NOMENCLATURE COMMITTEE FOR FUNGI

2 February, 2008

Commenting procedures

All proposals for our action are numbered by IAPT as published in TAXON, and we assume that all members have TAXON available in its entirety. As Secretary, I will send whatever references are necessary (either articles in TAXON or elsewhere) to those who lack **and** request them. **Commentaries** contain the accumulated responses sent to the Secretary by CF members who wish to comment on TAXON proposals or a previous commentary. Before emailing input to the Secretary, members should edit for clarity & brevity and separate comments according to proposal number. Members are also *encouraged* to forward any **published** comments regarding proposals to the Secretary, who will forward them to all CF members. The Secretary will invite guest experts to provide background comments on more complicated or controversial proposals: these comments will precede member comments. Commentaries are emailed to Committee members as PDF attachments at least four times a year and will be posted on the CBS Nomenclature website http://www.cbs.knaw.nl/nomenclature/index.htm. Discussions are cumulative, so that members can safely refer to the most recent commentary, which will contain all discussion on a topic up to that date. Text pertaining to proposals that have been recommended or rejected by the Committee after a ballot will be removed from future commentaries.

Discussions are presented in three sections in the following order: PROPOSALS TO CONSERVE OR REJECT NAMES, PROPOSALS TO AMEND THE CODE, and GENERAL REMARKS (if present). Proposal sections are organized numerically by proposal, with each proposal topic headed by proposal number, informal title, and reference citation. Where votes have been held, ballot tallies still appear in the second paragraph (see Ballots, below). When necessary, I will assign temporary CF numbers to topics or unassigned proposals; these discussions will stand at the end before GENERAL REMARKS and remain until reported in TAXON.

Member comments are assigned alphanumeric codes that begin with the first four letters of the member's name and end with a date-based number in YMMDD format that codes the date a comment was sent to the Secretary. For each proposal, comments are arranged in chronological order according to submission date. Comments circulated in previous commentaries are displayed in 10-pt font; new comments are displayed in slightly larger (11-pt) font with an arrow (→) flagging the member code on the first line. Guest comments follow the guest's name, location, and YMMDD

Ballots will be held two or more times as year as needed. Commentaries include a running tally of all Committee votes, which are reported as YES : NO : MORE DISCUSSION NEEDED : ABSTENTION. A 60% majority (now **9!**) of the whole Committee must vote YES or NO to remove a proposal from discussion. Members are urged to return every ballot and vote on every ballot issue, even when abstaining. Members "who, for three times in sequence over a period of nine months or more, fail to respond to requests for voting or subsequent reminders, are considered to have resigned" and will be replaced. Any qualified request for more discussion may outweigh a majority decision of the Committee and the respective proposal will remain open for further discussion.

PROPOSALS TO CONSERVE OR REJECT NAMES

Prop. 1708, to conserve the name *Perenniporia* against *Physisporus* with a conserved type (*Basidiomycota*). Proposed by Cony Decock & Joost Stalpers. Taxon 55(1): 227.

[See also Decock & Stalpers, 2006, Taxon 55(3): 759-778 for a detailed discussion of the taxonomy.]

Voted May 1, 2007: 13 : 1 : 1 : 1. Although this has been supported by 81% of the Committee, this proposal will not yet disappear from the Commentary by request of the Chair (see DEMO 50402).

Vote change January 2, 2008: 14 : 1 : 0 : 1. With the request for further discussion removed (see DEMO 80102), this proposal has been forwarded to the General Committee as recommended and will be removed from our lists.

Committee comments:

NORV 60530: The authors provide excellent support for conserving *Perenniporia* Murrill against *Physisporus* with *Perenniporia medulla-panis* as the conserved type. I support this proposal.

REDH 61130: This made a lot of sense and certainly clarified a very messy uncertain situation in order to stabilize the usage of the name. It would do no good to go back to using the very confusing *Poria*. I support the proposal.

REDH 61206: I just noticed an error in Prop. 1708 that I should have picked up on earlier (when editing it). In their proposal they propose to conserve *Perenniporia* with *Perenniporia medulla-panis* (Jacq. : Fr.) Murrill as conserved type. However, Murrill did not make that combination. It should instead have been written, *Perenniporia medulla-panis* (Jacq. : Fr.) Donk (Persoonia 5: 76. 1967). The authors correctly treated the authority (p. 768) in their longer nomenclatural paper in Taxon 55(3): 759-778. 2006.

Should the proposal pass we will need to put down the correct authority. This is a case of conservation under Art. 57, although that is not spelled out. Please include these comments in the circular.

NORV 61215: Redhead (REDH 61206) correctly notes that the proper name for the proposed type should be *Perenniporia medulla-panis* (Jacq.: Fr.) Donk.

GAMS 70223: 1708 is certainly needed and the fungus is rather well-known. Re Redh 61206: Donk used the correct generic spelling *Perenniporia* when he made the combination in 1967, thus no [sic]. [see NORV70223 below.] I do not understand why this should be handled as rejection under Art. 57 and not simply as conservation under Art. 14.

MAY 70228: The authors suggest two ways to deal with the situation around *Perenniporia-Poria*. One is to conserve *Poria* Pers. over *Poria* Adans. They mention that *Poria* has been proposed for conservation several times in the past. This in indeed the case, but those proposals were not necessarily rejected. Certainly Donk's 1941 proposal to conserve *Poria* was withdrawn, rather than rejected, when it was realised that Gray's use of *Poria* was post 1821 (and hence there was no need to protect *Poria* "Pers. per Cooke" against *Physisporus*). *Poria* is referred to as a 'waste basket'. Other large genera of the older works that are now recognised in a much restricted sense include *Agaricus* (for a while referred to as *Psalliota*), *Polyporus*, *Hydnum* and *Thelephora*, and we live with the fact that there are hundreds to thousands of epithets in these genera that no longer belong in the strict sense. As far ago as 1964, Wright did the right thing in taking up *Poria* in a narrow sense. However, the choice comes down to usage. It is not specifically stated in the proposal how many current floras and so on use *Perenniporia* as opposed to *Poria* sens. str., which would be useful in making a decision.

In addition to numerous works by Decock, I could find *Perenniporia* used by Corner (Ad Polyporaceas), Ryvarden & Gilbertson (European Polypores), Quanten (Polypores Papua New Guinea) and Ryvarden (Genera Polypores), while *Poria* does not seem to have been used in a restricted sense since Ginns (1984: *Mycotaxon* 15). This usage does carry significant weight in leaning towards conservation of *Perenniporia* with conserved type against *Physisporus*. It might be a good idea to reject *Poria* Adans., which is something quite different, so it cannot return under any guise.

PENN 70321: I support the Proposal, subject to the correction of the type citation. The utility of preserving the widely established usage of *Perenniporia* is an over-riding argument.

REDH 70329: Jim Ginns used *Poria* Pers. sensu stricto in our Canadian "Compedium of plant disease and decay fungi in Canada 1960-1980" but that is no longer supportable given the earlier *Poria* Adans. The USDA "fungi on plants and plant products in the United States" went the other route and adopted *Perenniporia*. Having looked at *Poria* Adans. and the cited illustration, I agree with Decock that it is difficult to say what it represents, but certainly not anything congeneric with *Perenniporia.* Therefore, rejecting it against *Perenniporia* would not prevent its usage in the future. Therefore, a proposal to reject it as a stand-alone must be published, if that is the desire of the committee. It should not affect the current proposal, however.

DEMO 70502: Since February I hardly could get out of University work. I am very sorry I have not yet commented on some proposals, especially those dealing with polypores, a subject I am directly concerned with. I would urge a vote is postponed on 1708, for if it is useful to conserve *Perenniporia,* there are problems in the way it is proposed and this has in part been said by Gams and May.

→**DEMO 80102:** I asked for more discussion for I was not fully satisfied with the supporting paper (Taxon 55(3): 759-778, 2006). The major issue is the treatment of *Polyporus unitus* Pers. : Fr., the current type. First the authors ignored its sanctioned status (in Elench. Fung. I: 116, 1828) , which in case of synonymy with *P. medulla-panis* would have given it precedence and

anyway led to envision a typification following Fries (Art. 7.8). Second they did not try to search for Mougeot material, either in distributed exsiccati or its original herbarium in MPU. The isotype in Bresadola's herbarium (S) has the advantage it is probably a kleptotype from the holotype. However it is a poor specimen and locating a larger part of the original collection might allow a better interpretation.

I have looked for a possible type of *Polyporus unitus* that could be Friesian instead of Persoonian. This could have possibly existed in UPS, since Fries in the Systema mentioned he had seen specimens from Sommerfelt and Mougeot. I contacted Svengunnar Ryman and he informed me that Fries herbarium contained two collections of *P. unitus* but one from Norway was from Blytt and the other one was from St Petersburg, sent by Weinman. Ryman also mentioned that a small possibility exist that Fries had seen the Sommerfelt and Mougeot collections in Copenhagen. I have not yet investigated this possibility, but the identity of *Polyporus unitus* is not essential to the proposal. I also contacted the curators in Montpellier for Mougeot and they promised to enquire but I still have not the answer. Thus at the moment I still have not located another collection than the S one. Since anyway it is better to have *Perenniporia* conserved against *Physisporus* and have it with as conserved type *P. medulla-panis*, I accept the proposal. I also support the epitypification. I thus will vote for the proposal. It can thus be considered accepted without a new vote.

Prop. 1719, to conserve the name *Spilomium graphideorum* with a conserved type to safeguard the current usage of the generic names *Lecanographa* and *Milospium* (*Fungi*). Proposed by David L. Hawksworth. Taxon 55(2): 528–529.

Voted May 1, 2007: 14 : 0 : 0 : 2 : 0. With 88% CF support, this proposal will disappear from future Commentaries. However, the following comments have been taken into consideration in my recommendation that the orthographic recommendation below be made when the species is listed in the Melbourne *Code*. (See Committee for Fungi Report 14).

GAMS 70410: although all the literature concerned speaks of *graphideorum,* I strongly recommend to take the opportunity of this conservation to correct the epithet linguistically to *graphidearum,* the genitive plural of *Graphideae,* to which the name obviously refers.

→**REDH 80104:** Although we voted to approve this, Walter Gams raised the issue of orthography (*graphideorum* vs. *graphidearum*). Does the proposer, David Hawksworth agree with Walter's suggestion? Does the CF agree then to make this change? If both Walter and David agree, I suggest we put forward the "correction".

Prop. 1732, to conserve the name *Pseudocercospora* against *Stigmina* and *Phaeoisariopsis* (*Hyphomycetes*). Proposed by Uwe Braun & Pedro Crous. Taxon 55(3): 803.

Voted May 1, 2007: 10 : 0 : 5 : 1 : 0. With 63% CF support but with five votes for discussion, this proposal is temporarily retained for comment.

Committee comments:

REDH 61130: This is one of the more controversial proposal and I am withholding judgment until I hear more from committee members more familiar with these fungi and genera.

NORV 61215: Although the proposers' statement that "it is inevitable that *Stigmina, Phaeoisariopsis* and *Pseudocercospora* be merged" might be challenged by those holding different taxonomic opinions, their proposal to conserve a unified genus under the name *Pseudocercospora* is a pragmatic and welcome solution. Nonetheless, I need opinions from members who better understand the ramifications of such a proposal before making a final decision.

GAMS 70223: Pedro Crous just informs me that the synonymy of the 3 genera is perfectly ascertained by cultural and molecular studies of the respective type species. While other genera of the cercosporoid fungi are still being debated, this situation seems perfectly settled. Other species placed in *Stigmina* must go into different genera. *Pseudocercospora* has become a very large and well-established genus.

PRIN 70301: As far as I can see, a central argument for the merging of *Pseudocercospora, Phaeoisariopsis* and *Stigmina* is the phylogenetic tree presented by Crous et al., Stud. Mycol. 55: 165. On that tree, members of the three genera form a moderately supported monophyletic group. Within this group, which consists of only five species, there is a single, moderately supported node combining three samples of *P. griseola*. All other nodes within the clade are unsupported. If there really are more than 1000 species in *Pseudocercospora*, I cannot see that it is "inevitable that *Stigmina, Pseudocercospora* and *Phaeoisariopsis* be merged". With a different (more comprehensive) taxon sampling, and a more comprehensive sampling of gene loci, *Stigmina* and *Phaeoisariopsis* might well end up as well supported sister groups to *Pseudocercospora*. Whether or not the anatomical details outlined by the authors support that the genera are merged, is beyond my expertise. Scott has already pointed out that others might hold different opinions. At present I would not support the proposal.

PENN 70321: I support the Proposal. The molecular evidence for synonymy seems robust; and failure to conserve *Pseudocercospora* as the name for the combined genus would be nomenclaturally disastrous.

REDH 70329: We should ask both authors if this is still the route they wish to take and to supply additional information. In asking my colleague, Keith Seifert, about the situation, he suggested we not rush to judgment (neither supporting nor not supporting it, but wanting more information on more taxa). If the authors still wish it to go forward, next time I will vote yes rather than more discussion as I voted this time (if still on the ballot).

CRAN 70409: In considering the conservation of *Pseudocercospora* against *Stigmina*, a different interpretation of Fig. 2 (Studies in Mycology 55:167) can be obtained if the node with the bootstrap value of 89, which includes all *Pseudocercospora* species of this study, is chosen. Then *Stigmina* becomes a sister group to *Pseudocercospora* and could remain as a closely related genus. We are asked to merge these two genera based on an excellent paper whose prime objective was the study of two distinct forms of *Pseudocercospora griseola*, the results of which I agree with. The bootstrap value of 100 is obtained for *Stigmina platani* when only two GeneBank sequences of this species are added. If the study had been centered on *Stigmina* with additional species sequenced and included in the clade, it is possible that the bootstrap value obtained would be less than 100, the node length shorter and the results may not support the merger of these two genera.

MAY 70430: I would like further discussion because as pointed out by PRIN 70301 the molecular analysis only included five species from this very diverse group, and taxonomic conclusions could change with better taxon sampling (and hence the potential nomenclatural actions required may be different).

→**GAMS 70614:** Additional information from Pedro Crous: PRIN 70301 requests more taxon sampling, but it is the type species of the 3 genera that have been analysed. The presence of synnemata (*Phaeoisariopsis*) and minute scar thickenings is common in the genus *Pseudocercospora* (for instance, the type *P. vitis,* commonly forms synnemata). Although *Stigmina* is characterized by having distoseptate conidia, several taxa with disto- and euseptate conidia have since been found in the Mycosphaerellaceae. Thus this feature also appears uninformative at generic level. Thus *Stigmina* can be accommodated in *Pseudocercospora*. It appears that these anamorph genera also cluster in the same clade within the *Mycosphaerellaceae* (although we consider the family to encompass several other genera as well). In an alignment of 200 additional taxa representing all *Mycosphaerella* anamorph genera known from culture (full LSU and SSU) species of these 3 anamorph genera cluster in the same clade with 99% bootstrap (unpublished data).

Props. 1733–1735, to conserve the names *Chrysomyxa empetri, C. piperiana,* and *C. ledicola* (*Uredinales*) with teleomorph types against their anamorph homonyms. Proposed by Patricia E. Crane. Taxon 55(3): 804–805.

*** ***(1733)*** *Chrysomyxa empetri* vs. *Chrysomyxa empetri (p. 804)*

Voted May 1, 2007: 11 : 2 : 2 : 1. (69% recommend)

*** ***(1734)*** *Chrysomyxa piperiana* vs. *Chrysomyxa piperiana (p. 804-5)*

Voted May 1, 2007: 11 : 2 : 2 : 1. (69% recommend)

*** ***(1735)*** *Chrysomyxa ledicola* +type vs. *Chrysomyxa ledicola (p. 805)*

Voted May 1, 2007: 10 : 2 : 3 : 1. (63% recommend)

SECRETARY'S COMMENT: These three linked proposals were temporarily retained for further input in Commentary 4, despite having received the necessary number of votes to recommend on the May 2007 ballot. After nine months, only one comment (see GAMS 70614 below, in support of all three proposals) was received. Therefore, I have forwarded our recommendation to conserve all three names with teleomorph types to the General Committee and will remove these proposals from Commentary 6.

Committee comments on Props. 1733–1735:

REDH 61130: Each of these makes sense and were long over due but note that an alternative solution might be possible. The author had indicated a willingness to go the route of epitypification prior to publication, but because the proposals had been delayed for so long it was decided to go ahead with them as published. Epitypification of the anamorphic names by teleomorphs is possible (Vienna Code: Art. 59. 7) when "no existing legitimate name for the holomorph" is available. Committee members should be aware of the fact that a special committee on Art. 59 has been established which may lead to recommended changes ranging from further modification towards single names or towards changing it backwards to pre-Vienna Code wording. It is not known how the Special Committee will lean. For now the safest route to go would be to support 1733 and 1734, which maintain current usage. 1735 is slightly different because a place of publication and a type need conservation, but is also supportable.

NORV 61215: I support these proposals.

GAMS 70223: Certainly to be supported. A sad consequence of the present situation around Art. 59. But I do not yet see a general solution by modifying the Code.

MAY 70228: I would prefer that the situation be solved by epitypification allowable under Art 59.7, if that would work in these cases. To use conservation over epitypification as a remedy sends a signal that the conservation route is available/preferable when the much simpler epitypification should be used (because it does not require formal proposals). Who knows where we are heading with changes to Art 59, but I imagine that epitypification could be a major part of it. Should many names be involved, it seems better not to have them all needing formal proposals for conservation, if epitypification would work as well.

PENN 70321: (*C. ledicola* – Props. 1735) I need elucidation of this Proposal. Shouldn't it also propose conservation of *Chrysomyxa ledi* Syd. & P. Syd. against the earliest teleomorphic name + type of the taxon, *Puccinia ledi* Berk. & M.A. Curtis ?

REDH 70326: To respond to the question above (PENN 70321), *Puccinia ledi* Berk. & Curt. cannot be transferred to *Chrysomyxa* because of the binomial combination is preoccupied.

REDH 70329: T. May comment: 70228: while I am inclined to support these proposals, I voted more discussion so that we can hear the opinions of others on this issue. It will be some time before the Special Committee on Art. 59 reaches consensus (if ever) but on the books now in the Code is the availability of epitypification. In fact John McNeill had hoped that epitypification had been used so that there would have been an example in the Code. This Committee could reject the proposals as such and in the report, designated epitypes (or ask the author to publish the epitypification). In all fairness to her, if we reject this because we prefer epitypification, we should notify her of the reasons and action to give her the option (given that she had been willing to entertain both types of solutions- conservation or epitypification). Timing and delays affected proposals' direction. Nonetheless, the easiest and safest solution is conservation (at least until Art. 59 wording is debated). I await further comments by others before voting one way or the other (if still on the ballot).

→**GAMS 70614:** Art. 59.7 now reads "… for which there is no existing legitimate name for the holomorph…". It would be a disastrous consequence of this passage if from now on any (older) anamorph taxon could later be epi-(teleo-)typified and thus get priority over an extant binomial (even in an inappropriate genus) that is available for the teleomorph. As long as this situation is not clarified in a modified Art. 59 (to which I would probably object), conservation as proposed is the only solution for these cases.

Prop. 1738, to conserve the name *Poria cocos* against *Daedalea extensa* (*Basidiomycota*).
Proposed by Scott A. Redhead & James Ginns. Taxon 55(4): 1027–1028.

Voted May 1, 2007: 14 : 0 : 1 : 1. With 88% CF support but temporarily retained for comment by request of the Chair (see DEMO 50402).

Vote change January 2, 2008: 15 : 0 : 0 : 1. With the request for further discussion removed (see DEMO 80102), this proposal has been forwarded to the General Committee as recommended and will disappear in Commentary 6.

Committee comments:

NORV 70128: The proposers clarify an unusually tangled nomenclature where "there are dual nomenclatures running in parallel — the correct one using *'extensa'* and the popular one, using '*cocos*'." They provide the history behind a continuing 23-year long resistance by the majority of the "scientific, medical, and other user communities" to use the nomenclaturally correct species epithet ('*extensa*') despite the Ginns & Lowe (1983) paper that publicized the proper use of '*extensa*'. Rather than continue to beat a dead horse, I agree with the Redhead & Ginns conclusion that "it is in the best interest of science to make correct the continued usage of '*cocos*' via conservation of *Poria cocos*."

GAMS 70223: To be supported.

MAY 70228: Despite *extensa* being the correct epithet, enough time has passed to show that the battle has not been won in convincing users to take up *extensa* in preference to *cocos*. In regard to the earlier anamorphic names, it is good to be dealing with anamorphic names that are so demonstrably anamorphic (sclerotia). How to get users to take up the taxonomically correct genus *Wolfiporia* is another problem, but beyond the scope of our committee.

PRIN 70301: It is certainly hard to deal with the stubbornness of the scientific community in this matter and it would perhaps be best to just give in and let them have their will. It just strikes me that, when only CAB abstracts are taken into account, there are 91 articles using "*extensa*" vs. 244 using "*cocos*". The situation will perhaps not be much better after the proposed conservation.

PENN 70325: This seems a tidy and satisfactory Proposal to resolve a tangled web of names.

DEMO 70502: More discussion is needed. A complex issue for a quick vote.

→**DEMO 80102:** I was hesitant on this because I am not in favour of following the tradition of a special group against good nomenclature. However here I am finally convinced the taxonomic literature has not been using very much the correct name. It is not a good example that people like Ryvarden have given but I accept the conservation. The proposal can thus be considered accepted without a new vote.

Prop. 1739, to conserve the name *Boletus applanatus* against *B. lipsiensis* (*Basidiomycota*).
Proposed by Scott A. Redhead, James Ginns & Jean-Marc Moncalvo. Taxon 55(4): 1029–1030.

Voted May 1, 2007: 15 : 0 : 1 : 0 : 0. With 94% CF support but retained for comment by request of the Chair. This will appear again on our March ballot.

Committee comments:

NORV 70128: There is no doubt that the shifting the starting dates for fungal nomenclature in the 1910 and 1983 CODES has considerably complicated the tortuous nomenclatural path of the well-known ganoderma first published by Batsch (1796) as *Boletus lipsiensis* but more commonly accepted using the '*applanatus'* introduced by Persoon (1800) four years later. Redhead & al. observe that current use of

'*applanatus*' is far more frequent than for the earlier named '*lipsiensis*', but perhaps more persuasive is their discussion of the type designations for each name. I tend to support this proposal.

GAMS 70223: I am really glad to see this proposal that will save the name of an extremely common forest pathogen.

PENN 70325: A highly desirable conservation to regularise 200 years of almost universal nomenclatural usage.

DEMO 70502: The proposal ignores a lot of literature and arguments for dropping *G. applanatum*. It does not even give reference to the comments by Demoulin, Hawksworth, Korf and Pouzar in Taxon 30: 59, 1981 in discussing the change in starting point. The European literature is especially badly covered and the proposal so unsatisfactory I will publish a rebuttal in Taxon as soon as I find time.

→**DEMO 80102:** This proposal contains a large number of misinformations that need rectification. The situation cannot however be properly appreciated without knowledge of the taxonomic situation, a summary of which I gave at the XV Congress of European Mycologists (Saint Petersburg, Set. 16-21, 2007, Abstr. pp.35-36).

From a nomenclatural standpoint the important thing is that the genus is notoriously taxonomically difficult ("the most difficult genus of all polypores", Ryvarden and Gilbertson, European Polypores, p.268, 1993) with the result that the most popular species concept, that of Ryvarden and his followers is, at least for the tropical species, too broad. This situation also leads, and this is most relevant in the case of "*G. applanatum*", to numerous misidentifications: "(*G. lucidum* and *G. applanatum*)…are probably two of the most poorly understood species of *Ganoderma* and two of the most frequently misapplied names", Seo and Kirk, *Ganodermataceae:* Nomenclature and classification, in *Ganoderma* diseases of perennial crops, Flood, Bridge and Holderness eds, 2000, p.5).

Demoulin, Hawksworth, Korf and Pouzar thus analyzing the consequences of the change in starting point they were proposing (Taxon 30:52-63, 1981) wrote " If *Ganoderma applanatum* is a well known name, it is however quite ambiguous for "it has been made a receptacle for quite a number of closely related or more or less similar species from all parts of the world. The resulting mess has not yet been sorted out…"(Donk, 1974, p. 218). In Europe the confusion with *G. adspersum* (S. Schulz.) Donk has been especially frequent. The loss of such a name is thus not very dramatic."(p.59).

The proposal thus starts with the incorrect statement that *G. applanatum* is one of the most easily identified and ubiquitous fungi in the Northern Hemisphere. It is true Ryvarden and Gilbertson (l.c. p.271) wrote that *G. a.* is easy to recognise in the field. This may sometime be true, especially when it is attacked by the specific gall-forming dipteran *Aganthomya wankovici*, but the insect is much rarer than the fungus (Audibert, Bull. Mens. Soc. Linn. Lyon 74:5-10, 2005) and those authors contradict themselves on the following page where they write from *G. australe* (that is *G. adspersum*) that "the only reliable character separating young specimen of this species from *G. applanatum* is the larger spores". The proposal uses as reference Overholts (1951), a reference ten years older than the characterisation by Steyaert of *G. adspersum* (as *G. europaeum*). I doubt nowadays any serious student of ganodermas would be certain "*G.a.*" is ubiquitous in the Northern hemisphere.

The same criticism applies to the statement that it is a major cause of primary white decays. Neither the old Overholts reference nor more recent compilations can inform us on the real ecology of *G. lipsiense* (*G. applanatum s. str.*). With almost an half century of field experience with ganodermas, in the study of which I benefited from the guidance of R. L. Steyaert, I can concur with Butin (Tree diseases and disorders, pp.168-169, 1995) that "*G.a.* "lives principally as a pure saprotroph on old, already dead stumps" in contrast to the similar *G. adspersum* "which is a weak parasite". If I have seen trees uprooted, with severe property damages, by a

butt rot due to *G. adspersum,* I have never seen this with *G. lipsiense* and believe records of pathogenesis by this species are mostly due to confusions with *G. adspersum* (and even *G. pfeifferi*). *G. adspersum* is also the most common *Ganoderma* in western Europe, at least in parks and on alignment trees, which concurs with the statement of Lonsdale (Principles of tree hazard assessment and management, 1999, p. 103) that "Recent records suggest that *G. adspersum* is the more common species, at least in southern England, and that the two have been confused". The comment GAMS 70223 is thus completely wrong and in fact shows how confusing the name *G. applanatum* is.

Another misinformation is that *G. a.* is "In eastern Asia it is one of the traditional medicinal fungi". It is true it is occasionally cited as such but "in therapeutic practices and literature citations, Lingzhi (*Ganoderma*) usually refers to the species *G. lucidum* (Chang, Sh.-T. and Miles, Ph. G., Mushrooms/Cultivation, Nutritional value, Medicinal Effect, and Environmental Impact, Second ed., p. 357, 2004).

From an economic standpoint, beside a possible use in biotechnology, the major importance of ganodermas of the subg. *Elfvingia* is their role in butt rot of alignment and park trees. The literature on this subject is not cited in the proposal, while it is there one may find the best recent information on those fungi, at least for Europe, type locality of the conflicting names. At first glance one may think this literature supports the proposal, for *G. applanatum* is the most frequently used name. At closer glance one however find that the tendency is to shift to *G. lipsiense*. This is logical for references using the confusing name *G. applanatum* do not convey reliable information, while the use of *G. lipsiense* indicates a more accurate modern concept. One should see for example the nice manual "Dynamique de dégradation des arbres par des champignons lignivores" edited by Hainaut Développement in 2004. Most significantly, if the English edition (Tree diseases and disorders, 1995) of the renowned treatise by Butin still uses *G. applanatum*, the third German edition (1996) adopted *G. lipsiense*.

All this being said, one should not spend too much time trying to figure out what the authors who are cited for using *G. applanatum* had in mind. I stopped at the first one: Bessette et al., 1997: they give the spore size as 7-11x5-7,5 mu, values much too high for *G. lipsiense*. Many of those references are anyway uninteresting for being anterior to the time (1981) when *G. lipsiense* became the correct name. Nobody questions that *G.a.* has been a widely applied name and still is. The issue is whether this is desirable.

The proposal also gives a distorted view of the post 1981 adoption of *G. lipsiense*. It does not give the reason advanced by Moncalvo and Ryvarden (1997) for abandoning *G. lipsiense:* that no authentic specimen remains. This, of course, is no nomenclaturally valid argument and is contradicted by the proposal, which refuses the neotype designated by Kreisel, insisting the plate is the holotype. One should be aware that the code (Sydney) in force when Kreisel designated his neotype could be considered as not making the illustration a mandatory holotype (Art. 9.3, the type **may** be a figure) and the concept of epitypes did not exist. As stated before, it can occur that *G. lipsiense* can be recognised on sight, and this is the case for the Batsch plate, which as usual is excellent. If the proposers had stuck to the Moncalvo and Ryvarden opinion, which unfortunately has been influential, for example in the establishment of the British checklist of Legon and Henrici (2005, p.394) which qualifies *Boletus lipsiensis* of nomen dubium, the proposal, which is based on the synonymy of *Boletus applanatus* and *lipsiensis* should not have been made.

Taxonomically important references that adopted *G. lipsiense* are omitted: the monumental Dutch checklist of E. Arnolds, Th. W. Kuyper & M. E. Noordeloos (1995, p.475), accompanied

by the equally impressive Atlas of Nauta & Vellinga, and the Russian flora of Bondartseva (1998). It is incorrect to place those nomenclaturally researched works on the same level as papers in medical journals where names are uncritically copied and where the persistent use of catchall names like *G. applanatum* contribute to widespread confusion. It is also incorrect to claim field guides use *G. applanatum* while the handiest one for Western Europe, Bon, has used *G. lipsiense* in his last edition (2004).

There is one electronic source of information to which the proposers do not refer while it might give us the most cogent reasons to retain *G. lipsiense*, it is GenBank.

To be certain of the interpretation of the deposited sequences, we have sequenced in my lab DNA extracted from fruitbodies or cultures for which I was certain of the identity. The sequences were the classical one in polypore phylogeny, including the two ITS, the 5,8S and a part of the 28S rRNA genes (from primer LR7). Most of the variation was to be found in the ITS and we have made trees by both maximum parsimony and maximum likelihood of the 233 ITSI+ITS2 sequences present in GenBank at the time, including with those our own sequences. Both MP and ML allow to retrieve two very distinct clades which include our references for *G. adspersum* and *G. lipsiense*. As expected the *G. adspersum* clade includes sequences labeled as such and labeled *G. australe* (but, and this is another story, *G. australe* appears in many other places) and the *G. lipsiense* clade includes sequences labeled as such as well as *G. applanatum* and *G. sp*. **There are however only 3 *G. applanatum* for 13 *G. lipsiense*.**

There are in fact few sequences labeled *G. applanatum* in GenBank, three correctly identified and one not. We thus have also used two sequences considered *G. applanatum* which are not in GenBank but answer identifications in reputable culture collections, one is MUCL 39327 (G.Castillo, unpubl. Thesis 2002) and IMI 157816, published but not deposited by Smith and Sivasithamparam (Mycol. Res. 104: 943, 2000). Both turned out to be *G. adspersum* which confirms this is frequently misidentified as *G. applanatum* and explains the anomaly in Smith and Sivasithamparam study, were the purported English *G. applanatum* was quasi identical to CBS 222.48 a culture of *G. adspersum* I had deposited there. It seems in fact no real sequence of *G. applanatum* has ever been used in the published studies I am aware of.

In conclusion, if *Ganoderma applanatum* was a correct name it should be proposed for rejection like *Lichen coeruleonigricans*, *Lichen jubatus*, *Betula alba*, *Crataegus oxyacantha*, *Drosera longifolia*, *Eriophorum polystyachion* etc. Now that it is incorrect we should not let it come back while the taxonomy and nomenclature of *Ganoderma* is on its way to be clarified.

→**REDH 08104:** As Vincent noted, we did not cite his earlier publication (Taxon 30: 59. 1981), which I would have preferred to have cited. In actuality it was noticed after our proposal was "in press" but I did not insist on it being inserted because I had already bothered John McNeill enough on this article and felt it could be mentioned in our discussions and also that times and the Code had changed since 1981. Nevertheless, I sincerely apologize to Vincent for my oversight and thank him for including it in our discussions. The issue of *G. applanatus* vs. *G. lipsiensis* appears to be a passionate one for Vincent, and in the analysis by Demoulin, Hawksworth, Korf & Pouzar (1981) they concluded that the "..loss of such a name is thus not very dramatic." However, as noted in our proposal (which initially was my idea – if anybody is to be blamed) is not borne out by the continued common use of the epithet "applanatum" versus the infrequent use of "lipsiense". There is only a slight shift towards *G. lipsiense* (again we noted this). The issue seems forced and this is one of the reasons the Code allows for conservation of species epithets, an option not available in earlier times. Historically, *B. lipsiensis* and *B. applanatus* have been interchanged and listed as synonymy of one another.

Fries (Syst. Mycol. 1821: 374) stated under *Polyporus fomentarius* "b. dilatatus, planiusculus, rufescens. *B. lipsiens*. Batsch. C. I. f. 130. *B. applan*. Pers. l. c. p. 2". In 1836-38 (Epicr.: 465) and 1874 (Hymen. Europ.: 557-558) Fries lists Batsch f. 130 under *Polyporus applanatus*. When Atkinson adopted "lipsiense" it was to replace "applanatum". There seems to be no argument for distinguishing *B. lipsiensis* from *B. applanatus*. Therefore, they are synonymous and one name must prevail. If there are counter arguments then we should discuss them.

It then remains for us to judge the wisdom of using one name over the other. Vincent has made several good points about the recent restricted applications of the name *G. lipsiense,* but I am not convinced that it really is being used in such a restricted fashion, rather than simply being substituted for *G. applanatum*. I respectfully disagree with Vincent. This surely is what a committee such as ours must do – debate, weigh options, decide, and make recommendations. My personal feeling is that it is doing a disservice to mycology to continue to use the two names as alternatives and the more commonly used named ought to be conserved. But if the committee overwhelmingly decides this is not the case, at least an effort will have been made and published making clear that such is not to be the case.

One further comment is perhaps appropriate in consideration of Vincent's desire to limit usage of an epithet to restricted versus broad historical application, and that is to say, that this type of problem is not new, as it repeatedly crops up. The case of *Armillaria mellea* comes to mind, where in the past essentially all annulate *Armillaria* species were called *A. mellea*, but today the application is restricted.

A poster was displayed at the International Mycological Congress in Cairns, Australia in 2006 outlining the proposed conservation proposals on *Ganoderma applanatum* among others, and no objections were raised by any of the participating mycologists. On the contrary, there was general support for all the proposals, which were seen as sensible.

Prop. 1741, to conserve the name *Pleurotus japonicus* against *Agaricus guepiniformis* and *Pleurotus harmandii* (*Basidiomycota*). Proposed by Scott A. Redhead & Hitoshi Neda. Taxon 55(4): 1032-1033.

Voted May 1, 2007: 15 : 0 : 1 : 0 : 0. With over 94% CF support, this proposal has been recommended for conservation in Report 14, with recommendation that the orthography of '*guepiniformis'* be corrected to '*guepiniiformis'*. Retained in this Commentary to include REDH 80104, this discussion will not appear in Commentary 6.

Committee comments:

NORV 70205: A well-researched and well-written proposal, which I support.

GAMS 70223: To be supported.

PENN 70325: I support this Proposal to conserve a well-known and widely used name against two obscure earlier synonyms.

DEMO 70502: On the whole I support the conservation. There is however a mistake in orthography. According to Art. 60.8, guepiniiformis, not guepiniformis is the correct spelling (inelegant as it is). If this receives a majority of yes, more discussion may not be needed if the necessary orthographic correction is made.

→**REDH 80104:** I have no objection to Vincent's correction from *guepiniformis* to *guepiniiformis*.

Prop. 1742, to conserve the name *Lyophyllum* with a conserved type (*Basidiomycota*). Proposed by Scott A. Redhead, Valerie Hofstetter, Heinz Clémençon, Jean-March Moncalvo & Rytas Vilgalys. Taxon 55(4): 1034–1036.

Voted May 1, 2007: 11 : 1 : 4 : 0 : 1, With 69% CF support but with four votes for discussion, this proposal is retained for additional comment. To be included on the forthcoming ballot.

Background & Guest comments on Prop. 1742:

Matheny via NORV 70131: Brandon Matheny, senior author of the "Major clades of *Agaricales:* a multilocus phylogenetic overview" for the 2006 AFTOL issue of Mycologia (98: 984–997), regards this proposal as sensible.

Committee comments on Prop. 1742:

NORV 70205: There is no question that applying *Lyophyllum* to brightly pigmented species and *Calocybe* to grey-brown species would confuse most agaricologists, and that including several currently accepted segregate genera into one mega-*Lyophyllum* genus would severely reduce the information carried with the generic name. I commend the authors for using both molecular and morphological characters to select an appropriate new type species of *Lyophyllum*. As the four stated advantages for selecting *L. leucophaeatum* as type far outweigh the three stated disadvantages, I support the proposal.

GAMS 70223: Needed to retain the application of the generic name for some very common fungi.

MAY 70228: Moving to a new type for *Lyophyllum*, based on relatively recent data needs to be considered very carefully. For the *Coprinus* example that was debated at length recently, further analyses of DNA (such as by Walther et al, Mycol. Res. 109: 525) have changed the situation as to whether the segregates are monophyletic, especially in regard to *Psathyrella* (although not at all the basic fact that *Coprinus* sens. strict. is not related to all the other *Coprinus*).

I wonder if the authors of the *Lyophyllum* proposal are going to publish the six gene phylogeny (referred to on p. 1035) soon and does this include species such as *L. hypoxanthum, L. buxeum* and *L. musashiense*, which are discussed by Hofstetter et al. (Mycol. Res. 106: 1057) as possibly also belonging to the *L. leucophaetum* + *Calocybe* clade. Because *L. leucophaetum* is at the base of this clade, there remains the possibility that it might form a separate group to *Calocybe* with more taxon sampling, in which case a solution would be to leave the name *Lyophyllum* with it, and provide a new name for the 'major' *Lyophyllum* clade.

PENN 70325: The Proposal is a pragmatic solution, avoiding major dislocations of previous nomenclatural usage at the cost of minor nomenclatural changes.

REDH 70329: (Re MAY 70228) Admittedly yes, another difficult and peculiar situation. The authors of the phylogenies had been prepared to bite the bullet (so to speak) and describe a new genus, for which they had coined *Paralyophyllum,* but were hesitant (primarily because of the *Coprinus* controversy). There was relief to propose conservation of a different type. As to why I choose to put this forward as option one, rather that favour retention of the type as it stands, whereas for *Coprinus* I seemingly went the other way, I can only say that in the case of *Coprinus,* the type was among the best known species even to nonmycologists, but in the case of *Lyophyllum leucophaeatum,* hardly anybody knows it, while many people know species in the *L. decastes* group. So it is not an identical case when options are weighed. Should the CF not support this proposal, the authors are quite prepared to describe a new genus. It is simply preferred that this not be forced upon them. The other species Tom May mentions I do not think are among the taxa in the current papers.

MAY 70430: I have already indicated my concerns in MAY 70229, and would like further discussion on this proposal also.

DEMO 70502: Too complex an issue for an early vote.

→**DEMO 80102:** The need for a conservation is linked to a taxonomic concept and I am not familiar enough with *Lyophyllum* to make myself an opinion on the correctness of this taxonomy, less to convince people it would be so inadequate that it is not worthwhile taking into consideration. In so far as people will use a taxonomy, which makes a conservation useful we must accept it. I however still have a problem with the proposal. Why avoid *L. decastes* "a name far more familiar to mycologists than *L. semitale,* because of problems with the typification of the basionym"? I indeed know *L. decastes* and not *L. semitale* (not even pictured by Courtecuisse) and find that if there is a problem with its typification, let us solve that by

conservation at this occasion. And if *L. decastes* is not what we thought why not come back to the even better known *L. aggregatum*? Scott please explain.

→**REDH 80104:** With regard to the suggested choice of a conserved type, such as *semitale* vs. *decastes*, a name perhaps more familiar to mycologists, it seems from what Heinz Clémençon says, and Jean-Marc Moncalvo agrees, that the typification of *Agaricus decastes* Pers.: Fr. is a problem that could lead to that name being placed in *Tricholoma* because the name seems to have been misapplied almost consistently. However, I have not investigated that aspect thoroughly. A less problematic way to resolve the issue was to select as type, a species for which there existed reliable type material and *L. semitale* met those requirements. If that is a major concern of the majority of the committee, then I can go back and investigate the issue further. But if the major concern regards the shift, it is irrelevant. To me the main issue is whether or not *Calocybe* should become *Lyophyllum* and most of the *Lyophyllum* becoming something else, for which a new name would be needed.

Prop. 1744, to conserve the name *Lichen hagenii* (*Lecanora hagenii*) with a conserved type (lichenized *Ascomycota*). Proposed by Lucyna Sliwa & David L. Hawksworth. Taxon 55(4): 1038-1039.

Voted May 1, 2007: 14 : 1 : 0 : 1 : 0. With 88% CF support, this proposal will be removed in the next Commentary.

Committee comments:

NORV 70210: As the evidence for the need for a conserved type is convincingly presented and the proposers preferentially selected a type with samples already distributed in an exsiccatum, I support this proposal.

REDH 70222: I support this proposal.

GAMS 70223: I do not know these organisms, but the proposal shows the relevance of conservation.

MAY 70228: The authors of the proposal consider that if *umbrina* (in a restricted sense) were to remain the correct name for *hagenii*, then this could cause 'major confusion as the numerous old records of *L. umbrina* might all retroactively be considered to refer to *L. hagenii*'. If the proposal is accepted, there would not seem to be any less confusion in this regard, because a consequence is the sinking of *umbrina* under *hagenii* (due to the former being lectotypified by material that conforms to the modern sense of *hagenii* as would be established by conservation with a conserved type as proposed).

For this reason, I wonder if there is any merit in also rejecting *umbrina* - however, doing this alone does not solve the situation because *hagenii* needs re-typification, and perhaps it is overkill, and so the confusion about *umbrina* in the strict sense of its type versus the broad sense will just have to be dealt with (and perhaps a little better to have the confusing epithet *umbrina* under *hagenii* that the other way around). The argument that *L. hagenii* has been applied consistently for more than half a century does carry weight to support the proposal. The result of sinking *umbrina* under *hagenii* also serves to prevent *umbrina* from being taken up in any sense (under the current circumscriptions of species).

TRIE 70319: I support this proposal and suggest that we also note that the points made by Sliwa in her paper on the typification of *Lecanora dispersa* and *L. albescens* (Mycotaxon 2006. Mycotaxon 97: 291–297) show that the author of the proposal is experienced with these groups within *Lecanora*.

PENN 70325: The Proposal conserves the current name of a widespread taxon, and avoids the complications of confused and misapplied competing names.

Prop. 1756, to conserve the name *Roccellina* against *Roccellaria* (lichenized *Ascomycota*). Proposed by Anders Tehler. Taxon 56(1): 254–255. (2007)

Voted: Vote not yet taken.

Committee comments:

NORV 70321: The author contrasts the widespread acceptance of *Roccellina,* proposed by Darbishire in 1898 and now represented by 27 taxa, to the monotypic and far less well-known *Roccellaria,* established

a year earlier by the same author. I tend to support the proposal, which is motivated by the fact that molecular analyses now place *Roccellaria* nested within a paraphyletic *Roccellina*, but would welcome additional comments before voting to conserve a later name.

REDH 70322: I support this seemingly reasonable proposal.

→**DEMO 80102:** This is the kind of proposal that I feel oblige to vote for, despite the fact I enrage that it has been made. Its only justification is that the proposer does not accept paraphyletic taxa. As a evolutionist, admirer of E. Mayr, and supporter of the call for paraphyletic taxa in Taxon 54, 5, 2005, it makes me sick that the ayatollahs who have found in Hennig their prophet cannot accept that a genus originates in another one. However nomenclature must follow existing taxonomies and we are obliged to provide for the taxonomy of cladists, like we must pass conservation proposals for the lumpers. I am however very anxious that similar proposals can come out in legions more and more paralyzing our work.

→**REDH 80104:** I still support this proposal.

Prop. 1757, to conserve the name *Psilocybe* (*Basidiomycota*) with a conserved type. Proposed by Scott A. Redhead, Jean-Marc Moncalvo, Rytas Vilgalys, P. Brandon Matheny, Laura Guzmán-Dávalos & Gastón Guzmán. Taxon 56(1): 255–257. (2007)

Voted: Vote not yet taken.

Committee comments:

NORV 70321: Yet another difficult instance where molecular analyses support fragmentation of a large, polyphyletic genus, here into two main clades. The authors make an excellent case for modern recognition of the name *Psilocybe* through its (famously) hallucinogenic representatives. The currently accepted lectotype of the polyphyletic genus is the "common moss inhabiting, non-hallucinogenic species, *P. montana,"* a taxon that "does not produce psilocybin and … falls within the other main clade, which when separated generically, leaves the hallucinogenic species without a generic name." Unfortunately, the lectotypification of *P. montana* [by Donk in 1949, 1962] was preceded by an earlier lectotypification [of *P. merdaria* by Clements & Shear in 1931] that "cannot be superseded except by conservation." The authors therefore follow an admittedly novel approach by proposing to conserve the name *Psilocybe* with yet another proposed lectotype, the well-known hallucinogenic *P. semilanceata* (accepted as lectotype between 1938-1968 by many authors), leaving the name *Deconica* (typified by *Agaricus physaloides* Bull.) available for the non-hallucinogenic clade.

The authors also offer a second option [B, 'not recommended'] that would "leave the typification as generally, but incorrectly, accepted until now", with *P. montana* as type, after explaining that the previously proposed *P. merdaria* is atypical of the clade and noting that then a new name would be needed for the hallucinogenic clade. In view of the nomenclatural arguments and that the often legally controlled hallucinogenic compounds psilocin and psilocybin are named after the genus, I tend to support Proposal A with *P. semilanceata* as type and reject Proposal B with *P. montana* as type.

→**PENN 70821:** I support the main proposal to conserve *Psilocybe* as the name for the /psychedelia clade, with *P. semilanceata* as conserved type. A manuscript I have recently reviewed (shortly to be submitted for publication) will add another element to the mix — the type of the polyphyletic genus *Weraroa*, *W. novae-zelandiae*, lies within the /psychedelia clade and is reputed (according to various web postings) to be hallucinogenic. This raises the possibility that, if *Psilocybe* is retained as the name of the non-hallucinogenic clade ("option B"), the earliest available generic name for the /psychedelia clade will be *Weraroa*, typified by a secotioid New Zealand endemic species. It is not desirable that such a well-known clade/genus should be typified by such an anomalous species of limited distribution.

→**DEMO 80102:** I would have preferred to see this proposal based on a more specific taxonomic revision of *Strophariaceae* than the general tree of Matheny and al. 2006, a consensus tree that does not allow to judge the branch lengths and do not accept the ref.

Walther et al. 2005 as demonstrating polyphyly (short branch length and a situation that could well be paraphyly). Nonetheless if people want to split *Deconica* from *Psilocybe* it is better to fix the type of *Psilocybe* with the best known species. The intricate typification story, not quite objectively told in the paper, might thus be forgotten.

→**REDH 80104:** This proposal is based upon the consistent trend seen in phylogenetic research, both published and on going. Conservation is required in any event because nobody is using the correct lectotype. Additionally, we can ensure stability by the suggested choice. Notably I had the various authors, who are experts on *Psilocybe* all agree on this proposal. Gastón Guzmán is the world's expert on *Psilocybe*. Laura Guzmán-Davalos, his daughter and now molecular systematist, obviously supports the move.

Prop. 1769, Proposal to conserve the name *Cortinarius speciosissimus* against *C. rubellus, C. orellanoides,* and *C. rainierensis* (*Basidiomycota*). Proposed by Bruno Gasparini, Stig Jacobsson & Karl Soop. Taxon 56(2): 596–597. (2007).

Voted: Vote not yet taken.

Background & Guest comments on Prop. 1769:

→**Joe Ammirati** (Seattle WA) **80131:** [*Response to secretarial invitation to comment*] There is some difficulty with the interpretation of the name *'rubellus'*, as indicated in the proposal. [That interpretation] does not bother me however. *'Rainierensis'* is not a good name to use because the protologue is incomplete. *Speciosissimus'* is fine; Favre was a good worker and just used a name that was taken (*'speciosus'*). He likely did not know about Earle; few people did. *'Orellanoides'* is not more useful or helpful than *'speciosissimus.'* What a whole lot of ferment over one species — or is it one? who knows? — one morphological species. It has always struck me as odd that Fries did not name this as a distinct species; he must have seen hundreds of [representatives] around Femsjo. I hope this is settled: I think I have written about this species under all four names.

→**Ursula Peintner** (Innsbruck) **80201:** [*Response to secretarial invitation to comment*]. **The problem of synonymy:** Synonymy of *C. rubellus* and *C. speciosissimus* is currently commonly accepted, and synonymy with the other below mentioned taxa is strongly suggested based on morphological characters. However, it would be nice to have a study confirming synonymy with phylogenetic methods; both geographical separation, and adaptation to a range of hosts can be strong drivers for speciation.

Based on my restricted, partly unpublished rDNA ITS dataset on this group (see Figure), sequences named "*C. rubellus"* (mainly from Scandinavian sources) form a well-supported clade that is sister group to a sequence of "C. orellanoides" (IB 19980157) collected and identified by Moser from Sweden (Femsjo) in association with *Picea*. I doubt that this epithet was applied to this latter collection in the original sense (broad-leafed forests). *C. orellanus* and *C. eartoxicus* Gasparini (2004) are clearly distinct. No sequences of *C. speciosissimus* or *C. rainierensis* are available from public databases. Thus, based on this dataset, synonymy issues cannot be clarified.

The following hypothesis need still to be proofed based on Type material or material from Type localities

i) Is C. *speciosissimus* a synonym of *C. orellanoides*? (conifer associated spp: *C. rubellus, C. speciosissimus*; broad leafed trees: *C. orellanoides*)

ii) Is *C. rainierensis* from U.S.A. synonym of *C. rubellus?*

These issues can be solved by studying

i) Henry´s type material in PC and recent material of *C. orellanoides* from broad-leafed forests.

ii) Study *C. rainierensis* (typus or other) material from U.S.A. with morphological and molecular methods.

As this is a group of very important toxin producing fungi, it would be worth to carry out these studies. The resulting data would put the synonymy discussion on a solid base.

What is the most commonly used name? A search in four public databases (see table below) showed that *C. speciosissimus* is the most commonly used name by a broad public, followed by *C. rubellus*.

epithet	Medline	Google	ITS GenBank	UNITE
C. orellanoides	1	953	1	1
C. speciosissimus	26	6650	0	0
C. rubellus	0	4930	2	3
C. rainierensis	0	369	0	0

Should *C. speciosissimus* be conserved? *C. rubellus* is the oldest valid epithet for this taxon and has priority over *C. speciosissimus*. Having *C. rubellus* as valid epithet has the advantage that this is the oldest name having priority over all other (later) synonymous epithets (e.g. *C. orellanoides, C. rainierensis*). Therefore there is no need to reject one (or two) earlier epithets.

Having *C. speciosissimus* conserved has the advantage that this is a commonly used name for an important toadstool. However, what happens if *C. orellanoides* or *C. rainierensis* turn out to be distinct taxa? Can these epithets still be applied, even if they were rejected as proposed in this proposal 1769? [*Secretarial note: the names are rejected only when treated as synonyms of the conserved taxon. They remain available for taxa not regarded as synonymous.*]

A final decision has to be taken by the committee based on a careful evaluation of pros and cons, and of potential consequences.

Committee comments on Prop. 1769:

→**DEMO 80102:** I support the proposal but would like to see clarified the situation of the type of *C. orellanoides*. There is a contradiction in what Henry did publish and what was accepted by Høiland in 1985. The original description of 1937 is clearly based on material from Fontainebleau (BSMF 53, p.61, under *C. orellanus* one reads that a neighbouring species exists and that "L'ayant rencontré en abondance cette année dans la Forêt de Fontainebleau, nous allons en donner une description détaillée", p.64 "trouvé en grande abondance en 1936 dans la Forêt de Fontainebleau (Route de la plaine de Macherin)…de juillet à octobre sous les hêtres" and p.65 , Fontainebleau figures again in the Latin description. When in 1981 Henry writes again on the species and mentions "Holot. N°80767 in herb. cons." This logically should mean that from several collections made between July and October at the same spot in Fontainebleau, one numbered 80767 is the holotype. How then is it possible that Henry did send as type to Høiland a collection 3258 from Frasne, knowing that this locality is in the Jura region several hundred km from Fontainebleau? Even if the Henry collection is not available for loan, I am sure Bart Buyck could answer the proposers on how many collections of *orellanoides* are in the Henry herbarium and how they are labeled. He may also know if Henry had some peculiar concept of types and would name 'holotype' something that is not a holotype.

→**REDH 80104:** This proposal had many, many delays and problems with it as it was being edited, and therefore it was published with the type problem around *C. orellanoides* not being

fully resolved. We can ask the authors to further investigate or we can approach the curator directly. I suggest further discussion for now.

→**NORV 80115:** I also feel more discussion is needed. (80201 addition: I welcome the comments from Joe Ammirati and Ursula Peintner above and look forward to what additional insights Jacques, our own 'resident' expert, can provide.)

Conservation of *C. speciosissimus* seems premature, given the increasing number of researchers adopting the earliest name, *C. rubellus*. The nomenclator shows that Melot (1987) followed the Code by adopting the 1887 Cooke name, *C. rubellus,* over the 1937 Henry (*'orellanoides'*), 1950 Smith & Stuntz (*'rainierensis'*), and the 1953 Kühner & Romagnesi (*'speciosissimus',* nom. nov. for Favre's 1948 illegitimate later homonym, *'speciosus'*) epithets. Prop. 1769 contends that *C. speciosissimus* is the name "more commonly used in popular parlance, at public exhibitions, and in herbaria in Europe" but the list of supporting references is not convincing. Granted, in his 1980 Norwegian monograph on subg. *Leprocybe,* Høiland did use *'speciosissimus'* (which he then considered close to *C. orellanoides* and possibly synonymous with *'Telamonia' rubella*). But Høiland later (1985) adopted the earlier name, '*orellanoides',* after demonstrating that there was no statistically significant spore-size difference between his two '*speciosissimus'* and *'orellanoides'* lectotypes. Most recently, Høiland & Holst-Jensen (2000) use *'rubellus'* when referring to the taxon in their phylogenetic *Cortinarius* paper.

Moser's use of '*speciosissimus'* is somewhat parallel. The Moser 1978-1983 volumes treat '*speciosissimus'* and '*orellanoides'* as names of different taxa. Later, Moser & Jülich (Farbatlas III: *Cortinarius* 4, 1985) and Keller & Moser (Die *Cortinariaceae* Östereichs: 21, 2001) adopt '*orellanoides,'* listing *'speciosissimus'* as a synonym. While Keller & Moser include '? = *C. rubellus* Cooke 1881' [sic] in the nomenclator, they refer to Høiland for additional information ("Zur taxonomischen Abgrenzung dieses Schleierlings vgl. HØILAND (1985). Alle angeführten Belege des Herbars WU als *C. rubellus* COOKE").

The other papers listed in Prop. 1769 provide little support for conserving *'speciosissimus'*. Four toxicological papers were written before the full synonymy was known, and three other references (by Bresinsky & Besl, Thorn & Malloch, Horak) cite at least two of the names for independent taxa. A cursory search of my library did uncover 10 additional *C. speciosissimus* citations: **[1]** Ryman & Holmåsen (Svampar, 1984), citing the three other epithets as synonyms; **[2–9]** Phillips (Mushrooms & other fungi … 1981: 134), Ammirati & Traquir (Poisonous mushrooms of the northern United States and Canada, 1985: 99), Singer (*Agaricales*, 1986: 651), Bon (Mushrooms & toadstools …, 1987: 24), McKnight & McKnight (Mushrooms, 1987: 293), Buczacki (Fungi of Britain & Europe, 1989: 133), Pegler & Spooner (Mushroom identifier, 1992: 47), and Courtecuisse (Mushrooms of Britain & Europe, 1999: 459), of which none include synonyms, and [**10**] Benjamin (Mushrooms: poisons & panaceas, 1995: 246-247), citing *C. orellanoides* and *C. rainierensis* as independent taxa.

There nonetheless appears to be a steady shift over the past two decades toward adopting *C. rubellus* as the preferred synonym. Prop. 1769 cites 7 references that use *'rubellus'*. My personal library search uncovered 8 more: W.G. Smith (British Basidiomycetes 1908), Kibby (Mushrooms & other fungi, 1992: 119; Mushrooms & other fungi of North America, 1993: 118), Laessøe & al. (The Mushroom Book, 1996: 167), Laessøe & Lincoff (Mushrooms 1998: 72), Breitenbach & Kränzlin (5: 158, 2000), Iliffe (Field Mycology 4: 75-76, 2003), and Shibata (Mycoscience 45: 395, 2004). Five fewer publications compared to those using

'*speciosissimus*,' to be sure, but of the 14 published in the 'post-Melot era,' 10 provide synonyms while another treats two 'synonyms' as independent taxa.

Prop. 1769 quotes Gasparini (2004), who "pointed out that *C. speciosissimus, C. orellanoides,* and *C. rubellus* have at times all been placed in synonymy with each other and concluded that *C. rubellus* should be considered a 'nomen incertum,' and therefore *C. orellanoides* had priority." I do not follow. This implies that a name listing synonyms that elsewhere are treated as independent challenges that name, but such is not the case. Following this line of reasoning, the fact that many authors (including Ryman & Holmåsen) cite *C. rubellus* as a synonym of *C. speciosissimus* would make *C. speciosissimus* a 'nomen incertum.' Gasparini's argument is, at best, unconvincing.

A sentence in the proposal's final paragraph warrants clarification. The statement "Conservation was also suggested by Brandrud & al…." is true, but we should note that they wanted to conserve *C. rubellus* over *C. speciosissimus,* not the other way around. As *'rubellus'* already has priority, there was no need to pursue conservation.

While there has been, and still is, a high degree of taxonomic confusion regarding the names *C. orellanoides, C. rainierensis, C. rubellus,* and *C. speciosissimus,* conservation should not be used to solve a demonstrably taxonomic issue. For the time being, I have no problem with referring the earliest name, *C. rubellus,* to a unified species concept.

Prop. 1770, Proposal to conserve *Calvatia* nom. cons. (*Basidiomycota, Lycoperdaceae*) against an additional name, *Lanopila*. Proposed by Johan Coetzee & Abraham E. van Wyk. Taxon 56(2): 598–599. (2007)

Voted: Vote not yet taken.

Committee comments:

→**DEMO 80102:** I was a lector of Coetzee's thesis and know the issue well. The proposal is OK.

→**REDH 80104:** I support this proposal.

→**NORV 80115:** The proposal logically outlines support for conservation of the well-known name, *Calvatia,* for a cosmopolitan genus represented by >35 medium- to large-sized puffball species that dehisce through irregular fragmentation of the peridia. Typified by *Lanopila wahlbergii* (now a synonym of *Calvatia argentea*), the earlier named *Lanopila* was incorporated into *Langermannia* 44 years ago, during which time the name fell from common use. Kreisel's 1992 widely accepted reincorporation of *Langermannia* into *Calvatia* leaves *Lanopila* as a nomenclatural threat to *Calvatia*. I support conserving *Calvatia* over *Lanopila,* agreeing with the authors that it is a logical next step to the previous conservations of *Calvatia* over *Omalycus, Langermannia* and *Hippoperdon*.

Prop. 1792, Proposal to conserve the name *Phaeographis,* with a conserved type, against *Creographa, Ectographis, Flegographa, Hymenodecton, Platygramma,* and *Pyrographa (Ascomycota: Ostropales: Graphidaceae*), along with notes on the names *Graphina* and *Phaeographina*. Proposed by Robert Lücking, Klaus Kalb, Bettina Staiger & John McNeill. Taxon 56(4): 1296–1299. (2007).

Voted: Vote not yet taken.

Committee comments:

→**REDH 80104:** I support this proposal. It is complex and a similar proposal failed earlier because of the absence of monographic and phylogenetic research, but now that seems to have

been solved. The additional time bought by the earlier failures allowed these authors to also uncover the differences in publication dates that result in a different interpretation of some typifications.

→**NORV 80125 :** Prop. 1792 is indeed complex. Fortunately, the authors deliver a well-composed tour de force that (in hindsight) illustrates the wisdom in waiting to conserve a name until the taxonomy is more or less settled. Lücking & al. clarify a previous nomenclatural morass, yet still evoke the confusion caused by botched reprints so vividly that even non-lichenologists become curious as to how the Gordian knot will be sliced. With the fates of 4 *Graphidaceae* generic names (originally 4-ascospore based) inextricably intertwined, they cover *Graphina*, *Phaeographina*, and *Phaeographis* sufficiently to clarify only the last name requires conservation. The three names were twice proposed for conservation (in 1930 and 1981) but rejected due to their 'uncertain taxonomic application.' The 1981 proposal was debated for 11 years, rejected due to unsettled taxonomy, reopened for further debate for 6 years, and twice more rejected, at which point then Secretary Gams did note "negative votes … should not encourage displacement of these names by older ones."

The new proposal addresses Staiger's concept of the *Graphideacdeae* that for the first time sorts out (morphologically and molecularly) taxonomic relationships between the genera while providing 'a solid framework to establish synonymies and resolve resulting nomenclatural problems.' The ensuing presentation leads me to support conservation of the name *Phaeographis* with the conserved type *(Opegrapha dendritica* Ach.) over 6 earlier names. However, as I have not read background or opposing materials, my decision is not yet firm.

SPECIAL DISCUSSIONS & QUESTIONS:

Ascomycota 70125, to advise the General Committee for Botanical Nomenclature (as allowed by Art. 32.4.) whether the Nomenclature Committee for Fungi considers that the phylum name *Ascomycota* Cavalier-Smith, Biol. Rev. 73: 247 (1998), meets minimal standards for validation.. Input requested by GC Secretary Barrie. For background information, see Letter4Ascomycota.pdf (sent to Committee members by CF Secretary on 22 February 2007).

May 1, 2007, Poll (nonbinding): 11 feel the description is valid & no further discussion needed; 3 want additional discussion; 5 (some voting for additional discussion) abstained.

Background & Guest comments:

Eriksson via NORV 70222: Since my previous mail to you we have had some further discussions in the AFTOL group, which may be of interest to the Nomenclature Committee for Fungi.

To Scott Redhead 2007-02-16: I sent a short synopsis of our discussions to Lorelei Norvell, as you suggested. I suppose the Committee for Fungi will decide that CS´s description was acceptable. But, there are cases when a description is not acceptable - when it is a nonsense description ("fructus delicatus" for a *Malus* sp.), when it is wrong (ascomata almost always blue, for Ascomycota) or when it does not distinguish a group from its sister group (ascomata of septate hyphae). In our case Ascomycota was described "sporae intracellulares", which is correct, but does it distinguish the ascomycetes from the basidiomycetes? It depends on how we interpret basidiospores. The double investing membranes surrounding the nucleus in young ascospores have not been seen in any basidiomycetes (Franz Oberwinkler, in litt.). I would like to see the following Note in our manuscript under Phylum Ascomycota Cavalier-Smith (but modify or correct it if you find errors):

Cavalier-Smith gave a very short description in Latin of the new phylum Ascomycota: "sporae intracellulares". This description is correct. Ascospores are formed within two investing membranes in young asci. No investing membranes have been seen in basidia, but the basidiospores have often been interpreted to consist of an outer layer of "basidial remnants" and an inner "spore proper" (for a comparison between different terms for the basidiospore wall-layers, see Kirk et al. 2001: 61).

Any mycologist studying Fig. 8 in Dictionary of the Fungi must wonder whether ascospores and "spore proper" in basidiospores are homologous or not, despite the latter lack investing membranes, and by that Basidiomycota, in fact, have intracellular spores (enclosed in "basidial remnants"). Whether this is the case or not is not important, but it is important that we inform about this uncertainty, and that we aware of it have accepted CS´s description. It would have been better if CS had separated the two phyla as John Walker did in Fungi of Australia (1996): Teleomorphic spores ascospores > **Ascomycota** [vs.] Teleomorphic spores basidiospores > **Basidiomycota.**

From Scott Redhead 2007-02-16: Firstly, Ove, I think that your extra paragraph would be a good addition, but because the manuscript has been submitted, reviewed, revised, and accepted (and maybe even revised slightly since, such as removing "Bold"), it may be asking too much to add it. But that would be between the senior author and the editor[author]. Certainly, the paper will be published before the Committee for Fungi has debate and recommended things unless something drastic happens.

Secondly, Robert, I agree there is not yet consensus, but I disagree with regard to the elevation of Ascomycetes Berk. It has already been noted that that basionym, if that is what it was supposed to be, was not fully cited (not supplied with a full bibliographic citation), and that the spelling of Ascomycetes would not necessarily change at different ranks because it is not an automatically typified name, and thirdly that Berkeley did not include all Ascomycota, he specifically excluded lichenized Ascomycota, that distinction being a higher level choice for him before he allowed the choice of ascus presence to influence him. It would be a step backwards to go back now in this group to discuss that scenario.

To Scott Redhead 2007-02-19: Thank you for the comments. It was also my conclusion that CS did not fully cite Berkeley and, therefore, only his description should be discussed. It may be argued that morphologically the ascomycetes have intracellular spores, the basidiomycetes have not (as we have now been informed that there have never been found any investing membranes in basidia). Evolutionarily, however, possibly also the forerunners of the basidiomycetes had intracellular spores (judging from the terminology based on TEM graphs in Fig. 8 in Kirk et al. 2001), but that can not be referred to when we discuss the validity of the name Ascomycota Cavalier-Smith. So, I think we can withdraw our questions to the Committee for Fungi and just accept the name.

From Scott Redhead 2007-02-19: Thank you once again for the insight. I think we can proceed on the assumption that Ascomycota CS is valid, but it is not necessary to withdraw the request. There were no examples in the Code for that article, and nobody had ever tested the rule. This could be a test case. Lorelei Norvell (secretary of the Committee for Fungi) had been prepared to ask about it in a expeditious fashion in the up coming circular, I am told.

GAMS 70223: 1. With his minimalized diagnoses, Cavalier-Smith is obviously making a fool of the sacred Art. 36 of the *Code*. A similar case is the order name *Mortierellales,* which he introduced with a quite inappropriate diagnosis but which is now generally recognized (e.g. AFTOL). The only question that remains is its relationship to the *Endogonales,* a name that of course is older but probably refers to a distinct order.

2. The division name *Ascomycota* is much older than Cavalier-Smith 1998. It has only not been formally introduced with a Latin diagnosis. The question remains about the basionym, whether such a raise in rank can be recognized. E.g., while compiling recent taxonomic mycological literature, Gams & Jülich in Prog. Bot. 46: 279, 1984 changed from *Ascomycotina* (used in the preceding 1982 issue of the series) to *Ascomycota,* following M. E. Barr in Mycologia 75: 1-13, 1983, where she uses the division rank without explicit diagnosis and author citation; Müller & Loeffler, Mykologie, 5th edn, p. 216, 1992, also use the rank of division (without author citation, only a German text).

3. I feel it is too much honour for the minimalistic approach by Cavalier-Smith to ascribe the name *Ascomycota* to him. We should find a way of recognizing the simple raising of rank by, e.g. Margaret Barr, in the authorship.

TRIE 70223: In reading all the comments on /Ascomycota /Cavalier-Smith I think that the two Latin words have to be treated as a correct description and therefore the name should be regarded as validly published. We should make a recommendation for that to the General Committee.

Eriksson via NORV 70223: I was much against Cavalier-Smith's validation of the name *Ascomycota* as his concept of the phylum seemed to be based on old literature (Syllabus der Pflanzenfamilien, and other works) and did not considered molecular data and modern literature. But I had to give up as I was told

there were formal ICBN reasons for accepting **Ascomycota** Cavalier-Smith. I can now see two alternatives:

1. **Follow the ICBN strictly** and accept *Ascomycota* Cavalier-Smith. Do then not use that name in practice but *Ascomycota* sensu NN & NN 200x, which should be the first published version of the concept that is currently accepted (i.e. its subdivision into subphyla and classes).
2. **Do not follow the ICBN,** but the Committee for Fungi decides that *Ascomycota* starts with a publication with a modern concept of the phylum.

We have the following list to begin with, but you can certainly find more.

Berkeley M.J. 1857. *Introduction to Cryptogamic botany*. H. Bailliere, London. The name was ***Ascomycetes*** and not Ascomycota. His group did not include the lichens.

Bold H.C. 1957. *Morphology of plants*. Harper & brothers Publ., New York. The first time the name ***Ascomycota*** is used (for a division), but a *nomen nudum* and without a modern subdivision of the phylum: one class and 5 orders (pp. 196-197: Class: *Ascomycetes* [comprising the following orders and families]: Endomycetales (Endomycetaceae), Aspergillales (Aspergillaceae), Sphaeriales (Fimetariaceae), Erysiphales (Erysiphaceae), Pezizales (Pezizaceae)

Whittaker R.H. 1959. On the broad classification of organisms. *The Quarterly Review of Biology* 34: 220 (*nomen nudum*). Without a modern subdivision of the phylum.

Barr M.E. 1983. The ascomycete connection. *Mycologia* 75: 3 (*nomen nudum*) with a classification of classes and subclasses that is not based on molecular data and that is very different from the one we have in the AFTOL paper.

Eriksson O.E. & Winka K. 1997. Supraordinal taxa of Ascomycota. *Myconet* 1: 4. (http://www.fieldmuseum.org/myconet/printed.asp). The Ascomycota and Basidiomycota were discussed as follows (signatures in nSSU rRNA, numbering according to Van de Peer et al. 1996, 1997): ***Ascomycota***: There are usually no problems to determine whether a fungus belongs to the phylum *Ascomycota* or to the *Basidiomycota,* but some yeasts, endophytes, and fungi that do not produce any kind of diaspores can be difficult to identify to phylum. There are, however, a number of molecular signatures that are diagnostic (see *Basidiomycota*).

Basidiomycota: The following sites in our matrix of SSU rRNA showed differences between *Basidiomycota* and *Ascomycota:* *Signatures in *Basidiomycota:* 8 114c:a; 303u:a. 8/9 126a:g. 9 140u:a; 150c:u. 11/8' 291g:c. 12 335u:a. 13 369u:a. 17 480g:a (exc. *Coprinus*). 23/23-1 638c:u; 640u:c (exc. u in Boletus, Ustilago). 23-2 699u:c. 23-2/23-5 704c:u. 23-7 785u:c; 792c:u. 23-9' 848c:u. 25 883c:u. 23' 970g:a. 23'/27 978a:g. 27 993g:a; 1009c:u. 27/28 1021c:a (but u in *Sporobolomyces, Leucosporidium*); 1022a:g. 37 1239g:u. 41 1306c:g; 1313g:c. 46 1482a:g (exc. a in *Ustilago, Tilletia, Udenomyces*). 46'/45' 1521u:g. 48 1589c:u (exc. c in *Ustilago*). 1590a:g (exc. a in *Ustilago*). 1602u:c (exc. u in *Ustilago).*

Cavalier-Smith T. 1998. A revised six-kingdom system of life. Biological Reviews 73: 247. With a Latin description of two words ("sporae intracellulares"). His classification of ***Ascomycota*** is quite unacceptable and resembles old "practical" systems.

Hibbett D. et al. 2007. A Higher-Level Phylogenetic Classification of the Fungi. *Mycological Research* (submitted manuscript). The classification of subphyla and classes follows that in Eriksson & Winka 1997 with some exceptions: ***ASCOMYCOTA*:subphylum *Taphrinomycotina*** O.E. Erikss. & Winka 1997 [with classes] *Neolectomycetes* O.E. Erikss. & Winka 1997, … *Pneumocystidomycetes* O.E. Erikss. & Winka 1997, … *Schizosaccharomycetes* O.E. Erikss. & Winka 1997, … class *Taphrinomycetes* O.E. Erikss. & Winka 1997; s**ubphylum *Saccharomycotin*** O.E. Erikss. & Winka 1997 (class *Saccharomycetes* O.E. Erikss. & Winka 1997); **subphylum *Pezizomycotina*** O.E. Erikss. & Winka 1997[with classes] … *Arthoniomycetes* O.E. Erikss. & Winka 1997, … *Dothideomycetes* O.E. Erikss. & Winka 1997, … *Eurotiomycetes* O.E. Erikss. & Winka 1997 (incl. class *Chaetothyriomycetes* O.E. Erikss. & Winka 1997) , … *Laboulbeniomycetes* A. Engl. 1897 (class accepted after 1957) , … *Lecanoromycetes* O.E. Erikss. & Winka 1997, … *Leotiomycetes* O.E. Erikss. & Winka 1997, … *Lichinomycetes* V. Reeb, Lutzoni & C. Roux (new class after 1997) , … *Orbiliomycetes* O.E. Erikss. & Baral 2003 (new class after 1997) , … *Pezizomycetes* O.E. Erikss. & Winka 1997, … *Sordariomycetes* O.E. Erikss. & Winka 1997

If the Committee for Fungi decides to choose alternative 2, I think the best solution would be *Ascomycota* Hibbitt et al. 2007. There are several reasons to choose that solution.

1. The paper is published in a journal that is widely distributed and available electronically.

2. It contains the current classification of *Ascomycota,* based on both molecular and morphological data
3. Many in the AFTOL group have been involved in the discussion on the nomenclature of the name *Ascomycota*.

For a description a reference may be made to the molecular differences identified by Eriksson & Winka 1997.

PRIN 70301: If I understood everything correctly, Scott Redhead outlined three possible ways to interpret Cavalier-Smith's description of *Ascomycota*. (1) Recombination of Berkeley's (1857) name to a different rank ("new status of something published by Berkeley" as Scott put it), (2) new description of "*Ascomycota*" validated by reference to Berkeley (1857) or (3) new description of "*Ascomycota*" validated by a Latin diagnosis. The fact that Berkeley's name was "*Ascomycetes*" and not "*Ascomycota*" could perhaps be interpreted as an error in the citation of the basionym (33.5). But both (1) and (2) require direct reference to Berkeley (1857) which is clearly not the case here. Missing page numbers are an omission according to Article 33.5, which would make the name invalid.

It remains to clarify whether the Latin diagnosis fulfills the requirements of Article 32.2. Ove Eriksson indicated that the important point is whether the two-word Latin description "defines the *Ascomycota* versus *Basidiomycota*". However, in light of Art. 32.2 it is not relevant whether the description objectively does so. For a valid description of the name "*Ascomycota*", it suffices that the description is distinctive "in the opinion of its author". This is clearly the case here. The example of "fructus delicatus" given by Ove Eriksson is not a good example, because it is purely aesthetic and therefore, according to Article 32.3, would not validate a description.

I think that Cavalier-Smith published a valid new name.

REDH 70301: While I continue to appreciate and am sensitive to the discomfort in accepting Ascomycota Cavalier-Smith because of the reasons given by Ove Eriksson, I do not think that the Committee for Fungi currently has the mandate to make a decision on option 2 in Ove's email of Feb. 23/07. Firstly, the General Committee was asked for a clarification under Art. 32.4 as to whether a descriptive statement satisfies the requirements of Art. 32.1(d). Note that 32.1(d) does not specify Latin. It is Art. 36 that specifies Latin requirements and that special provision is covered by Art. 32.1(e). Nonetheless I think we can assume that we should be looking at the Latin. For this we must look at Art. 32.2, and ask did the author publish a statement that in his opinion distinguished the Ascomycota from the Basidiomycota (the only two taxa he compared). And the answer is yes, in his opinion he did that. In fact he was purposely trying to validate many higher level taxa by fulfilling the requirements of the Code. We are not being asked to judge whether in the opinion of others, whether the diagnosis was accurate or whether he used correct terminology. In his mind, he did it correctly.

That being said, I will feel obliged to say yes, the name Ascomycota Cavalier-Smith is valid. However, illegitimacy based upon homonymy only extends to family level (Art. 53.1). Illegitimacy because of being superfluous is also restricted to those names with types (Art. 52.1). Descriptive names such as Ascomycota do not have types (Art. 16.1). And the principle of priority does not apply above the rank of family (Art. 16, Note 2). One could create a second Ascomycota (which would need a Latin diagnosis or description to be valid), and use it rather than Ascomycota C-S. It would be difficult to argue for conservation of it over the other given that there is no priority issue. The question would be, why do it? But I am willing to go along with whatever is the majority decision by the CF in their suggestion to the GC.

HAWK 70326: As the Code specifies "in the opinion of the author", not whether the author is right or wrong or makes any scientific sense, we are stuck with this though I would still insert "Bold ex" and will propose altering the next Code to enable that to be done where authors have blatantly validated already in-use names in works they cite but do not mention in the citation. In this case Cavalier-Smith clearly had the 1995 Dictionary of the Fungi (which he quotes) and used "*Ascomycota*" but just did not want to give any credit to earlier authors which is what so annoyed Ove! OK, the Dictionary did not mention Bold, but his usage is traceable back through Whittaker's papers.

GAMS 70410: I gather that it should be possible one way or the other to retain Bold as author of the division name. To me the correct inclusion of the relevant subtaxa has much more impact, independent of a Latin diagnosis, than a however poorly written "Latin" diagnosis.

PENN 70415: [I] abstain [on the ballot vote], but I believe further discussion is needed.

CRAN 70424: I hope we have a little more discussion on the validity on the name *Ascomycota.* There is a lot of discussion going on about Cavalier-Smith's work but it seems to be taking place outside the committee and there is rumor of another phylogeny of the fungi to be published in Mycological Research. In older literature, I have seen diagnoses at the species level of one Latin word and a measurement as: Sporae 12-25 X 4-8 micrometers. At the level of *Ascomycota,* Cavalier-Smith should have included the terms ascus and ascospores.

→***Blastocladiomycota* 70425&731:** to advise the General Committee for Botanical Nomenclature (as allowed by Art. 32.4) whether the Nomenclature Committee for Fungi considers that the phylum name *Blastocladiomycota* Doweld, Prosyllabus Tracheophytorum tentamen systematis plantarum vascularium (*Tracheophyta*)" p. LXXVII (2001) to be validly published by meeting minimum standards for valid publication. Input requested by GC Secretary Barrie on July 31 to review the question of whether or not Doweld's Latin diagnosis meets the minimal standards for publication. in conjunction with the previous discussion on the validity of *Ascomycota* Cavalier-Smith.

Scott Redhead's text below provides the necessary background information (see also Dowald2001.pdf) and Fred Barrie's text adds questions that should be addressed.

→**REDH 70425:** Once again, on the behalf of a group of mycologists involved in phylogeny and the classification of fungi at higher level ranks, I am formally asking for the General Committee as outlined in Art. 32.4 to help resolve whether a name is valid under Art. 32.1(d) and Art. 32.2. Previously we had questioned whether *Ascomycota* was valid and we are still debating that issue. However, now we have run into an even more marginal case that I believe does not meet the standard, hence in my mind is invalid.

In 2001, Alexander Doweld published a 110 page book in English and Russian (with considerable Latin content), the "Prosyllabus Tracheophytorum tentamen systematis plantarum vascularium (*Tracheophyta*)", which was reviewed in Taxon 53:231-232 (2004), and which escaped notice by mycologists until this year because the title did not reflect a mycological content. The book was published in advance of an upcoming publication, a new syllabus, intended to replace or update Engler & Prantl's opus. Prof. Doweld, who served on the Special Committee on Higher Level names that reported to the Vienna Congress, is well versed in nomenclature. In the Prosyllabus he presented an overall classification of the tracheophytes that included automatically typified names at all ranks, including kingdom level. In the section, "Conspectus dispositionis Tracheophytorum" (pgs. I-LXIII), bryophytes, flowering plants, conifers, ferns, club mosses, etc were covered, and many new names were proposed, almost all with their own Latin diagnoses, and typifications noted in linked footnotes. Occasionally prof. Doweld changed the status of a previously published name, such as creating subclass *Juncidae* (p. LXII) by elevating the order *Juncopsida* Bartl. (1830).

However, prof. Doweld went further than just publishing on tracheophytes, and published an appendix, "Conspectus dispositionis dominii *Eucaryota*" (pgs. LXVII-LXXIX) in which he treated algae, protozoans, fungi, sponges, and miscellaneous other groups. In the appendix he relied heavily upon previously published synopses, and for the fungi, he particularly relied upon two sources: one restricted to *Ascomycota,* namely Eriksson, O.E. & Winka, K. (1997). Supraordinal taxa of *Ascomycetes.* Myconet 1(1): 1-16; and the other, Cavalier-Smith, T. (1998) A revised six-kingdom system of life. Biol. Rev. Cambridge Phil. Soc. 73: 203-266. The latter is the same publication being discussed in regard to the name *Ascomycota.* In the Prosyllabus Appendix, the many new names are mainly based upon changed status and borrowed (cited) Latin published for other names. For example, Doweld simultaneously proposed the names

Phyl.: *Developayellomycota,* Class: *Developayellomycetes,* Order: *Developayellales,* and Family: *Developayellaceae* (p. LXXI) by reference to the exact same Latin diagnosis published by Cavalier-Smith (1998) for the subphylum name *Bigyromonada.* The nested taxa do not differ in circumscription from one another except in rank.

Questionable as that honesty may be, in the case of *Blastocladiomycota,* he proposed to validate that name as a nomen novum for the infraphylum name *Allomycotina* Cavalier-Smith 1998: 246, via reference to the Latin under that name. *Allomycotina* was typified by the genus *Allomyces,* but there is no family name *Allomycetaceae,* and following Art. 16.1 (although not cited) Doweld replaced all such higher level names not based upon legitimate family names, with names based upon those with legitimate family names. However, it cannot be interpreted as a nomen novum (there potentially being two different types, *Blastocladia* versus *Allomyces*) and therefore the name *Blastocladiomycota* can only be interpreted as a wholly new name, requiring a Latin description or diagnosis.

It is the inadequacy of the Latin as a diagnosis, as defined by Art. 32.2, that is at question, because we do not believe that Doweld at all understood the fungi or what he was trying to classify. The lamentably brief Latin diagnosis provided by Cavalier-Smith (1998, p. 266) for *Allomycotina* read "zoospora cilio unico instructa", which means "with uniciliate zoospores". Within the framework of Cavalier-Smith's classification, this diagnosis made sense (to Cavalier-Smith the way he presented it, although it may be disputed by more knowledgeable mycologists). Cavalier-Smith classified *Allomycotina* in subphylum *Melanomycotina* which was within the phylum *Archemycota* within subkingdom *Eomycota.* Cavalier-Smith differentiated the subphyla within *Archemycota* by features of the golgi apparati. Within this framework, it made sense to differentiate infraphylum *Allomycotina* from infraphylum *Zygomycotina* by the presence of uniciliate zoospores (in fact any zoospores), because the other uniciliate taxa in *Archemycota* were in another subphylum, namely *Dictyomycotina,* where the class *Chytridiomycetes* was placed.

Prof. Doweld took an appropriately worded Latin diagnosis (within a classification framework by one author) and applied it to a distinct classification of his own, where it is woefully inappropriate and inadequate (in my mind) and couldn't possibly have served to differentiate *Blastocladiomycota* from *Chytridiomycota.* The proposed classification places phylum *Blastocladiomycota* seemingly in a list of Incertae sedis on par with phylum *Chytridiomycota* in the same subkingdom, *Mucorobiotina,* and which has many taxa producing uniciliate zoospores. There is no discussion on the differentiation of the phyla from one another, there being only reliance on Cavalier-Smith's classification, but also the abandonment of Cavalier-Smith's hierarchy. Taken out of context, the cited Latin fails to be a "statement of that which in the opinion of its author [Doweld] distinguishes the taxon from other taxa" (Art. 32.2). The author, Doweld, failed to distinguish the phyla in subkingdom *Mucorobiotina* from one another, and therefore it is not a diagnosis (despite being in Latin) as defined by Art. 32.2. As such it fails to fulfill Art. 32.1d – i.e. it is not a diagnosis. The Latin phrase cited is simply nonsense in this case.

We mycologists therefore do not recognize *Blastocladiomycota* as having been validly published in 2001. Nonetheless, we would like a formal ruling on this issue, to ensure it is a correct decision. Addressing this issue in combination with that on the *Ascomycota* will help define the limits of Art. 32.1 and perhaps provide examples for Art. 32.4.

Please find attached several pages from Prof. Doweld's 2001 publication. Previously you had available the publication by Cavalier-Smith.

We are also entertaining the idea of suppressing the entire Appendix (all fungal, protozoan and algal names). For this reason, perhaps the committee dealing with algal names could also be consulted, in addition to that for fungi.

→**GC Secretary BARRIE 70731:** Several points the committee may wish to consider in its debate are: **a)** To what degree need we worry about names published above the rank of family, where the principle of priority does not apply? and **b)** even if Doweld's statement is not diagnostic, as defined in Art. 32.2, is it a description? Must a description be diagnostic?

[CF Secretary note: I had assumed it was necessary to present such 'extra-ordinary' requests only after direct request by the GC Secretary. Barrie's explanation clarifying the usual protocols is retained here for the committee's benefit, although not pertaining to *Blastocladiomycota* directly.] As to protocol: Article 32.4 was deliberately worded similarly to Art. 53.5, stating that a request for a decision may be submitted to the General Committee for consideration. Article 53.5 goes back a long way, and, as far as I know, in practice most queries have been sent not to the Secretary of the GC but directly to the Secretary of the relevant Permanent Committee [CF Secretary note: the term "permanent committee" here refers to nomenclature committees such as the Committee for Fungi, for Vascular Plants, etc.]. The Secretary for the Committee for Vascular Plants, for example, has received many requests under Art. 53.5, and probably, now, a few under Art. 32.4, that neither I nor my predecessor learned about until we received a copy of his circular. As with proposals published in Taxon, the GC has never demanded that a PC refrain from reviewing such a proposal until a formal, written request from the GC Secretary has been sent. In cases where the request has been sent exclusively to me, obviously I would refer it, since the PC Secretary would be unaware that the request had been submitted. However, unless there is a question as to whether or not the proposal is appropriate for the committee, or if you'd like some weight behind refusing to consider a proposal, please consider any submission under Art. 32.4 or 53.5 sent to the PC Secretary as a submission to the General Committee and a request by the GC that the proposal be reviewed.

→**NORV 80125:** Discussion of *Blastocladiomycota* should also cover two publications not alluded to above. In their discussion, "A molecular phylogeny of the flagellated fungi (*Chytridiomycota*) and description of a new phylum (*Blastocladiomycota*), James et al. (Mycologia 98: 860–871) erect *Blastocladiomycota* T.Y. James with a Latin diagnosis and no reference to Doweld's 2001 work.

The name *Blastocladiomycota* T.Y. James was subsequently adopted in "A higher-level phylogenetic classification of the *Fungi* by Hibbett et al. (Mycol. Res. 111: 509–547), although the authors, unlike James et al., were at that time aware of Doweld's (2001) publication. Although, not stated, it was the opinion of the authors that Doweld failed to meet minimum standards, which precipitated this request to the Committee for Fungi.

To the root of the R~RR~RH~RRH problem:

→**KIRK 70727** requests advice regarding the spelling of an epithet containing '...**riz.**..' or '...**rriz.**..' or '...**rhiz**...' or '...r**rhiz**...' "I am now in discussions with a 'third party' as to what the correct spelling of *Pisolithus arrhizus* should be (I use this form only because it gets the most hits on Google - not the best way to make decisions but ...).

Lycoperdon arhizon Scop. - as cited in Persoon in the synonymy in the Synopsis - but with 'arrizon' cited in Index Fungorum as the original orthography (can someone check this in the original?).

Scleroderma arhizum Scop.: Pers. - the sanctioned spelling

Pisolithus arhizus (Scop.: Pers.) Rauschert

So, should we stick with the sanctioned spelling of 'arhizus' or adopt the 'botanical Latin' spelling of 'arrhizus'?

GENERAL REMARKS:

Article 59

The Special Committee on Names for Pleomorphic Fungi (Chair Rossman, Secretary Redhead, and members Bischoff, Gams, Grgurinovic, Hawksworth, Hosoya, Kirk, Sampaio, Seifert, Spiegel & Wingfield) has been formed. Members are instructed to evaluate the impact of modifications (or elimination of) **Art. 59**, and forward recommendations to the Nomenclature Committee for Fungi and General Committee for further input before presentation to the Nomenclature Section at the 18th Botanical Congress in Melbourne. Please contact Scott Redhead for additional information.

Walter Gams resigns

Walter, member since 1984 and CF Committee Secretary for 12 years ending in 2005, officially resigned his Committee membership in September. We will miss his valuable input and wish him well in his working retirement. His new contact information is Walter Gams, Molenweg 15, 3743 CK Baarn, Netherlands; E-mail (NEW) <walter.gams@orange.nl>.