

# NOMENCLATURE COMMITTEE FOR FUNGI

31 May, 2007

## 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. 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.

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 of the appropriate section.

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.

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 **10!**) 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. 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).

### 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 is 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 "Compendium 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.

**Prop. 1709**, to conserve the name *Chroogomphus* against *Brauniellula* (*Gomphidiaceae*, *Agaricales*, *Basidiomycota*). Proposed by M. Catherine Aime & Orson K. Miller, Jr. †. *Taxon* 55 (1): 228.

**Voted May 1, 2007:** 16 : 0 : 0 : 0 : 0. With 100% CF support, this proposal will be removed in the next Commentary.

**Committee comments:**

**NORV 60915:** I foresee a number of like proposals for the future as morphologically based generic concepts give way to 'sequence-based' generic concepts. The decision to confer conservation upon the more widely known name would be well advised in this instance, presuming that the taxa are congeneric. I support this proposal.

**REDH 61130:** This makes sense as *Chroogomphus* is far better known. The only question arising here is whether the two are synonymous. Manfred Binder may have additional information on these genera and had questioned it. We should ask for his input into the taxonomy. Nonetheless I would support the proposal.

**GAMS 70223:** Well founded for a common agaric.

**MAY 70228:** I support this proposal. There are likely to be some more situations where molecular data confirms that well-established genera introduced for agarics are predated by genera introduced for gasteroid forms (secotioid, hymenogastroid, etc). I don't think that in such cases the agaricoid genus should automatically be given privilege over the gastroid - we need to get used to this intriguing mix of gross morphologies in genera of the euagarics and elsewhere. Nevertheless, for *Chroogomphus* there are good reasons to conserve the name against *Brauniellula*, both in terms of the relative number of species involved, and also because of the importance of *C. rutilus* as a red-listed species with interesting chemical properties.

**PENN 70321:** I support the Proposal, provided all available molecular evidence confirms that the two taxa are congeneric.

**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 be removed in the next Commentary.

**Committee comments:**

**NORV 60915:** The proposal well clarifies the Byzantine nomenclatural history behind *Milospium graphideorum* (Nyl.) D. Hawksw. and provides good support for designating a type based on the lichenicolous hyphomycete. I support this proposal.

**REDH 61130:** I know little about these fungi, but the proposal made sense to me and effectively will stabilize the situation.

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

**MAY 70228:** Conservation of *Spilomium graphideorum* with a conserved type for the lichenicolous component is worthwhile in protecting usage of both *Milospium* and *Lecanographa*.

**TRIE 70319:** I support this proposal for stability reasons.

**PENN 70321:** I support the Proposal; conservation provides an elegant solution to a tangled historical situation.

→ **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.

**Prop. 1730**, to conserve the name *Strigula schizospora* against *S. gibberosa* (*Ascomycota*: *Pryenulales*: *Strigulaceae*). Proposed by Robert Lücking. Taxon 55: 801.

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

**Committee comments:**

**REDH 61130:** This proposal made sense to me. I support it.

**NORV 61215:** Lücking provides excellent support for conserving a "widely accepted and commonly used name for a widespread and abundant species" over an earlier named synonym. I support the proposal.

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

**PENN 70321:** I support the Proposal, conserving a widely accepted name with a quality type, and rejecting an obscure name with a depauperate type.

**Prop. 1731,** to conserve the name *Lichenochora* against *Paralaestadia*. Proposed by Nikolaus Hoffmann, Josef Hafellner & David L. Hawksworth. Taxon 55(3): 802.

**Voted May 1, 2007:** 14 : 0 : 0 : 2 : 0. With 88% CF support, this proposal will be removed in the next Commentary.

**Committee comments:**

**REDH 61130:** I support this proposal.

**NORV 61215:** The authors explain to my satisfaction why the name for a genus already containing 25 species but proposed only 17 years ago should be conserved over a 'name apparently never used since 1921 for these fungi.' I support this proposal.

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

**MAY 70228:** I support this proposal. The earlier name *Paralaestadia* has been so rarely used that it is sensible to conserve *Lichenochora*. The second last paragraph of the proposal is confusing in that it states that Hoffmann and Hafellner (2000) 'introduced the new generic name *Lichenochora*'. Since it was in this publication that the type of *Paralaestadia* was found to belong to *Lichenochora*, this statement creates the impression that a new name was introduced where an old one was known to be available. However, as is clear from the publication details at the head of the proposal, *Lichenochora* was introduced by Hafellner in 1989. It is also relevant that most of the 25 species of *Lichenochora* were introduced (or combined in) that genus prior to 2000, again supporting conservation of *Lichenochora* rather than resurrection of *Paralaestadia*.

**TRIE 70319:** I support this proposal for stability reasons.

**PENN 70321:** I support the Proposal.

**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).

**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.

\*\*\*\*SECRETARY'S COMMENT: Discussion pertaining to the three proposals as a unit will be placed after the above header. Comments directed to only one proposal will be placed after the appropriate header below along with ballot results.]

#### Committee comments:

**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.

→**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, designate 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).

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

**Voted May 1, 2007:** 11 : 2 : 2 : 1 : 0. With 69% CF support but with 2 votes for discussion, this proposal is temporarily retained for comment.

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

**Voted May 1, 2007:** 11 : 2 : 2 : 1 : 0. With 69% CF support but 2 votes for discussion, this proposal is temporarily retained for comment.

**PENN 70321:** (*C. empetri* & *C. piperiana* – Props. 1733–1734) I support the Proposals, but could be persuaded in favour of the epitypification alternative.

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

**Voted May 1, 2007:** 10 : 2 : 3 : 1 : 0. With 63% CF support but with 3 votes for discussion, this proposal is temporarily retained for comment.

**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.

**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 : 0. With 88% CF support but temporarily retained for comment by request of the Chair (see DEMO 50402).

#### 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.

**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 temporarily retained for comment by request of the Chair (see DEMO 50402).

**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.

**Prop. 1740**, to conserve the name *Agaricus lepideus* against *A. suffrutescens* (Basidiomycota).

Proposed by Scott A. Redhead, James Ginns & Tom. W. May. Taxon 55(4): 1030–1032.

**Voted May 1, 2007:** 16 : 0 : 0 : 0 : 0. With 100% CF support, this proposal will be removed in the next Commentary.

**Committee comments:**

**NORV 70205:** The widespread and well-known train-wrecker, *Neolentinus lepideus*, is definitely a destructive fungus of significant economic importance. It is indeed unfortunate that when Fries proposed *A. lepideus* in 1815, he sanctioned it against older synonyms but not against *A. suffrutescens*, another earlier named taxon (1805) that he regarded as a distinct species. Eventually, the lesser-known ‘*suffrutescens*’ came to be regarded as a synonym of ‘*lepideus*’ by most mycologists. This situation continues to the present day even though May & Wood (1995) treated ‘*lepideus*’ as a synonym of their new combination, *Neolentinus suffrutescens*, correctly in accordance with the Sydney Code (1983). This confusing nomenclatural situation is exacerbated by the fact that some authorities list the two names as synonyms while others continue to treat them as independent species.

This situation is definitely complex. As Redhead & al. note, “Adoption of *N. suffrutescens* is also historically unusual in that its basionym is not the oldest validly published name, but enjoys a special status because of a series of unanticipated repercussions of changes to the Code otherwise designed to stabilize nomenclature.” Here the proposers equate ‘valid publication’ with the fact that as both names were sanctioned by Fries in 1815, they both have the same ‘valid’ publication date. The fact that ‘*suffrutescens*’ as applied to the taxon in question is definitely 10 years older than ‘*lepideus*’ definitely

confuses, however. The fact that the fungus in question is economically well known under the ‘*lepideus*’ epithet and that May (of May & Wood, 1995) is a co-author – along with a desire to cut short potentially pointless semantic discussions — all lead me to support this proposal.

**GAMS 70223:** Another very well known species that deserves to retain its name. The formulation in line 3 of Norv 70205 is not quite adequate: There are simply 2 competing, sanctioned names, among which normally the oldest must be chosen.

**NORV 70223:** GAMS 70223 is correct that NORV 70205 confuses the issue. On first reading the proposal, I erroneously focused on the species epithet rather than the name *Neolentinus suffrutescens*, which, in fact, was NOT proposed by May & Wood until 1995. I regret any confusion my incorrect comment may have caused!

**PENN 70325:** Although recent New Zealand usage has favoured *A. suffrutescens*, I am persuaded that the conservation of *A. lepideus* is sensible and desirable.

**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 will be removed in the next Commentary. However, the Secretary will forward to the General Committee the Chair’s recommendation to correct the orthography of *guepiniformis* to *guepiniiformis* (see DEMO 50402).

**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.

**Prop. 1742**, to conserve the name *Lyophyllum* with a conserved type (*Basidiomycota*). Proposed by Scott A. Redhead, Valerie Hofstetter, Heinz Cléménçon, 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 temporarily retained for comment.

**Committee comments:**

**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.

**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 than 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 leucophaetum*, 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.

**Prop. 1743**, to conserve the name *Hygrophorus hudsonianus* against *Endocarpon viride* and *Normandina davidis* (lichenized *Basidiomycota*). Proposed by Scott A. Redhead & Per M. Jørgensen. Taxon 55(4): 1036–1037.

**Voted May 1, 2007:** 14 : 1 : 1 : 0 : 0. With 88% CF support but with one vote for discussion, this proposal is temporarily retained for comment.

**Committee comments:**

**NORV 70210:** An overdue proposal that I firmly support.

**GAMS 70223:** It is an awkward situation that when a particular genus has been formally rejected, the associated species must be rejected in a separate action. But so it goes.

**PRIN 70301:** I support the conservation of *H. hudsonianus* against *Endocarpon viride*. But when I read that the East Asian material of *Normandina davidis* has more deeply lobate thalli, I wonder whether it is completely certain that it is synonymous with *H. hudsonianus*, especially as the authors cite more East Asian material with the same character. Is there any East Asian material known with "typically" lobed thalli? If not, conservation against *N. davidis* seems somewhat premature to me.

**PENN 70325:** I support this Proposal, completing unfinished business from the St Louis Congress, and conserving a widely used name against two obscure names based on sterile thalli.

→**REDH 70329:** (Re PRIN 70301) Seemingly there were less lobed specimens from eastern Asia, but of course unless one does molecular analysis, nothing definitive can be said. Ernie Brodo had no difficulty in identifying the type as being conspecific and we looked at additional samples from Canada and Eurasia comparatively. Also, even if later proved to be nonsynonymous, the epithet *davidis* is still available when not synonymized. Conservation only applies when synonymous. Notably we two authors were polar opposites on the earlier issue of rejection of the generic names, but now that water has passed under the bridge (as the saying goes), we wish to move on to tidy up things in a spirit of cooperation.

**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; not included on May ballot as not all members have had time to consider the proposal.

**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.

**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; not included on May ballot as not all members have had time to consider the proposal.

**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.

### SPECIAL DISCUSSIONS:

**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.

### 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 “sporaes 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: “sporaes 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*, 5<sup>th</sup> 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 (Fimetiariaceae), 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 (“spora 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] *Neoelectromycetes* O.E. Erikss. & Winka 1997, ... *Pneumocystidomycetes* O.E. Erikss. & Winka 1997, ... *Schizosaccharomycetes* O.E. Erikss. & Winka 1997, ... class *Taphrinomycetes* O.E. Erikss. & Winka 1997; **subphylum Saccharomycotina** 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* Hibbett 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*.

**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.

**Timing of ballots**

→**DEMO 70502:** We are in a difficult period in the university with major changes in cursus, supposedly for the purpose of European uniformization. Since February I hardly could get out of University work... As to proposals published in Taxon 55(4), my opinion as president is that they should not have been put on a ballot yet, given the fact this number was only received here in February. I however decided to vote on the apparently straightforward cases.

**Harrie Sipman retires from Committee after 14 years**

**From the Secretary:** Harrie, who has served the Nomenclature Committee for Fungi well since 1993, requested to resign in March, effective with his return of his 1 May 2007 ballot. We regret seeing such a reliable and informed member leave the Committee and wish Harrie well.