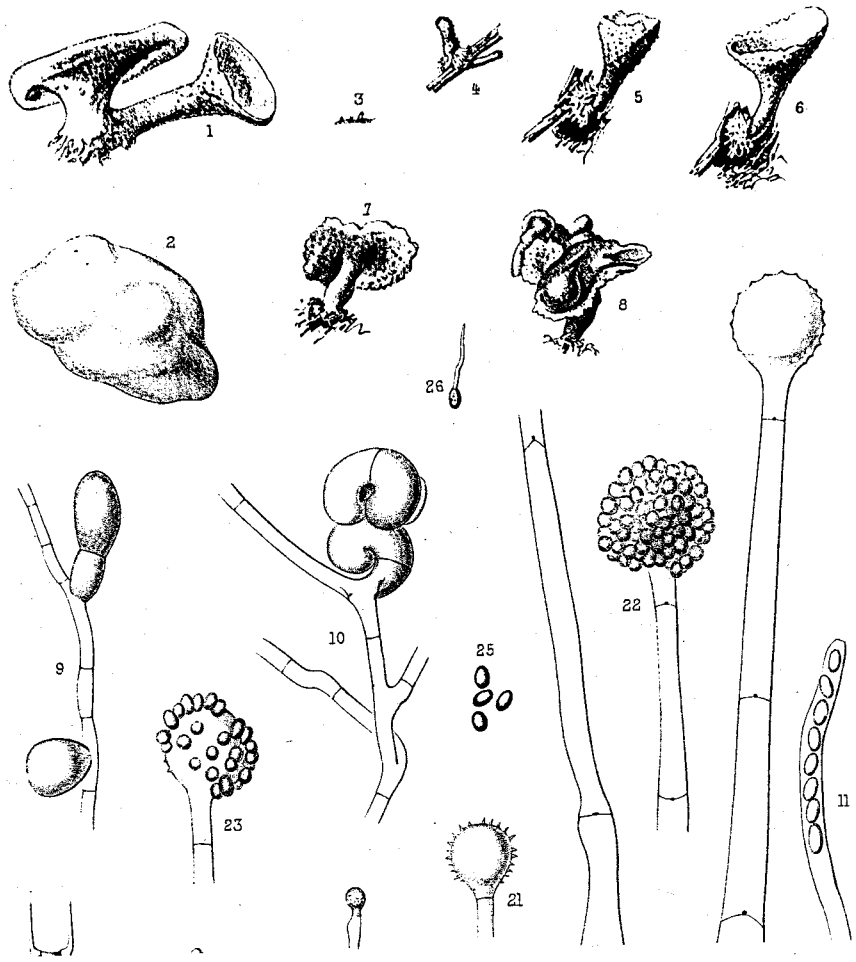

Fig. 3. Part of Vuillemin's (1887) figures of *Peziza asterigma*

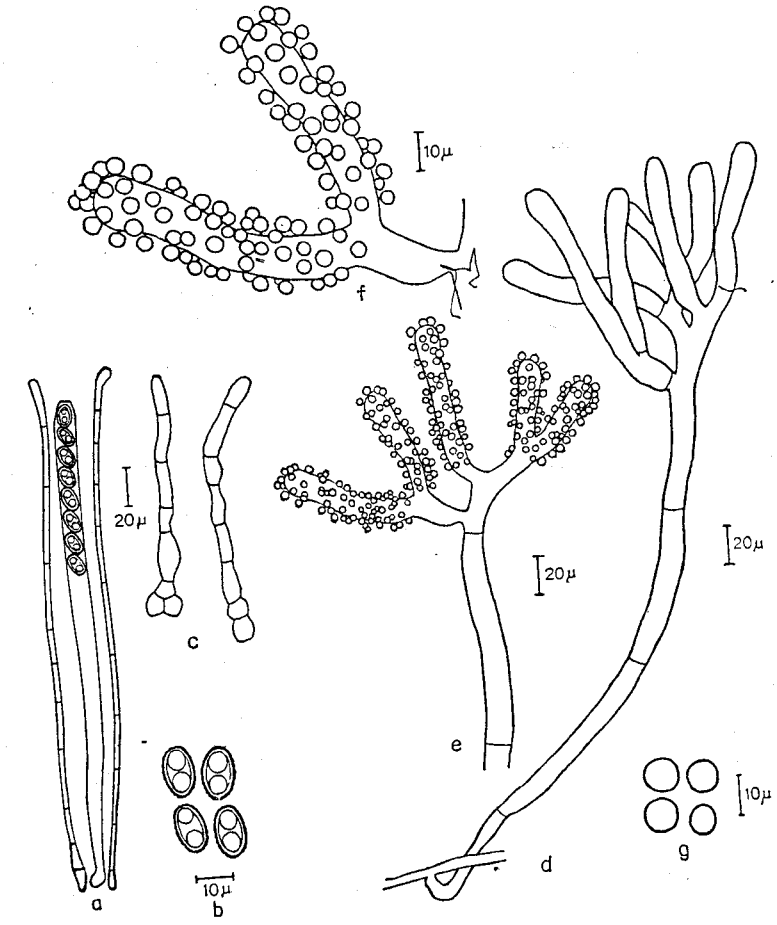

Fig. 4. Schneider's (1954) drawings of *Plicaria fulva*, now *Peziza ostracoderma*

which I curated as part of my duties as mycologist here on my return to Cornell in 1951. In the early 1960's Hennebert came to Ottawa, Canada to study with Stan Hughes, and made the first of many trips to Cornell to consult our herbarium and to work with me. Over 40 years of friendship and collaborative work with him ensued. His intensity is almost unparalleled among mycologists I have encountered, and at times we have worked together days and nights on end without sleep. Such are the rewards of true friendship. Two of our joint collaborations deserve comment here. Our 1975 paper[7] on "the peat mould," that same greenhouse *Peziza* and its conidial state, brought together its tangled synonymies and at last assigned the conidial state to what appears to be its correct name, *Chromelosporium fulvum* (Link) McGinty,[8] Hennebert, & Korf.

The other, much more important paper, is that of 1973 by Hennebert[9] alone, entitled "*Botrytis* and *Botrytis*-like genera," to which I contributed some of my knowledge of the discomycetes, mostly during my stay as a Fulbright Scholar at Louvain in 1972-73. I want to show you some figures from that paper, to orient you on current names for some of these interesting conidial fungi. Fig. 5 reproduces the

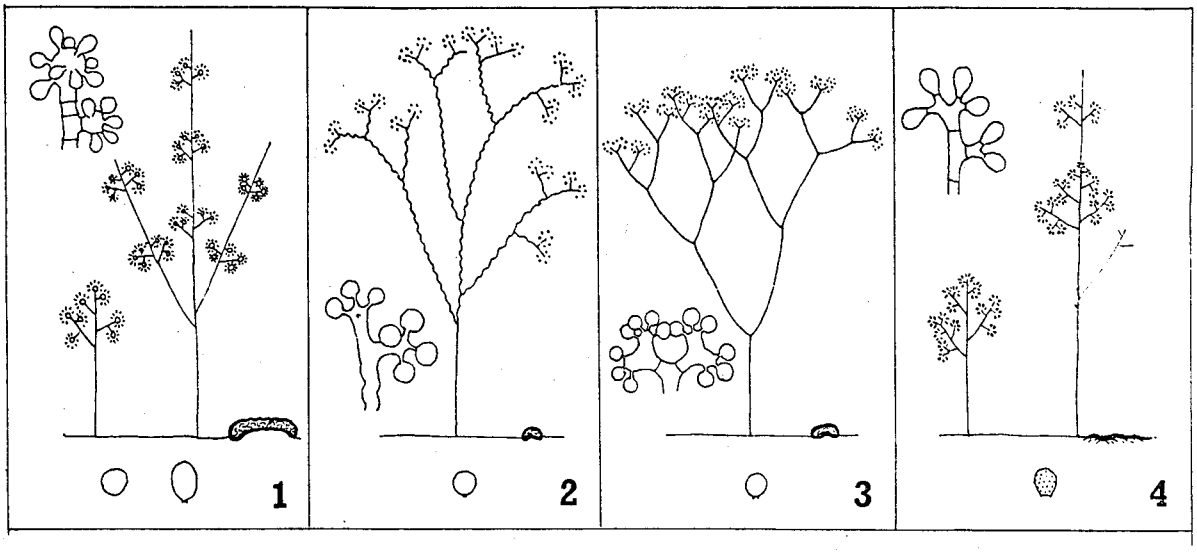

Fig. 5. From Hennebert (1973)

first four figures of his schema, giving you a picture of the conidial states of Sclerotiniaceae, which you will remember are inoperculate discomycetes. (1) *Botrytis* Persoon is the conidial state of most species of *Botryotinia* Whetzel, (2) *Streptobotrys* Hennebert is the conidial state of *Streptotinia* Whetzel, differing in twisted conidiophores, (3) *Amphobotrys* Hennebert is the conidial state of certain species of *Botryotinia*, differing in regularly dichotomously branching

[7] Hennebert, G. L., & R. P. Korf. 1975. The peat mould, *Chromelosporium ollare*, conidial state of *Peziza ostracoderma*, and its misapplied names, *Botrytis crystallina, Botrytis spectabilis, Ostracoderma epigaeum* and *Peziza atrovinosa*. Mycologia 67: 214-240.

[8] Which McGinty that was, we failed to specify. It was, of course, Professor N. J. McGinty, also known as Curtis Gates Lloyd, perhaps speaking from his grave. McGinty was recently honored by having a new journal, *McGintya*, named after him. His career was discussed in a seldom-cited but arguably the late A.P.O. Cryphal's most important nomenclatural paper of 1956, which I brought to mycologist's attention in *Mycotaxon* 6: 193-194. 1977 (see also *Mycotaxon* 27: 628. 1986).

[9] Hennebert, G. L. 1973. *Botrytis* and *Botrytis*-like genera. Persoonia 7: 183-204.

conidiophores, and (4) *Verrucobotrys* Hennebert is the conidial state of *Seaverinia geranii* (the first fungus I ever published on), with verrucose conidia, and no sclerotia formed. In Fig. 6 I reproduce the *Botrytis*-like conidial states that either are

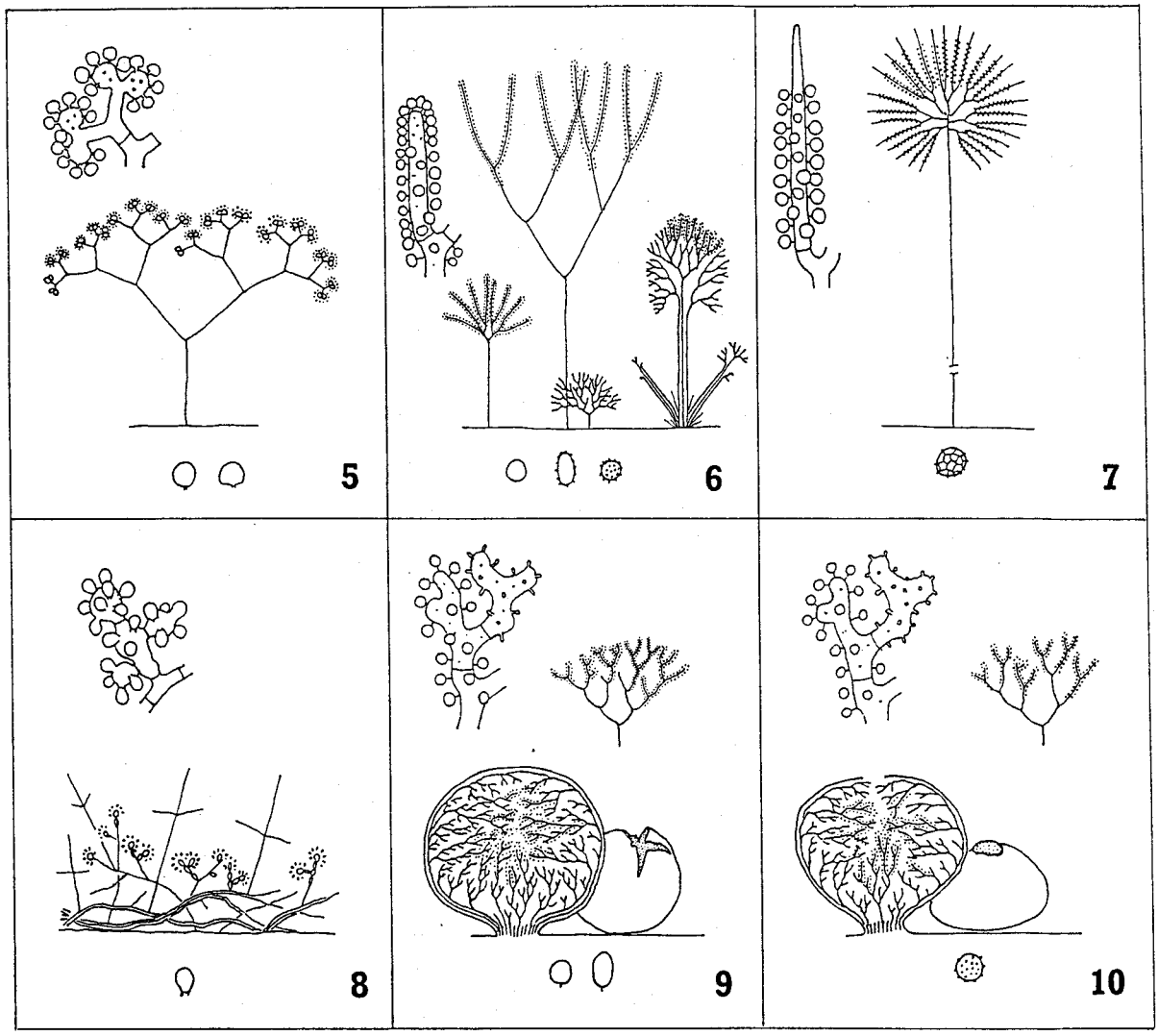

Fig. 6. From Hennebert (1973)

known to connect to operculate discomycetes or have no known connections. (5) *Dichobotrys* Hennebert is the conidial state of species of *Trichophaea* (some authors segregate spherical-spored members of this genus as *Sphaerosporella* (Svrček) Svrček & Kubička which I do not accept as being different from *Trichophaea*), with dichotomously branching conidiophores and almost globular sporogenous cells. A fairly similar conidial state, differing in having echinulate conidia, has been described more recently[10] for another operculate discomycete, *Pyropyxis* Egger, which appears to be a rather distant relative of *Trichophaea*, suggesting that possibly a new genus for that conidial state needs to be considered. (6) *Chromelosporium* Corda is one of the conidial states known for *Peziza* (some

[10] Egger, K. N. 1984. *Pyropyxis*, a new pyrophilous operculate discomycete with a *Dichobotrys* anamorph. Canad. J. Bot. 62: 705-708.

authors recognize the spherical-spored species of this apothecial genus as *Plicaria,* which I have not yet accepted), as well as for another close relative, *Muciturbo* Warcup & Talbot[11] (probably a synonym of the much older *Ruhlandiella* Hennings[12]). Here we see those elongated clavulae bearing conidia on regularly or irregularly dichotomously branching conidiophores presumably now familiar to you. One of the generic names I had considered for the peat mould fungus, *Hyphelia* Fries, is a synonym. Not illustrated by Hennebert is the apparently unnamed though vaguely similar conidial state[13] produced by *Lepidotia hispida* Quél. (also called *Peziza quelepidotia* Korf & O'Donnell). The remaining four genera have no known ascus state: (7) *Pulchromyces* Hennebert is appropriately named (for its translation is "beautiful fungus"), (8) *Phymatotrichopsis* Hennebert is for a fungus with absent to irregularly alternate branching conidiophores previously called *Phymatotrichum* Bonorden, and then the final two to which I draw your particular attention: 9) *Ostracoderma* Fries, which you will recall was the name early suggested to me for *Chromelosporium fulvum,* the peat mould, and (10) *Glischroderma* Fuckel. Both have irregularly dichotomous or coralloid conidiophores, and both have a feature unknown in any other Hyphomycete: the conidia and conidiophores are enclosed in a mycelial membrane. Indeed, they look a bit like puffballs. Hennebert and I term these "peridial hyphomycetes," a type of conidial fructification not mentioned in any of the recent arrangements of conidial fungi.

What are these "peridial hyphomycetes," and with what can they be confused? If we ignore the peridium, of course they closely resemble *Chromelosporium,* Hennebert's (6) in figure 6. But the peridium has seldom been ignored in these fungi, and it has led to some bizarre placements for them.

First, let's look at *Ostracoderma.* The name means "shell-like skin," referring to the smooth, pellicular, brittle peridium. Fries placed the genus in a new family Trichodermataceae, intermediate between Hyphomycetes and Gasteromycetes. Stan Hughes used the name[14] to include not only the peridiate monotype species, but also those recognized by Hennebert as *Chromelosporium,* where no peridium is formed. Three peridiate species, all more recently described under the synonymous generic name *Lycoperdellon* Torrend, were described as Gasteromycetes or puffballs, quite incorrectly as we now know, and the conidiophores were erroneously assumed to be aberrant basidia. Electron microscopic studies and even light micrographs reveal paired Woronin bodies near septa, and this genus must thus be a conidial state of an Ascomycete; M. A. Donk[15] implied that the similarities between *Lycoperdellon* and the conidial state of *Peziza ostracoderma* indicate that *Lycoperdellon* might well be the conidial state of a Discomycete. There is even an invalidly published family name, Lycoperdellonaceae Heim, to include this one genus, and an illegitimate ordinal name, *Lycoperdellonales* Zeller. Zeller mistakenly included within it also the truly basidiomycetous Gasteromycete genus *Leucophleps* Harkness, and placed his new order in the Fungi Imperfecti.

[11] Warcup, J. H., & P. H. B. Talbot. 1989. *Muciturbo:* a new genus of hypogeous ectomycorrhizal Ascomycetes. Mycol. Res. 92: 95-100.

[12] Dissing, H., & R. P. Korf. 1980. Preliminary studies in the genera *Ruhlandiella, Sphaerosoma* and *Sphaerozone* (order Pezizales). Mycotaxon 12: 287-306.

[13] Korf, R. P. 1973. On Boudier's genus *Lepidotia* (Pezizaceae). Persoonia 7: 205-212.

[14] Hughes, S. J. Revisiones Hyphomycetum aliquot cum appendice de nominibus rejiciendis. Canad. J. Bot. 36: 727-836.

[15] Donk, M. A. 1962. The generic names proposed for Hymenomycetes—XII. Deuteromycetes. Taxon 11: 75-104.

Now, then, let's look at the second of these genera, *Glischroderma*. It, too, was based on a single species, *G. cinctum* Fuckel, which was also placed in the Gasteromycetes. No other species has ever been described in the genus, but it is the basis for a monospecific family Glischrodermataceae R. Heim, and an order Glischrodermatales Malençon. The species has been rarely encountered: it was distributed in 1863 from a firesite collection in Germany, and described[16] in 1870. Fig. 7 is a copy of Fuckel's rather poor illustration. In 1909, 1910, and 1912 the next known collections were taken by Carlton Rea in two British localities, on charcoal heaps. Rea commented[17] on the appearance: "At first sight this species looks exactly like *Lycogala epidendrum* seated on a circular mass of white mycelium." He also referred to the peridium as "at first pale grey and slightly sticky." E. A. Rea and G. Lister illustrated his material (Fig. 8), the first to show the branching of "the capillitium," but which we now know to be the conidiophores. Note, too, the

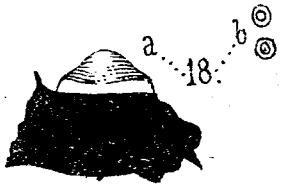

Fig. 7. From Fuckel (1870)

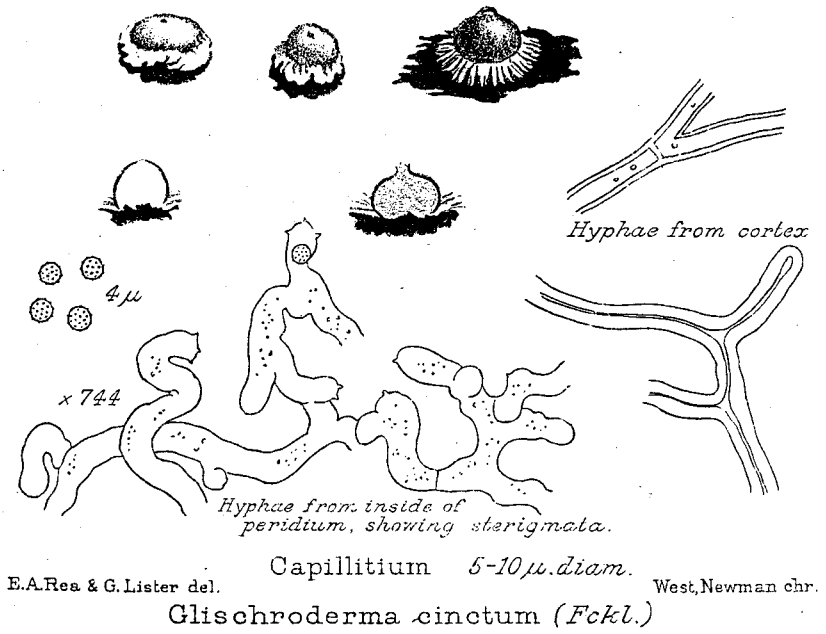

Fig. 8. From Rea (1913)

apical pore they illustrated on the peridium. Another collection, also taken in 1912, from Belgium, was finally reported in 1966 by Demoulin[18], again on old ashes. Apparently the only other collection ever reported of this species, this time found in Morocco in 1960, was discussed by Malençon[19] in a well-illustrated, seminal paper that proved beyond doubt that this is no Gasteromycete, but another peridial conidial fungus. Like Rea, he illustrated an apical pore on the peridium, which was not noted by Fuckel, and again the collection came from an old firesite. Fig. 9 is a part of his illustration of the developing fructifications, like all the others you have seen rather hemispherical, and Fig. 10 is his drawings of the developing conidia on the conidiophores, showing the nuclear condition. Note the warted conidia, possibly the only character, according to Hennebert[9], which would distinguish this genus

[16] Fuckel, L. 1870. Symbolae mycologicae. Jarhb. Nassauischen Vereins Naturk. 23-24: 1-459, 6 pl.
[17] Rea, C. 1913. *Glischroderma cinctum* Fckl. Trans. Brit. Mycol. Soc. 4: 64-65, pl. 2 (lower).
[18] Demoulin, V. 1966. Un Gastéromycète remarquable de la flore belge: *Glischroderma cinctum* Fuck. Naturalistes Belges 47: 404-406.
[19] Malençon, G. 1964. Le *Glischroderma cinctum* Fuck., sa structure et ses affinités. Bull. Soc. Mycol. France 80: 197-211.

from *Ostracoderma*, with smooth conidia.

By now you must be wondering why has Korf gone on so long about these peridial hyphomycetes? Well, the reason is that I wanted to bring you up to speed with a little piece of my current research. Just five weeks and two days ago I was leading my mycology class in the field when a student handed me a most peculiar, slimy fungus growing on dead leaves in a boggy area of the Lost Gorge, at Alpine, about fifteen miles from here, asking me "what's this?" Figs. 11-12 are photographs of the collection, consisting of a few fruitbodies, looking rather like aethalia of an odd slime mold, perhaps *Lycogala epidendrum* (L.) Fr., but on cutting open with a white interior. By now you've guessed it: this is no slime mold, for it is composed of hyphae, and the interior is filled with typically branching clavulae and warted conidia. The peridium was markedly sticky and greyish; not being a Greek scholar, it was only when I looked up the derivation of the generic name, "sticky skin," that I realized that the glutinous peridium was another character which may serve to separate *Ostracoderma* and *Glischroderma*. This then might mark the sixth known locality, and the

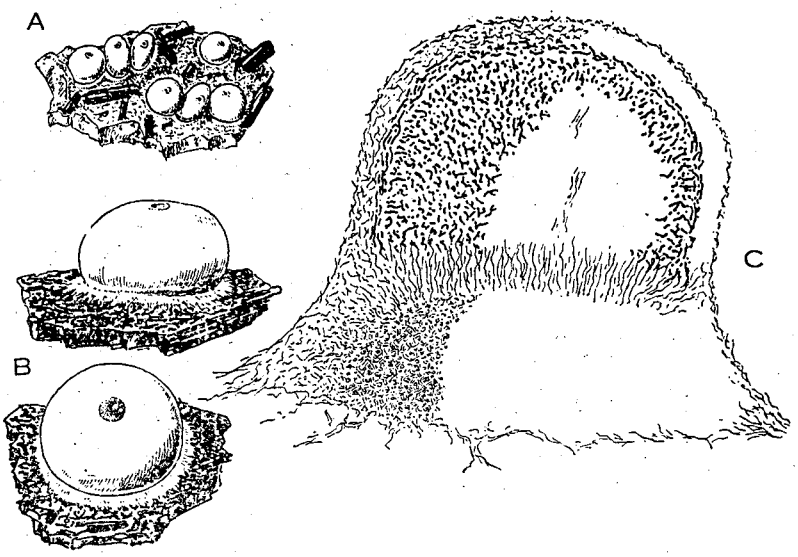

Fig. 9. Part of Malençon's (1964) illustration

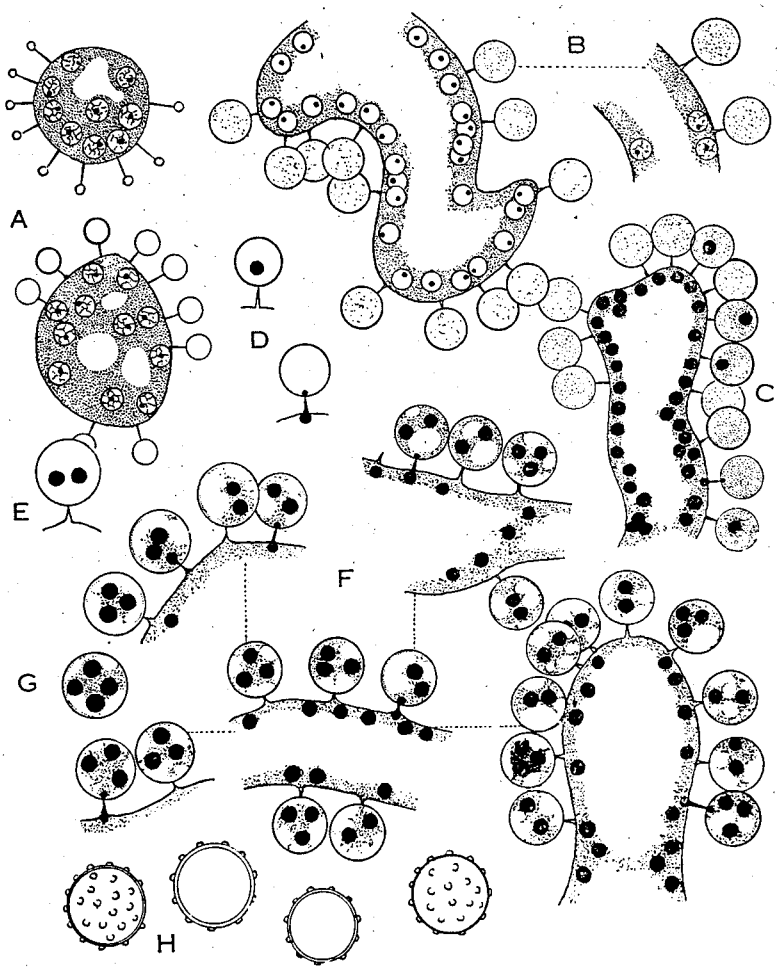

Fig. 10. From Malençon (1964)

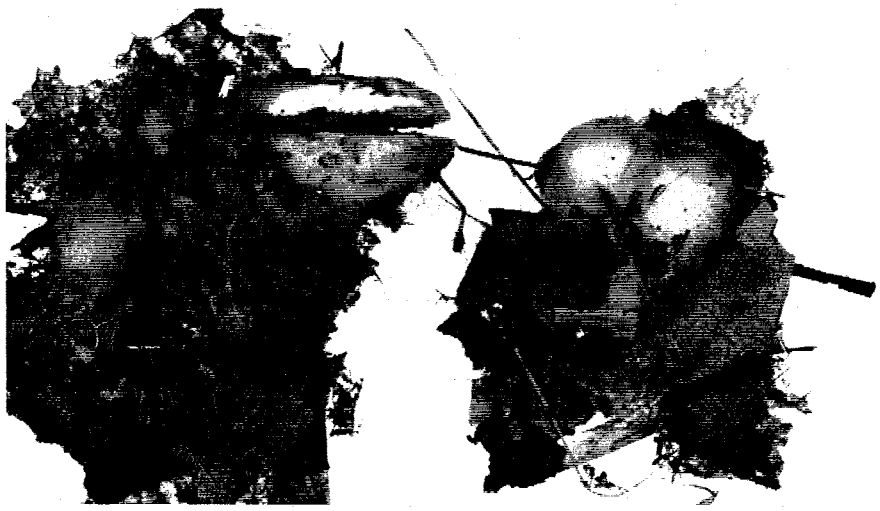

Fig. 11. CUP *62646*, × 2, the largest fruitbody sectioned and half turned upwards

Fig. 12. CUP *62646*, × 6 (approx.), viewed in section, showing the glutinous peridium both above and below the conidiogenous region

first new world locality, for Fuckel's species. What seemed a little odd was that the fruitbodies are not hemispherical, as illustrated by all authors, but far more appressed. Also this was not on a firesite. It was collected in just the place and on the kind of substratum where I would look for (and had found the same day) species of *Chromelosporium*, one of the conidial states of *Peziza*. The thought obviously crossed my mind: can this, too, be a conidial state of one of the bog-inhabiting species of *Peziza*? I (and, independently, two of my graduate students) attempted to culture this fungus, both from the conidia and from the peridial hyphae, but apparently without success. Hennebert[9] had noted that all reported attempts to culture *Ostracoderma* were unsuccessful, and that nobody had ever tried to culture *Glischroderma*. Apparently it, too, is reluctant to enter into culture (as, indeed, are a rather large number of *Chromelosporium* species). A return a few days later to the Lost Gorge by one of my students and me yielded no additional material of this fungus.

On the field trip the following week my students and I tried unsuccessfully to find it. But the next week on September 28th a nice collection, somewhat different-appearing in lacking much of a glutinous appearance to the peridium, pinkish rather than pale orange, and strigose or even scaly, was brought in from Michigan Hollow, near Danby, illustrated in Figs. 13–15. It again has the conidiophores and conidia of a *Glischroderma*. The fungus enmeshes parts of twigs, stems, etc. within

the developing fruitbody, and it is clear that the peridium eventually ruptures (Fig. 15) to expose the powdery spore mass and conidiophores, even though there is no apical pore as illustrated by Rea and by Malençon from British and African material. The following week, October 5th, our field trip was to Malloryville Bog, also known as Eames Bog, and we obtained two more collections, one huge one by my teaching assistant, Kathie Hodge, shown in Figs. 16–18. And just last week Wednesday, October 12th, on our last scheduled field trip, we found three more collections here in Coy Glen, just outside the city of Ithaca limits. Now that we know what to look for, it's everywhere!

How could I have missed seeing a fungus this common all these fifty years of collecting? All I can tell you is that, as you have seen, it really can look pretty uninteresting in the field. I'm convinced I must have seen it often, and merely ignored it, thinking it to be some undeveloped Basidiomycete, perhaps a young stage of a *Sebacina* Tulasne & C. Tulasne, maybe even *Sebacina incrustans* (Pers.) Tul. & C. Tul., a fungus that often climbs up the stems of plants in the woods in autumn, and that is notariously difficult to demonstrate basidia in. Of course now I want to know what this apparently very *common* fungus really is. It certainly must be a species of *Glischroderma*. One question is can this be Fuckel's species? It differs in an apparently significant ecological feature, not being on firesites and charcoal, and morphologically in two features, in being much thinner and thus not at all hemispherical, and in definitely lacking any apical pore. The second question is that if it represents a new taxon, should that be a new species or should I recognize it at some infraspecific rank, perhaps a subspecies *incrustans* of *G. cinctum*? Here is a case where DNA fingerprinting could certainly help me out, clearly the case if old herbarium collections of Fuckel's species from Europe can still yield good DNA. And, further, by comparison of molecular data from our collections with similar data from species of *Peziza*, I might well conclude whether this is, as I believe, a conidial state of an as yet unconnected *Peziza* or not. I am enlisting the help of one of my colleagues, Keith Egger, with such DNA data on *Peziza*, and have sent freeze-dried material of the American *Glischroderma* for him to analyze. Then, too, I'll have to make a decision as to whether *Ostracoderma* and *Glischroderma* should be treated as the same genus or as two. If the two generic names are to be synonymized, then our American material would instead become a species of *Ostracoderma*, the older generic name. Once more, DNA studies could be helpful.

So much for the last six weeks of my current research. You'll see I haven't come very far from my first report on Discomycetes to the national meetings in 1948. You could conclude that I'm almost back to square one, still unsure of the generic name I should apply to a *Botrytis*-like conidial fungus.

To round out this talk, I'm going to peer into the future, to see what I think will be done, to guess how mycology will develop, and to make a plea for what I think is important, both here at Cornell and in a worldwide perspective as well.

As most of you know, I was trained as and claim to be a mycological taxonomist, that is to say someone interested in discovering what kinds of fungi are out there, and how they may possibly be arranged in some system that both reflects their presumed phylogenetic history and provides keys that will enable others to be able to recognize them when they are again encountered. I'll not bore you with the

Fig. 13. CUP *62647*, × 1.25

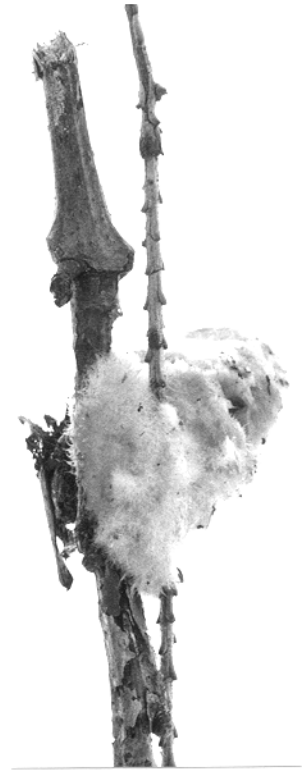

Fig. 14. CUP *62647*, × 2.25

Fig. 15. CUP *62647*, × 1.7

Fig. 16. CUP *62651*, × 0.98

Fig. 17. CUP *62651*, × 0.55

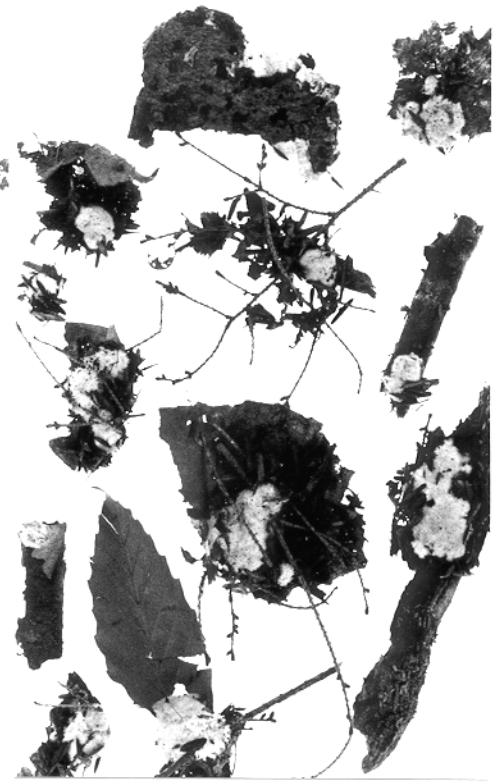

Fig. 18. CUP *62651*, × 0.52

semantic differences between a taxonomist and a systematist—if they differ at all. What I want you to know is that taxonomy has had an increasingly bad press in most of the last 50 years, particularly as new techniques and new ideas have demonstrated on the one hand how poor our classifications are, and on the other hand have been held to be irrelevant to the problems that need attention. If you will, taxonomy and taxonomists fell out of favor. The hopeful news is that within the last few years we have seen a major shift in attitude, particularly by those scientists concerned with biodiversity and with the loss of species worldwide. Suddenly we are aware that we don't know what plants and animals we have on this planet, and how fast these are disappearing as the great rain forests of the tropics rapidly fall to development. Yet I challenge any of you to find statistics on how many *fungal species* are being lost—somehow the debate centers on plants and animals, and the fungi get left out of the equation. If we are to believe the most recent estimates on numbers of fungal species worldwide, the figure stands at 1,500,000, considerably higher than the number of plant species of all "plant" groups. And the other challenging statistic now being widely accepted is the estimate that of those 1,500,000 species, we have described so far not more than 4 to 5% of them. To do a "species inventory" for any area of the world, however small—projects now on the international drawing boards—we will be faced with an immense need to collect and to identify those uncataloged species of fungi, to document their presence, and to extrapolate on how many of these we lose each day to advances of technological civilization. Suddenly we need an immense army of mycological taxonomists willing to devote time to identifying fungi and to preparing and publishing monographs.

My crystal ball sees no such development in manpower, for even as I speak here we see institution after institution, in this country and worldwide, either losing their mycology positions entirely, or replacing the taxonomic mycologists who have retired with others who are at one of "the cutting edges" of taxonomy.
What are some of those "cutting edges"? During my time as a taxonomist I have seen many that were so considered: genetic mating systems, the "new systematics," numerical taxonomies based on phenology, the chemistry of carotenoid pigments, isozyme work, ecology, and particularly the techniques of transmission electron microscopy, allowing us to look at micromophology, all these took center stage for a period of time. The development of another micromorphological tool, the scanning electron microscope, which produces seductive three-dimensional representations, in the fungi most typically of sculptured spores, has led to a new wave of presenting "modern" illustrations. Each of these newer *techniques*, and I emphasize that each is fundamentally a technique, has siphoned off the attention needed for producing useful monographs. Even the comparatively newer techniques of cladistic analysis (often equated with phylogenetic systematics) and molecular analysis of DNA, now at the forefront of fungal taxonomic studies, though yielding immensely interesting hypotheses on relationships, have decreased rather than increased the production of monographs. Those studies address fundamentally interesting questions on relationships, but at the price of failing to provide tools for other workers to use, namely monographs that are able to be applied in our quest for knowledge of that 95 or 96% of the fungi we haven't yet catalogued.

I do not mean to denigrate cladistics or molecular work. I've already indicated my real hope to use these techniques in assessing some of the questions I am interested

in, such as the real place of *Glischroderma/Ostracoderma/Peziza* in phylogenetic classification. If persons using these tools know the questions to ask, then we can expect fascinating results that are at least of great theoretical interest, even if not of great practical interest.

My crystal ball sees a future in which fewer and fewer mycologists will have classical taxonomic training in mycology allowing them even to ask the fundamental questions. I see a continuing trend toward the disappearance of mycologists who can identify a fungus, in large part because there are fewer and fewer mentors able to provide the background they need. As useful as cladistic methods are, and despite the controversies that exist within the discipline on how to use this new philosophy, immense energy and time can be consumed in attempting to decide amongst competing hypotheses of evolutionary relatedness that have been generated by computer programs, at the expense of time devoted to delimiting taxa and producing monographs. In Africa, tropical Asia, and South America the forests will continue to recede, and mycologists to record species diversity in such areas will not have been trained either indigenously, or, as has long been the tradition, in temperate centers for mycological taxonomy.

I sincerely hope my crystal ball is clouded, and that the bleak picture I have painted is all wrong. Maybe, just maybe, university administrators, department search committees, and the National Science Foundation and similar funding agencies will see that priorities need to be reassessed. Molecular techniques are simply immensely expensive compared to funding collecting trips to the unexplored areas of the world, and to documenting our flora, our fauna, and our mycota. Maybe, just maybe, recognizing and describing all that unknown 95 or 96% of our mycota will be just as "cutting edge" as some of the "sexier" new techniques appear to be.

To conclude, there is a brighter side to the picture I see. The crotchety museum taxonomist being replaced by a technician matching gene sequences from a fraction of the ribosomal DNA extracted from a fungus he or she would not recognize in the field—is only one scenario.

I have some words of advice to the fledgling mycologists studying today. First off, let's agree that there is lots more to study about the fungi out there than their taxonomy. Merely look at the most recent issues of, say, *Mycologia* and *Mycological Research,* and you will discover the breadth of our subject and the detail into which mycologists delve. The whole, closely related discipline of Plant Pathology has a mycological heritage, since a very large proportion of diseases are caused by fungi. Plant Pathologists cannot, however, function effectively if the organisms they study have no sound, phylogenetically derived, classification. Certainly one can spend a lifetime studying in detail one disease, one host, and one pathogen. But if the science of phytopathology as a whole is to prosper, the knowledge gained from such studies must be able to be plugged into the broader picture. And that is where taxonomy comes in. For the whole aim of taxonomy is to provide a fundamentally sound, phylogenetically based classification which allows *prediction*. Without such a *predictive classification* we would waste time and be forced to "reinvent the wheel" for each species we want to study.

At this point you'll realize I've already donned my preaching robes. What I want to say to budding mycologists is: if I can't convince you to become a taxonomist or to produce those essential monographs, I'll forgive you. As far as the Discomycetes are concerned, I've already published a brief paper[20] in 1991 outlining some of the unanswered questions I'd like to see attacked. I want to emphasize that what *you* need to do is to find role models. These need not be living persons with whom you can interact—start with the literature and find out who did what, why, when, and where. Maybe you'll be intrigued by the bizarre things fungi do. Read about the marvellous questions posed by people like A. H. Reginald Buller, or C. T. Ingold, and follow in their footsteps. Or if micromorphology is the tool you see as providing answers, for indeed workers have spent decades doing TEM studies of ascus apices, or ascus bases and their subtending septa, ascus wall structure, ascospore wall delimitation and layering, then choose a role model from among such workers as Jim Kimbrough, Joop van Brummelen, André Bellemère, and Franz Oberwinkler. If you are caught up in molecular work and cladistic analysis, find your role models among Ascomycete workers like Keith Egger, Linda Kohn, Ove Eriksson, and Meredith Blackwell, or Basidiomycete workers like Rytas Vilgalys, David Hibbett, and Jim Anderson. If, by chance, you decide there is still room in your future for classical taxonomic mycology, look to role models like Richard K. Benjamin, Stan Hughes, Emil Müller, and Leif Ryvarden. Learn that we now have new understandings of the reagents we typically used when examining dried herbarium specimens, that iodine reactions reported in the literature are now suspect, that potassium hydroxide solutions as mounting media have done more than reinflate hyphae to something like their original size and shape, they have also cost us valuable information, such as the presence or absence of gel sheaths, gelatinous apiculi, and even gross ornamentations on spores or hyphae, information that is frequently lost when using this convenient mountant. And by all means heed the clarion call[21] of Hans–Otto Baral for a return to light-microscopic study of *living* fungi, and for the need to develop what he has aptly termed "vital taxonomy." Of necessity also we must attempt to culture as many of the fungi we collect as possible, to learn about whether they have conidial or other anamorphic states not evident in the fruitbodies we collect, and to deposit cultures in permanent collections of living and lyophilized fungi as a genetic resource base for retaining some of the known fungal biodiversity.

Students, welcome to the fungi: they have more unanswered questions than any of us can ever answer, even for a few species or a single genus.

Thank you for your attention.

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[20] Korf, R. P. 1991. Discomycete systematics today: a look at some unanswered questions in a group of unitunicate ascomycetes. *Mycosystema* 3: 19-27.

[21] Baral, H.-O. 1992. Vital versus herbarium taxonomy: morphological differences between living and dead cells of ascomycetes, and their taxonomic implications. *Mycotaxon* 44: 333-390.

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