

NOMENCLATURE COMMITTEE FOR FUNGI
NOMENCLATURE CF COMMENTARY 9 — 15 JULY 2010

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 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 one to four times a year. 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. All commentaries, other committee correspondence, and formal reports can be accessed by members and non-members via the International Mycological Association website [<http://www.ima-mycology.org/CFF>].

Discussions are presented in four sections in the following order: PROPOSALS TO CONSERVE OR REJECT NAMES, PROPOSALS TO AMEND THE CODE, SPECIAL DISCUSSIONS & QUESTIONS (if present), 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 (Y)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 (Y)YMMDD.

Ballots will be held one 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 (9) of the complete 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.

Lorelei Norvell, *Committee Secretary*
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Proposals to conserve or reject names —

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 1 JUNE (EXTENDED TO 3 JULY) 2009: 4 : 4 : 4 : 1.

[To appear on Ballot 2010-1]

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-leaved 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-leaved 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 UNITE	Google	ITS GenBank	
<i>C. orellanoides</i>	1	953	1	1
<i>C. speciosissimus</i>	26	6650	0	0
<i>C. rubellus</i>	0	4930	2	3
<i>C. rainierensis</i>	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.

Joe Ammirati (Seattle WA) **80203:** (Responding to the phylotree included by Peintner 80201): I am not at all surprised by the result of the phylogeny. They don't always turn out to be so well divided but it has to be checked, [which] is why I put in the comment that there could be several "taxa", phylogenetically/biogeographically. I think with this information in hand I would not make a name change at the moment: fungus species are not about priority, but rather about relationships and change over time. One thing that needs to be done is extensive sampling across populations to be sure that variation within species is not greater than between species. I would stick with 'rubellus' for the time being, Gasparini notwithstanding.

Egon Horak (Innsbruck) **80215:** For obvious reasons, I agree with the three authors of the proposal to conserve *Cortinarius speciosissimus*. I disagree, however, with the arguments, e.g.:

1. *Cort. speciosissimus* K. & R. (1953): a) Is the combination valid? b) Have the authors re-examined Favre's type material (GC) and were the morphotaxonomic characters compared with 3) and 4)?

2. *Cort. rubellus* Cooke. - Was the authentic material collected in conifer or broadleaf forest? – In my opinion, the material for a selected lectotype (rather neotype!, cf. Brandrud & al.) must be based on fully documented specimens collected at or near the type locality (situated in conifer or broadleaf forest?).

3. According to the authors the type material of *C. orellanoides* Rob. Henry is characterized by the lectotype 80767, found in association of *Fagus* (?). The specimen reportedly is kept in PC and I see no reason why it is not available for analysis. Try again and I am confident that the Curator will help to locate the specimen(s).

4. *Cort. rainierensis* A.H. Smith & Stuntz. Who has re-examined the microcharacters of the type material, and is the vegetation (ectomycorrhizal host tree) at the type locality actually known?

Summing up:

1. The authors are requested to present a comparative study relating to all relevant microcharacters found on type material ref. to 1,3,4.

2. In addition data of pigment analysis are necessary to back up the identity of 1,3,4, before synonymy actually can be taken into consideration and qualify for proposal.

3. More emphasis must be directed to the ecology, i.e. which ectomycorrhizal host tree(s) is (are) in association with the species of *Cortinarius* enumerated above. It is unlikely that any one of these taxa of *Cortinarius* routinely enter ectomycorrhiza both with conifers (subalpine) and broadleaf trees (lowlands).

4. In the proposal no references or discussions are found ref. further taxa belonging to the „Orellani“-complex.

5. My guess: in depth analysis will probably reveal that *C. rainierensis* is contaxic with *C. speciosissimus*, and *C. orellanoides* is a later synonym of *C. rubellus*.

There are good and practical reasons to conserve *C. speciosissimus* but the proposal must present convincing facts first before the before mentioned 4 species actually represent the same taxon.

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 Frasné, 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 & Holmåsén (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 & Holmåsén) 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 had 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.

REDH 90428: As noted by Secretary Norvell, conservation of one name against others still allows for the usage of the other names if they subsequently are found to be different taxa. Hence, although there is debate about the number of taxa involved, when lumped together it may seem to be preferable to use the more commonly accepted name. I tend to weakly support the proposal now. If after a committee vote it takes only one shift to resolve the proposal I will consider changing my vote, but for now I will vote yes. All of the taxonomic issues do not need to be resolved to somewhat stabilize the usage of the names in the literature.

PENN 90527: I would like to hear more discussion to clarify the taxonomic uncertainties indicated by the Invited and Guest Commentators, and to resolve the omissions and inaccuracies in the Proposal.

DEMO 90705: Many people ask for a clarification of the typification of *C. orellanoides* I rather agree with Redh. 90428 but would prefer to see that settled first. If the authors of the proposal do not do it, could the secretary contact Bart Buyck in PC. Forwarding just my comments would be enough for him to see what material there is from Henry.

→ **RYMA 90731:** The description of *Cortinarius (Telamonia) rubellus* Cooke in Grevillea is not bad. The fungus was collected in "swampy places" in Orton Moss near Carlisle. Orton Moss is, according to Wikipedia 2009, //a former raised mire, traditionally used for peat cutting. Swampy places in coniferous forests are at least around Uppsala a very typical habitat for this species. Indeed also Favre collected his *Cortinarius speciosus* (= *C. speciosissimus*) in "11 hauts-marais, 27 explorations. Pinèdes et bordures d'épicéas, des parties

sphagneuses aux parties relativement desséchées, dans la vacciniaie, parmi les Sphagnum...."

In the protologue of *Cortinarius rubellus* there is a reference to Cooke Illus. t. 835. This picture is very good and the illustrated fungi are easy to recognize with the yellow bands on the stem and the red-brown cap, which is conical in the young fruit bodies and with an umbo on the older one. There are needles attached to the base of the stems, probably of *Pinus*, so there must have been coniferous trees near by. It is hard to understand why this illustration has not been recognized much earlier. But now, when the name *Cortinarius rubellus* is used more and more, I think we should continue with that, and not conserve the much younger name *Cortinarius speciosissimus*.

→ **PRIN 91223**: I vote that still more discussion is needed on this proposal. I agree with NORV 80104 that there are two issues at hand, a relatively confused taxonomy and the nomenclatural business. I think we must be careful not try and resolve a basically taxonomic question by taking nomenclatural steps. I am especially unhappy with the fact that the authors cite only two types and that there seem to be confusions around the typification of *C. orellanoides*. Since names are tied to types and not to taxonomic concepts or circumscriptions of species, I think it is vital to study type material (or clearly explain why this is impossible) before making nomenclatural proposals or decisions. Considering the numbers provided by Ursula Peintner I am also not convinced that retaining *C. rubellus* would be a disaster. The name seems to have been used frequently.

PROP. 1810, Proposal to conserve the name *Hemipholiota* against *Nemecomyces* (*Agaricales*, *Basidiomycota*). Proposed by Stig Jacobsson & Jan Holec. TAXON 57(2): 641–642. (2008).

[To appear on Ballot 2010-1; additional discussion needed]

Background & Guest comments on Prop. **1810**:

SUMMARY: *Pholiota destruens* (Brond.) Gillet (→ *Hemipholiota populnea* (Pers. : Fr.) Bon) and *Nemecomyces mongolicus* Pilát represent two names with conspecific types. *Hemipholiota* (Singer) Bon was proposed as a subgenus of *Pholiota* in 1951 (validated in 1962) and as genus in 1980 (validated in 1986). *Nemecomyces* was proposed in 1933 for a fungus collected in Mongolia that Pilát did not recognize as a mushroom already well known in Europe and North America. Noting that *Nemecomyces* was 'published by mistake,' Jacobsson & Holec propose to conserve the name *Pholiota destruens*, the basionym of the type species for *Hemipholiota*, a genus supported as distinct from *Pholiota* by DNA sequence analyses.

Committee comments on Prop. **1810**:

NORV 80923: As the name *Nemecomyces* appears restricted to type studies and *Hemipholiota* is a name less likely to confuse, I support this proposal.

REDH 90428: I support this proposal and if it had not been proposed by the authors, I would have proposed it in any event.

DEMO 90706: Lack of cystidia and some base changes offers little support for a genus, but insofar as people do it, we have to accept the change. I would have been happier if this had offered the opportunity to conserve *Agaricus destruens*. (This is very different from *Boletus applanatus* as the name here is not ambiguous.)

→ **PRIN 91223**: I support the proposal, although I would not necessarily use the age of an author as an argument here.

PROP. 1828, Proposal to conserve *Aspicilia aquatica* against *Lichen mazarinus* (Ascomycota: Pertusariales: Megasperaceae). Proposed by Anders Nordin & Per M. Jørgensen. TAXON 57(3): 989. (2008).

[To appear on Ballot 2010-1; additional discussion needed]

Background & Guest comments on Prop. **1828**:

SUMMARY: *Aspicilia aquatica* (Fr.) Körb. (1855) is the currently accepted name for a widely distributed crustose lichen. Du Rietz (1915) transferred the oldest name available for the species (*Lichen mazarinus* Wahlenb., 1812) to *Lecanora*, citing *A. aquatica* among the synonyms. Unfortunately Du Rietz also cited as synonyms names now associated with species accepted as distinct from *L. mazarina*. Conflicting opinions regarding the integrity of the species name and its application have led to a confused species concept. Nordin & Jørgensen propose to conserve *Aspicilia aquatica* ('the generally accepted and less often misapplied name') to prevent replacement by the 'obscure and taxonomically misused epithet *mazarina*.'

Committee comments on Prop. **1828**:

NORV 81115: I usually prefer to retain the earliest valid name for a taxon when its original author has clearly designated its type, regardless to confusion resulting from misunderstanding by later workers. Nonetheless, I tend to support this proposal, provided that the name *Aspicilia aquatica* is, in fact, the name more commonly accepted.

REDH 90428: I support this proposal.

DEMO 90706: OK.

→**TRIE 91030:** The name *Aspicilia aquatica* is commonly accepted, we have around 80 specimens under this name in the lichen herbarium of the Botanische Staatssammlung München (M). I support this proposal.

→**PRIN 91223:** I support the proposal. The case seems to be clear.

PROP. 1829–1830, Proposals to reject the names *Verrucaria thelostoma* (1829) and *Pyrenula umbonata* (1830) (lichenized Ascomycota). Proposed by Per M. Jørgensen. TAXON 57(3): 990–991. (2008).

[To appear on Ballot 2010-1]

Background & Guest comments on Props. **1829–1830**:

SUMMARY: The species now accepted as *Pyrenocarpon flotowianum* (Hepp.) Trevis. (1855, bas. *Verrucaria flotowianum* Hepp. 1853) was earlier referenced by two other species epithets, 'thelostoma' (*Verrucaria thelostoma* J. Harriman in Winch, 1807) and 'umbonata' (*Pyrenula umbonata* Ach., 1810) – the first based on a 'rather poorly developed' specimen that has 'puzzled lichenologists for centuries' and the second on a duplicate retained by Acharius in his herbarium, similarly undeveloped and misunderstood. Jørgensen notes that conservation of *Pyrenocarpon flotowianum*, a name that has remained constant since established as the type species, would preserve nomenclatural stability more than selection of two earlier names based on poor and difficult material. Rejection of the older names should reduce the impact caused by the taxonomic confusion resulting from the poor state of the original material. [**Secretary note:** Coppins & Aptroot located good type material in Newcastle of *V. thelostoma* after the above proposal was written. See Brian Coppins note below.]

Per. M. Jørgensen (Bergen, Norway) 80909: [information received in general letter] I have just received my copy of the most recent volume of THE LICHENOLOGIST where I to my surprise find that two more specimens of the original collection of *Verrucaria thelostoma* has been discovered in an obscure old herbarium in Newcastle. That is good news, particularly since they are better developed than the lectotype in

BM and the authors (Coppins & Aptroot) confirm that they are identical with *Pyrenocarpon flotowianum*, so that problem is solved, though the question of which epithet is best used remains for the committee to decide on. Coppins & Aptroot transfer the oldest epithet [*thelostoma*] to *Pyrenocarpon*. There were no clues concerning the Acharian role in the naming on these specimens either.

Brian Coppins (Edinburgh, UK) 90529: [*information received in response to spelling of flotowiana vs. flotoviana*] In the cases of flotov- and flotow- as orthographic variants (not errors), the original spelling should be followed.

Lecanora flotoviana Spreng., Neue Entd. 1: 221 (1820). Sprengel used the -v- form.

Verrucaria flotowiana Hepp (1853) (= *Pyrenocarpon flotowianum* = *P.*

thelostomum). Hepp used the -w- form.

On the subject of Prop. 1829–1830, I see no reason to reject *Verrucaria thelostoma* Ach ex J. Harriman, and if not rejected, there is no need to reject *Pyrenula umbonata* Ach. The combination, *Pyrenocarpon thelostomum* (Ach ex J. Harriman) Coppins & Aptroot was made in *Lichenologist* 40: 372 (2008). Per Magnus was under the impression that this name was based on a very poor type specimen in BM. This is not so. The BM specimen is a scrappy duplicate from Harriman's herbarium in HAMU [Newcastle], where there are two excellent specimens, which I have examined.

Committee comments on Prop. 1829–1830:

NORV 81115: Many thanks to Per Magnus for generously making available later information. He has made a persuasive case for rejecting two earlier names based on the poor type specimens and the confused concepts surrounding both names. However, because Coppins & Aptroot [*LICHENOLOGIST*, 2008, 40: 363–374] have now selected the oldest name after close examination of newly discovered material from the original type, which provided more information, I tend to reject the proposal. I encourage input from our lichenologists regarding whether the use of *Pyrenocarpon thelostoma* does threaten nomenclatural stability for the species.

REDH 90428: *Verrucaria thelostoma* was recently transferred to *Pyrenocarpon* in the following publication based upon isotypes in better condition: New species and combinations in *The Lichens of the British Isles*. Brian J. Coppins and André Aptroot. 2008. *THE LICHENOLOGIST* 40: 363–374.

I re-examined the conservation proposal in light of this new combination based on the discovery of presumed isotype materials in HAMU. Notably the specimens in HAMU mention “Whinstone,” which is not a place name but a description of a type of hard rock. Also, the datum is a better match to the protologue information than the “holotype.”

“V.C. 66, Durham: “On Whinstone near Eggleston, D[urham], [J.] H[arriman]” (HAMU—*isotypes*, two specimens).”

There being better information on the identity of the type for *Verrucaria thelostoma* (and by inference *Pyrenula umbonata*), I think it would be preferable to conserve *Verrucaria flotowiana* Hepp, *Flecht. Eur.*: no. 92 (1853) against *V. thelostoma* and *P. umbonata*, rather than outright rejection of the two names. This hedges the bet against future molecular work that might demonstrate that the two are not conspecific.

DEMO 90706: I confirm the comment by Coppins that flotov- and flotow- are orthographic variants for which the original spelling is to be retained.

→ **PRIN 91223:** Few specialists are working on these groups. In the light of Coppins' and Aptroot's paper, I do not see any convincing reason for rejection of these names and would rather follow Coppins & Aptroot.

PROP. 1831, Proposal to conserve the name *Mixia* against *Phytoceratiomyxa* (*Basidiomycota*). Proposed by Junta Sugiyama & Ken Katumoto. *TAXON* 57(3): 991–992. (2008).

[To appear on Ballot 2010-1; additional discussion needed]

Background & Guest comments on Prop. **1831**:

SUMMARY: *Mixia* C.L. Kramer (1959) was introduced to accommodate *Taphrina osmundae* Nishida (lectotypified by Sugiyama & Katumoto, 2008). Molecular analyses place the species in *Pucciniomycotina* (*Basidiomycota*), not *Taphrinales* (*Ascomycota*). The species has been associated with *Mixia* and exemplified as an enigmatic fungus in systematic and phylogenetic papers since first introduced. The type species of monotypic *Phytoceratiomyxa* Sawada (1929), *P. osmundae* — based on a single collection thought to be a myxomycete fern parasite and listed by Greuter & al. (1993) as a generic myxomycete name in current use — was treated as a ‘*nom dub?*’ in the DICTIONARY by Kirk & al. (2001). Discovery that both names represent the same basidiomycete species gives the earlier named genus, *Phytoceratiomyxa*, nomenclatural priority. Conservation of the better known *Mixia* would prevent the disruption that would be caused by adoption of the lesser used name, *Phytoceratiomyxa*.

Committee comments on Prop. **1831**:

NORV 81115: I support this proposal.

REDH 90428: I support this proposal. Its importance is self-evident.

DEMO 90706: OK.

→ **PRIN 91223**: I support the proposal.

PROP. 1852, Proposal to conserve the name *Olivea tectonae* (T.S. Ramakr. & K. Ramakr.) R.L. Mulder against *Olivea tectonae* (Racib.) Thirum. (*Basidiomycota*). Proposed by Andrew M. Minnis, Hye Young Yun & Amy Y. Rossman. TAXON 57(4): 1355–1356. (2008).

[To appear on Ballot 2010-1; additional discussion needed]

Background & Guest comments on Prop. 1852:

SUMMARY: *Uredo tectonae* Racib. (1900), described for the uredinial state (anamorph) of the teak fungus without designation of type specimen and specific type locality, is lectotypified from exsiccatae of original material received by BPI in 1899. Its telial state was described ‘nearly concurrently’ in Ramakrishnan & Ramakrishnan (1949) and Thirumalachar (1949). *Chaconia tectonae* T.S. Ramakr. & K. Ramakr. (1949), a validly published species name for the teleomorph based on a missing holotype, is lectotypified by a K collection sent in 1949 by the Ramakrishnans and which matches ‘in all regards’ the type description.

Olivea tectonae (Racib.) Thirum. (1949) was introduced as a teleomorph name but is derived from the anamorphic type of *Uredo tectonae* (Art. 59.3) and so cannot stand as a teleomorphic new name (Art. 59.6) because it lacks the necessary Latin diagnosis (Art. 36.1). Mulder proposed *O. tectonae* (T.S. Ramakr. & K. Ramakr.) R.L. Mulder (1973) based on *C. tectonae* for the teleomorph because he believed the Thirumalachar name not to be validly published. The Mulder name, however, represents an illegitimate later homonym (Art. 53.1) of Thirumalachar’s name for the teleomorph.

The authors stipulate that the fungus should be classified in *Olivea* but point out that as ‘*O. tectonae*’ is not available for the teleomorph, with no other epithets available, a new name is required for the teleomorph. They further note that a replacement name based on a teleomorphic type would disrupt current usage and that currently ‘*O. tectonae*’ is used indiscriminately as the correct name for the teak rust fungus with each authorship cited equally, indicative of the currently confused state. They note finally that *O. tectonae* (Racib.) Thirum is not the correct name for the anamorphic state, which belongs to the uredinial form genus *Uredo* rather than the telial genus *Olivea*. In keeping with Art. 14, the telial based *O. tectonae* (T.S. Ramakr. &

K. Ramakr.) R.L. Mulder is proposed for conservation against *O. tectonae* (Racib.) Thirum.

- >SECRETARIAL NOTE: Last year Dr. Carvalho and coauthors submitted a counter-proposal to retain the name *Olivea neotectonae*, which had been proposed in a publication overlooked by the authors of Prop. 1852. Following discussions with TAXON Nomenclatural Editor John McNeill and Assistant Nomenclatural Editor on *Fungi* Redhead, the authors agreed that the additional information and request to reject Prop. 1852 was best handled within the CF rather than publishing a counter proposal. The Carvalho & al. draft proposal is presented below to provide needed information; see also comments by Scott Redhead [REDH 91104] for important additional information. Refer also to the PDF of the missing 2007 publication by Buriticá & Salazar.]
- >**Anibal Alves de Carvalho, Junior (Brazil)** with Cassia M. Sakuragui & Joe F. Hennen: *Withdrawn*—Proposal to reject the name *Olivea tectonae* (T.S. Ramakr. & K. Ramakr.) R.L. Mulder and to accept *Olivea neotectonae* (T.S. Ramakr. & K. Ramakr.) Buriticá & Salazar.

“Minnis *et al.* (Taxon 57: 1355-1356. 2008) proposed to conserve the name *Olivea tectonae* (T.S. Ramakr. & K. Ramakr.) Mulder against *Olivea tectonae* (Racib.) Thirum. but they overlooked the publication of *Olivea neotectonae* (T.S. Ramakr. & K. Ramakr.) Buriticá & Salazar (Revista Fac. Nac. Agron. Medellín 60 (1): 3652. 2007) as a *nomen novum*.

Anamorph names cannot be synonyms of teleomorph names unless their description and type specimen reveal that they are teleomorphs. Minnis *et al.* (l.c.) mistakenly reported that *Uredo tectonae* Racib. (Paras. Alg. Pilz. Java's 1: 28. 1900) was the first name applied to the etiologic agent of teak rust, when in fact the first name published for this rust was *Aecidium effusum* Niessl (Hedwigia 20: 150. 1881). The only collection reported by Niessl, and thus the Type, was a collection from the Botanical Gardens of Calcutta (India) on *Tectonia grandis* collected by Dr. S. Kurz, s.n, s.d. Later, without reference to *Aecidium effusum*, Raciborski (l.c.) named the same rust species as *Uredo tectonae* Racib. from Java (Indonesia). More information about the Type of *U. tectonae* can be found in Minnis *et al.* (l.c.). Both names apply to the anamorphic uredinial stage of the teak rust. Sydow & Sydow (Monogr. Ured. 4: 422. 1924) mistakenly placed *Aecidium effusum* (published in 1881) as a synonym of *Uredo tectonae* (published in 1900). Because *Aecidium effusum* Niessl has priority, it is the valid anamorphic name (ICBN Principle III) even though it was placed in the wrong morphological genus. Numerous peripheral incurved paraphyses surmount a basal united plexus of fungal hyphae in *Aecidium effusum*, thus the most appropriated modern anamorph genus is *Malupa*.

Thirumalachar (Curr. Sci. 18: 176. 1949) and Ramakrishnan & Ramakrishnan (Indian Phytopathol. 2:19. 1949) described the teleomorph of teak rust independently and in the same year. Thirumalachar (l.c.) proposed the new combination *Olivea tectonae* (Racib.) Thirum. Minnis *et al.* (l.c.) reported that this validly published new combination "is an anamorphic name because it is based upon the anamorphic type of *Uredo tectonae* (Art. 9.3 of the ICBN, McNeill & al. *In Regnum Veg.* 146. 2006)". But, what invalidated it as a teleomorph name is the lack of a Latin diagnosis (ICBN Art. 36.1). Ramakrishnan & Ramakrishnan (l.c.) considering that presence or absence of paraphyses in the sori alone could not be used as a trait to differentiate genera, named this rust *Chaconia tectonae* as a new species. Their publication fulfilled

all requirements for a valid publication. However, Mulder & Gibson (CMI Descr. Pathog. Fungi Bact. 365:1-2. 1973) recognized the validity of the genus *Olivea* and made the new combination *Olivea tectonae* (T.S. Ramakr. & K. Ramakr.) Mulder based on the type specimen of *C. tectonae*. But since Thirumalachar had already published this name it could only apply to an anamorph even though it was placed in a teleomorph genus. Mulder's use of the epithet made the name a later homonym.

By 2005 the teak rust had been found in the Americas and was reported by several authors who used Mulder's name. Buriticá & Salazar (l.c.) understanding that Mulder's name was a later homonym (ICBN Art 45.3) proposed a new name, *Olivea neotectonea* with the basionym *C. tectonae*. These later authors gave the authorship of the new name to T.S. Ramakrishnan & K. Ramakrishnan and indicated the basionym as required by the ICBN Art. 33-4, 46-2 for new names. Therefore, the name that must be validated as the teleomorph causal agent of rust of *Tectona* is *Olivea neotectonae* (Ramakr. & Ramakr.) Buriticá & Salazar.

Committee comments on Prop. **1852**:

NORV 90323: The authors demonstrate that a legitimate name is needed for the teleomorph and present convincing logic for conservation of the telial-based name to remove the current nomenclatural confusion. I support this proposal.

REDH 90428: I support this proposal.

—>**REDH 91104** (responding to Dr. Carvalho prior to withdrawal of the Carvalho & al. proposal): Thank you for sending the PDF copy of the 2007 publication. Clearly Drew Minnis overlooked this publication. The citation of authorships in the 2007 publication [Buriticá & Salazar (Revista Fac. Nac. Agron. Medellín 60 (1): 3652. 2007)] is incorrect, however. The citation of authorships for the names was correctly presented in the Minnis proposal.

Olivea tectonae (T.S. Ramakr. & K. Ramakr.) R.L. Mulder in Mulder and

Gibson, C.M.I. Descr. Pathog. Fungi Bact. 365: 1. 1973

(*Chaonia tectonae* T.S. Ramakr. & K. Ramakr. in Indian Phytopathol. 2: 19. 1949)

Typus: India, [Tamil Nadu], "Walayar (Malabar, Madras Prov.)", "on living leaves of *Tectona grandis* L. October 13, 1948", T.S. & K. Ramakrishnan (Mycol. Sect., Agric. Res. Inst., Coimbatore).

Olivea tectonae (Racib.) Thirum. in Curr. Sci. 18: 176. 1949

(*Uredo tectonae* Racib., Paras. Alg. Pilz. Javas 1: 28. 1900) Lectotypus: Indonesia:

Java, Bogor, in foliis *Tectona grandis*, ? & M. Raciborski. Cryptogamae Parasiticae in Insula Java Lectae Exsiccatae. No. 38 (BPI No. 155900).

Therefore, the correction by Index Fungorum in authorship for the name *Olivea neotectonae* is also correct.

Instead of reading (as it did in the 2007 publication)

Olivea neotectonae (T.S. Ramakrishnan & K. Ramakrishnan) Buriticá y Salazar, nom. nov.

= *Olivea tectonae* Thirumalachar, Current Science 18: 178. 1949. nom. nudum

= *Chaonia tectonae* T.S. Ramakrishnan & K. Ramakrishnan, Indian Phytopath. 2: 19. 1949.

= *Olivea tectonae* (T.S. Ramakrishnan & K. Ramakrishnan) Mulder, in Mulder & Gibson C.M.I. Descript. Fungi Bact. no. 365. 1973. nom. nudum.

Anamorpho. *Uredo tectonae* Raciborski, Paras Algen Pilze Javas 1: 28. 1900.

It should have read:

Olivea neotectonae Buriticá & Salazar, nom. nov.

= *Chaonia tectonae* T.S. Ramakrishnan & K. Ramakrishnan, Indian Phytopath. 2: 19. 1949.

= *Olivea tectonae* (T.S. Ramakrishnan & K. Ramakrishnan) Mulder, in Mulder & Gibson C.M.I. Descript. Fungi Bact. no. 365. 1973.

non

= *Olivea tectonae* (Rac.) Thirumalachar, Current Science 18: 178. 1949.

= *Uredo tectonae* Raciborski, Paras Algen Pilze Javas 1: 28. 1900.

Therefore the available name for the teleomorph is *Olivea neotectonae* Buriticá & Salazar.

The Minnis proposal, to be successful, would need to include this name for rejection.

→ **PRIN 91223**: Supported.

—> **REDH 100714**: It is unfortunate that the earlier name *Olivea neotectonae* was overlooked by all of us during the editing of Prop. 1852, and it adds a complication to the original request. I am of two minds on the situation and would value input by others on the committee as to be best solution.

PROP. 1861, Proposal to conserve the name *Aspicilia farinosa* (Ascomycota: Pertusariales: Megasporaceae) with a conserved type. Proposed by Anders Nordin & Claude Roux. TAXON 58(1): 292. (2009).

[To appear on Ballot 2010-1]

Background & Guest comments on Prop. **1861**:

SUMMARY: *Aspicilia farinosa* (Flörke) Hue (1910; = *Urceolaria calcarea* var. *farinosa* Flörke, 1810) represents a crustose lichen species with 8-spored asci and small spores closely related to *A. calcarea* with 4-spored asci and larger spores. In the absence of Flörke's original material, Arnold (who listed *farinosa* at the subspecific level in *Aspicilia*) and Hue based their species concepts on the small-spored taxon, a concept followed by Nylander (1872) before them and accepted by lichenologists since. Sipman's 2007 discovery of original Flörke material with large spores in B labeled *Urceolaria calcarea* var. *farinosa* challenges the small-spored concept for *A. farinosa*. The poor condition of the rediscovered material, however, prevents establishing whether the material represents *A. calcarea* or a distinct species. The authors propose conserving the name with the Nylander collection so as to continue the current concept for the species as originally presented by Hue and Arnold.

→ **Anders Nordin & Claude Roux** (proposal co-authors) **91016**: We have **(B. Abbott in litt.)** discovered that *Aspicilia farinosa* ... was combined into *Aspicilia* at an earlier date than assumed (by us, Zahlbruckner, Hue and in INDEX FUNGORUM), namely in Flagey 1888: 131 (Flagey, C. 1888: Herborisation lichénologique dans les environs de Constantine (Algérie). REVUE MYCOLOGIQUE 10: 126-134.) This does not affect the conservation proposal other than that the author citation ought to be changed to *Aspicilia farinosa* (Flörke) Flagey instead of *Aspicilia farinosa* (Flörke) Hue. We hope this will not affect the proposal negatively.

→ **Harrie Sipman** (Berlin, Germany; former CF member) **91102**: Since the taxon [*Aspicilia farinosa*] is based on material from a locality near Berlin, which I know by own fieldwork, and since I discovered an original specimen in a returned loan that survived the herbarium fire in 1943, I have paid attention to it and have come to the conclusion that probably three different taxa have been confused under this name.

1. a pruinose variety of *Aspicilia calcarea*, distinguished by various authors in the 19th century among specimens from Central Europe, which has remained unnoticed in recent treatments.

2. a common and widespread mediterranean taxon with continuous thallus, related to *A. calcarea* by its spore characters.
3. a widespread but less common mediterranean taxon unrelated to *A. calcarea*, with octosporous asci and an algal stratum below the hymenium. This taxon seems to have its northern limit near Paris, from where an exsiccate is distributed.

The type locality where Floerke collected the original specimen makes very probable that he had the first taxon at hand, and not the other two. The surviving specimen is in poor condition but would fit the first taxon. It would not fit the second taxon due to its thick, areolate thallus. The absence of properly developed apothecia makes it difficult to say if it could belong to the third taxon.

To clear the situation I have proposed a new name for taxon 2, *Aspicilia substerilis* (Sipman 2007).

The conservation proposal seeks to conserve the epithet *farinosa* for taxon 3, in order to protect current use. However, I am in favour of maintaining the name in the original sense (taxon 1), because connecting it to taxon three would mean that the report of the species near Berlin becomes that of a mediterranean species; this species has apparently never been found again in Germany and would have to go on the Red List as extinct. This is evidently not appropriate. Also the significance of taxon 1 might need restudy.

Committee comments on Prop. **1861**:

NORV 90412: I support conservation of the name *Aspicilia farinosa* with Nylander's collection Herb. Lich. Paris No. 127 as type. Selection of material on which Hue based his transfer of *Urceolaria calcarea* var. *farinosa* to *Aspicilia* at species rank where it is accepted today will ensure continued nomenclatural stability.

REDH 90428: I support this proposal.

DEMO 90706: My impression is that an epitype would be more adequate. Could our lichenologists comment on that?

→**DEMO 91017** (*responding to new information presented by authors above*): I have annotated my copy of TAXON and will take this into account if I have to edit the entry if it is conserved. I however take up this opportunity to give you my opinion that an epitypification would have satisfied me more than a type conservation.

→**NORV 91017** (*responding to author message*): Your information comes at an excellent time, [as I shall be able to] add your comments to the Commentary [before the proposal is placed on the ballot, allowing] Committee members to make note of the change and vote accordingly. You are correct that this will not affect the conservation proposal or our recommendations to the General Committee. Additionally, ... a short explanation [will be added] to the October-December volume of MYCOTAXON [so as to] alert the wider mycological world to the change. We greatly appreciate your notifying us so promptly.

→**TRIE 91030**: In discussing the matter of Proposal (1861) with Harrie Sipman (B), he informed me about the problem that in 1810 Flörke described *Urceolaria calcarea* var. *farinosa* Flörke from the surroundings of Berlin where no mediterranean lichens occur. In 1910 Hue recombined the name to *Aspicilia farinosa* but applying the name to the mediterranean taxon. The material found in B clearly represents the holotype of the non-mediterranean taxon *Aspicilia farinosa* sensu Hue by its habit and ecology. Otherwise the mediterranean taxon would have been a new record for Germany.

Unfortunately the holotype (B 600004445) is in a bad condition and no apothecial characters (including spores and asci) could support the identification. No further original material exists, also not in the lichen herbarium at M. According to Nordin & Roux the material distributed by Nylander, Herb. Lich. Paris No. 127, and published sub *Lecanora cinerea* var. *calvarea* (*farinosa* Ach.) clearly represents the mediterranean taxon *Aspicilia farinosa* sensu Hue. The latter material could therefore not be selected as epitype, as far as I see, and should be also not be selected as type cons. Starting with around 1855 certain authors might have switched the use of the name and at least three taxa might be involved (according to H. Sipman, in phon.). So, perhaps it might be the best to look for a possibility to conserve at least the use of the epithet in its "mediterranean" sense. In this context it might be useful to check several publications cited by Zahlbruckner <http://194.203.77.76/libri-fungorum/Image.asp?ItemID=139&ImageFileName=0307b.jpg>, e. g. the publication Massalongo, A.B.1852 Ricerche Lich. (Is this a valid original description, as two possible synonyms are cited, but with question marks?) and Massalongo, A B 1856: Lichenes italici exsiccati VIII, Nr. 267 *Pachyspora farinosa* A. Massal., p. 148, 1856 (the first recombination at species level) and to study the material distributed there under *Pachyspora farinosa* A. Massal.

Summarizing, I see the need for further studies and would propose to reject the proposal.

→ **PRIN 91223:** Although the type of Flörke cannot reliably be assigned to any species, it is clearly in conflict with the current use of the epithet *farinosa*. Because an epitype is meant to "support" a damaged or otherwise uninterpretable type, I would vote for conservation with a conserved type. Otherwise one would have to choose an epitype that is in conflict with the type material.

PROP. 1862, Proposal to conserve the name *Psoroma versicolor* (*Degeliella versicolor*) against *Psoroma subdescendens* (lichenized *Ascomycota*, *Pannariaceae*). Proposed by Alan M. Fryday & Brian J. Coppins. TAXON 58(1): 293. (2009).

[To appear on Ballot 2010-1]

Background & Guest comments on Prop. **1862:**

SUMMARY: In combining *Degeliella versicolor* (Müll. Arg) P.M. Jørg. (2004), Jørgensen originally cited as basionym *Lecanora versicolor* Hook. f. & Taylor (1844), which Lamb (1963) had previously declared an illegitimate later homonym of *Lecanora versicolor* (Pers.) Ach. (1810). As Müller of Argau (1888) had combined Hooker & Taylor's name into *Psoroma*, under Art. 58.1, it was available as *Psoroma versicolor* Müll. Arg. (1888). Unfortunately, Müller's name was published in December, while *Psoroma subadscendens* Nyl. (1888) had been published earlier in the year for a South American collection now thought to represent the same taxon. Nonetheless, all recent floras have used either [after 2004] *Degeliella versicolor* or [pre-2004] *Psoromidium versicolor* (Müll. Arg.) D.J. Galloway (1983), with *Psoroma subadscendens* last used in 1953. Conservation of the name *Psoroma versicolor* will leave *Psoroma adscendens* available to represent the South American collection, should future research determine it to represent a separate species.

Committee comments on Prop. **1862**:

NORV 90412: I support the conservation of *Psoroma versicolor* to preserve nomenclatural stability, thereby leaving *Psoroma subdescendens* available to serve for the name of a possibly distinct taxon.

REDH 90428: I support this proposal.

DEMO 90706: OK.

→ **PRIN 91223**: Supported.

PROP. 1863, Proposal to conserve the name *Craterellus cinereus* (Pers. : Fr.) Donk with a conserved type against *Craterellus cinereus* Pers. (*Basidiomycota*). Proposed by Ibai Olariaga, Isabel Salcedo & Luis A. Parra. TAXON 58(1): 294–295 (2009).

[To appear on Ballot 2010-1]

Background & Guest comments on Prop. **1863**:

SUMMARY: *Craterellus cinereus* (Pers. : Fr.) Donk (1933, = *Cantharellus cinereus* Pers., 1794 : Fr. 1821) is now accepted in *Craterellus*, where it is supported by molecular analyses, stipe morphology, and possession of aliphatic carotenes. Donk's citation of *Craterellus cinereus* (Fr.) Quél. (Maire, l.c.: 89) was misleading, as both Quélet (1888) and Maire (1902) cite *Cantharellus* [not *Craterellus*] *cinereus* Pers. Donk seems to have attributed the *Craterellus* combination to Quélet by mistaking the generic abbreviation "C." [intended by Quélet for *Cantharellus*] as *Craterellus* due to the name's proximity to Quélet's *Craterellus* section. Donk did refer indirectly to the basionym (i.e., 'Fr.'), so that *Craterellus cinereus* (Pers. : Fr.) Donk is considered as validly published (Art. 33.2) but with a corrected authorship. That name, however, is an illegitimate later homonym (Art. 53.1) of *Craterellus cinereus* Pers. and has no special protection against the earlier (even if sanctioned) homonym. The earliest available species epithet in *Craterellus* could be based upon the synonym *Helvella hydrolips* Bull. (1790), except that the combination '*Craterellus hydrolips*' has never been made.

The two Persoon '*cinereus*' names above refer to two taxa with different types: *Craterellus cinereus* Pers. (1825) is based on a fungus described by Micheli that Fries (1832) regarded as a synonym of *C. cornucopioides* and which Micheli's illustration strongly resembles. *Merulius cinereus* (Pers. : Fr.) Pers. (1825) is based on *Cantharellus cinereus*.

Absent a correct name for *Craterellus cinereus* (Pers.: Fr.) Donk [*Helvella hydrolips* not yet combined under *Craterellus*] and considering the wide use of *Craterellus cinereus* (Pers. : Fr.) Donk for a well known species, Olariaga & al. propose to conserve *Craterellus* (Pers. : Fr.) Donk against the earlier homonym *Craterellus cinereus* Pers., a name last used more than 170 years ago by Fries as a synonym for the now well-established *C. cornucopioides*. The authors designate as type a specimen from Lundell & Nannfeldt's FUNGI EXSICCATI SUECICI.

Committee comments on Prop. 1863:

NORV 90422: I support this well-researched and logical proposal.

REDH 90428: I support this proposal. Investigation during editing confirms the confusion in the literature.

DEMO 90706: OK.

→ **PENN 91214**: (048–051) I support the Proposal in general. However, I would like further discussion of two aspects of the Proposal:

- (1) The typ. cons. prop. for *Cantharellus cinereus* Pers. : Fr. Contu et al. (Mycotaxon 110: 139–149. 2009) have designated a neotype from amongst the Persoon collections of *Cantharellus cinereus* at L. As an

alternative to the current proposal of the Lundell & Nannfeldt exsiccatum as a conserved type, would it be preferable to propose the exsiccatum as an epitype?

(2) The current text on p. 294, column 2:

Although Donk erroneously attributed the combination in *Craterellus* to Quélet, probably due to an oversight caused by the abbreviated generic name as “C.” and proximity of descriptions of species under both *Craterellus* and *Cantharellus* in Quélet’s 1888 publication, we accept the validity of the binomial *Craterellus cinereus* (Pers. : Fr.) Donk,

should be shortened to:

Although Donk erroneously attributed the combination in *Craterellus* to Quélet 1888, we accept the validity of the binomial *Craterellus cinereus* (Pers. : Fr.) Donk,

The deleted text, speculating on a reason for Donk’s error, is irrelevant and patently incorrect. A glance at Quélet (1888, p. 38) demonstrates that there is no abbreviated generic name; the epithet stands alone, with the name *Cantherellus* as a highly conspicuous bold genus heading earlier on the same page and as a conspicuous running heading on the facing page. The generic name *Craterellus* appears only once on these two pages, in an inconspicuous comment on p. 39: “Resemble à *Craterellus cornucopioides*.”

→ **PRIN 91223**: Supported.

PROP. 1888, Proposal to conserve the name *Glomus* (*Fungi*, *Glomeromycota*, *Glomerales*) as being of neuter gender. Proposed by Thomas W. Kuyper. *TAXON* 58(2): 647 (2009).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1888**:

SUMMARY: With 70 species, *Glomus* Tul. & C. Tul. (1845) comprises the largest genus in the phylum *Glomeromycota*, a monophyletic group of arbuscular mycorrhizal fungi thought to represent the sister group of *Ascomycota* + *Basidiomycota*. Art. 62 lists three criteria (“in descending order of importance”) — botanical tradition, the author’s original usage, and classical usage — that govern gender conventions. Unable to find examples of botanical usage (which he equates with ‘pre-Linnaean’), Kuyper notes that the masculine gender assigned by the Tulasnes should prevail over the neuter classical Latin. Although the masculine gender was used for *Glomus* from 1844–1982 in accordance with the Tulasnes’ original usage, since 1982 mycological usage has consistently treated *Glomus* as a neuter noun, in accordance with classical tradition following Trappe’s 1982 formal correction of all adjectival epithets to neuter. Kuyper further notes that the introduction of the neuter stem ‘Glomer-’ by Schüssler & al. in 2001 has also been adopted (over the previously used Glom-), that ‘changing gender (again)’ seems to serve no useful purpose, and that the neuter gender is best conserved in accordance with Art. 14.11.

Committee comments on Prop. **1888**:

DEMO 90706: I think the proposal is superfluous, for Kuyper has misunderstood what was meant by botanical tradition. I still need to check the archives of the editorial committee and consult with the General Rapporteur (John McNeill) but the best place for this would be as an addition to Ex. 1.

NORV 90706: My original thought was to support this proposal. However, if Vincent is correct and information relating to botanical tradition can be invoked, it would be better to add this as an Example.

→**TRIE 91030**: I agree, that the proposal might be superfluous.

→**PRIN 91223**: Re DEMO 90706: I am insecure what Art. 62 means by “botanical tradition”. Ex. 1 states that *Eucalyptus* (described in 1788) lacks a botanical tradition, which in my eyes must mean that it has no pre-Linnean (or pre-something else) tradition. That would mean that Kuyper has not misunderstood the CODE and that the proposal makes sense, wouldn't it?

NEW: PROP. 1896, Proposal to conserve the name *Lichen lichenoides* (*Leptogium lichenoides*) against *Lichen tremelloides* and *L. tremella* (lichenized *Ascomycota*). Proposed by Per M. Jørgensen. TAXON 58(3): 1002–1003 (2009).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1896**:

SUMMARY: Because the name *Tremella* L. (as typified by Donk, 1958) applies to a genus of heterocystous *Nostocaceae* with a starting point of 1892, the name *T. lichenoides* L. 1873 (basionym for *Leptogium lichenoides* (Wulfen) Zahlbr. 1924) is not validly published in accordance with Art. 13.1. Two names that might be available for the taxon – *Lichen tremelloides* Weiss (1770) and *Lichen tremella* Roth (1788, using the Linnean generic epithet ‘*Tremella*’) – are illegitimate and are no longer backed up by types due to the loss of the Weiss and Roth herbaria. The later name *Lichen lichenoides* Wulfen 1791 would have been validly published except that Wulfen cited his own collection of *Lichen tremelloides* Weiss as a synonym; under Art. 7.5 this makes *L. lichenoides* a type of *L. tremelloides*, thus rendering Wulfen's name also illegitimate.

Although Jørgensen lectotypifies and epitypifies *Lichen tremelloides* by selecting a Dillenius illustration cited by Weiss and neotypifies *L. tremella*, he proposes conserving Wulfen's name with a good Linnean specimen as type (thus preserving both Wulfen's and Roth's intention) as the best alternative, noting that then the species will retain its familiar name (albeit with a slight change in author citation) and that no further nomenclatural actions will be needed.

Committee comments on Prop. **1896**:

NORV 91005: Jørgensen's arguments are persuasive, and I favor this proposal.

→**PRIN 91223**: Supported.

NEW: PROP. 1897, Proposal to reject the name *Lecidea epiploica* (lichenized *Ascomycota*). Proposed by Per M. Jørgensen & Anders Nordin. TAXON 58(3): 1003–1004 (2009).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1897**:

SUMMARY: *Lecidea epiploica* Norman 1867 is a little-used name based on an unusual lichen specimen now accepted under a younger name, *Calvitimela perlata* (Haugan & Timdal) R. Sant. (2004). The taxon in question was first named *Lecidea* (= *Lecidella*) *bullata* (Körber) Th. Fr. (1874), an illegitimate later homonym of *Lecidea bullata* Meyen & Flotow (1843); to remedy this situation, Zahlbruckner proposed the new name *Lecidea bullosa* A. Zahlbr. 1925. However, Magnusson, who later discovered that Fries had misinterpreted Körber's original concept, proposed the name *Lecidea perlata* H. Magn. (1931) for Fries's material, a name that represents an illegitimate later homonym of *Lecidea perlata* Hue (1915). Magnusson's epithet was adopted when reclassified as *Tephromela perlata* Haugan & Timdal. (1994), a legitimate name that has since has been transferred to *Calvitimela*.

Rather than introduce an additional new species epithet, Jørgensen and Nordin propose that the ‘already forgotten name’ *Lecidea epiploica* be rejected to maintain nomenclatural stability.

Committee comments on Prop. **1897**:

NORV 91005: Although preferring proposals written to conserve rather than to reject names, I tend to favor this proposal.

→**TRIE 91030**: I support the proposal

→**PRIN 91223**: Supported.

NEW: PROP. 1898, Proposal to conserve *Stirtonia* A.L. Sm, (lichenized *Ascomycota*, *Arthoniales*) against *Stirtonia* R. Gr. bis (*Bryophyta*, *Dicranales*). Proposed by Andreas Frisch & Göran Thor. TAXON 58(3): 1004 (2009).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1898**:

SUMMARY: The name *Stirtonia* has been applied to two different genera: the first in 1920 to a monotypic moss genus *Stirtonia* (type sp.: *S. mackayi* R. Br. bis) and the second in 1926 to a small group of tropical lichens *Stirtonia* A.L. Sm. (type: *S. obvallata* (Stirt.) A.L. Sm.). Since the type species, *S. mackayi*, was transferred in 1901 to the moss genus *Trematodon* by Brotherus, *T. mackayi* “has been in continuous use” with *S. mackayi* cited only in the original publication. The lichen genus *Stirtonia*, which now accommodates 14 “apparently rare and only seldom collected” species, is nonetheless “well established among lichenologists working in tropical countries and ... in continuous scientific use to the present day.” A third name, *Stirtonia* Van Wyk & Schutte (1994) is an illegitimate third homonym for a vascular plant genus already replaced by *Stirtonanthus* Van Wyk & Schutte.

As *Stirtonia* R. Br. bis was not used except in the original description and as there is “no older name available for *Stirtonia* A.L. Sm.,” the authors propose that the lichen genus *Stirtonia* be conserved to ensure taxonomic stability: a new generic name and up to 14 new combinations would be needed should the proposal be rejected. Frisch and Thor also note, however, that no name is available for the moss species, *S. mackayi*, should it be shown to be distinct at the generic level from the type of *Trematodon*.

Committee comments on Prop. **1898**:

NORV 91005: As both *T. ambiguus* (the type species) and *T. mackayi* are noted as accepted names in the current literature, I tend to support this proposal. I nonetheless welcome additional assurance from bryologists that *Stirtonia mackayi* is likely to remain closely associated with the type of *Trematodon*.

→**TRIE 91030**: It might be precipitate to conserve *Stirtonia* A.L. Sm. because the taxonomy of *Stirtonia* and allied genera is still not settled at the moment, but nevertheless I will support it.

→**PRIN 91223**: I agree with NORV 91005 that Bryologists should be asked for their opinion first.

NEW: PROP. 1899, Proposal to conserve the name *Hebeloma cylindrosporum* against *Hebeloma angustispermum* (*Basidiomycota*). Proposed by Jan Vesterholt, Hervé Gyta, Roland Marmeisse, Henry Beker, Ursula Eberhardt, Edmondo Grilli & Herbert Boyle. TAXON 58(3): 1005 (2009).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1899**:

SUMMARY: This well-written proposal clearly supports conserving the later name *Hebeloma cylindrosporum* Romagn. 1965 over *H. angustispermum* A. Pearson 1951. Under the name *H. cylindrosporum*, the common European pine agaric is cited in 585 publications from 30 countries and one of the six most intensively studied ectomycorrhizal fungi due to its culturability whereas *H. angustispermum* was little known until Grilli (2006) first noted its synonymy with *H. cylindrosporum*, a synonymy since supported by ITS sequence analyses of the holotypes. Vesterholt & al. dismiss the challenge of an earlier name, , by noting that Grilli's 1987 proposed synonymy of *H. spoliatum* (Fr.) Gillet 1876 is not generally accepted, the basionym (*Agaricus spoliatus* Fr. 1838) has not been typified, and that *A. spoliatus* was 'described from mountainous coniferous forests' and *H. cylindrosporum* from lowland sandy pine forests.

Committee comments on Prop. **1899**:

NORV 91005: I support this proposal. [An entirely unscientific and hasty check of Google shows 157 initial 'hits' for *H. angustispermum* that collapsed to 31 at the end of the search, most of which pertained directly to the proposal above. For *H. cylindrosporum*, however, there were 28,6000 hits that eventually collapsed to 414.]

→ **PRIN 91223:** Supported.

NEW: PROP. 1918, Proposal to conserve the name *Dermatocarpon bucekii* (*Placopyrenium bucekii*) against *Placidium steineri* (lichenized *Ascomycota*, *Verrucariaceae*). Proposed by Ayhan Senkardesler. *TAXON* 59(1): 294 (2010).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1918**:

SUMMARY: *Placidium steineri* was included as a new species in a list of lichens from Turkey published in a larger publication by Wettstein. The name has been variously attributed to J. Steiner, who determined the taxon as new, and Wettstein, who named the species to honor Steiner. Although transferred to *Dermatocarpon* by Zahlbruckner, the name appears not to have been otherwise accepted after its publication in 1889. Examination of the holotype shows that it is synonymous with *Placopyrenium* (now *Dermatocarpon*) *bucekii*, which is well established in numerous floras all over the distribution range of the species" throughout the Mediterranean, southeast Asia, and the Ukraine." The author proposes to conserve "an uncontested species name in current use" over a forgotten and relatively unknown one.

Committee comments on Prop. **1918**:

→ **NORV 91215:** In the absence of opposing argument, I tend to favor this proposal.

NEW: PROP. 1919, Proposal to conserve *Lactarius* (*Basidiomycota*) with a conserved type. Proposed by Bart Buyck, Valérie Hofstetter, Annemieke Verbeken & Rubeyn Walleynt. *TAXON* 59(1): 295–296 (2010).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1919**:

SUMMARY: Due to molecular analyses that support four major, phylogenetically distinct clades for species now accepted in *Lactarius* and *Russula*, many basidiomycetologists (including the authors) would prefer to place the clades into four distinct genera. Transfer of one clade composed of *Russula* sect. *Compactae* subsect. *Ochricompactae* and *L. furcatus* to *Multifurca* leaves the remaining *Russula* species "firmly within a

monophyletic ‘Russula’ clade.” [Refer to Buyck & al. 2008 (Mailing 12) for information on *Multifurca*.]

The two other well-supported clades belong to a paraphyletic *Lactarius*, a large mushroom genus comprising over 400 species. The first clade includes species commonly placed in the subgenera *Piperites* (e.g., *L. torminosus*, *L. deliciosus*), *Plinthogali* (including *L. lignyotus*), *Russularia*, *Colorati*, *Tristes*, *Rhysocybella*, and all sequestrates that were until recently classified in *Arcangeliella*, *Zelleromyces*, and *Gastrolactarius*. The second clade contains species now classified in the subgenera *Lactarius*, *Lactifluus*, *Lactariopsis* and *Russulopsis* and other species not assigned at the subgeneric level in sections *Edules*, and *Panuoidei* and series *Gerardii*. Falling within this second clade (which contains 20–25% of the currently described *Lactarius* species) is the currently listed lectotype of *Lactarius*, *L. piperatus*, a name lectotypified by Earle in 1909 and first cited as type in the 1988 CODE when the orthography of the genus was conserved and accepted as such by most modern authors.

Two other names previously cited as lectotype of the genus—*L. torminosus* (first listed by Singer in 1936) and *L. lactarius* (first listed by Singer & Smith in 1946)—apply to common well-known taxa that “belong to the generic clade that does not include *L. piperatus* but ... comprises most of the well known northern hemisphere taxa that are part of the various revisions and monographs of the genus.”

So as to limit the number of name changes resulting from recognition of two phylogenetically supported genera, Buyck & al. propose *L. torminosus* as conserved type for *Lactarius* because *L. torminosus*, “clearly part of [Persoon’s] protologue of the genus,” is type of a primary and diverse subgenus (*Piperites*), its diagnostic white latex color is the most common in the genus (in contrast to the carrot orange latex characterizing the other possible lectotype, *L. deliciosus*), and original material cited in the sanctioning work is available. Lectotypification of *Lactarius* by *L. torminosus* would leave most current *Lactarius* names unchanged and avoid “hundred of combinations” in the forgotten genus *Lactariella* while those in the “*L. piperatus*” clade would revert to *Lactifluus*, where numerous significant combinations already exist. Although a few widely used names will still need to be transferred, conservation of *L. torminosus* as type would limit name changes to ~90 species, “considerably less than the several hundred combinations required if the type is not conserved as proposed.”

Committee comments on Prop. **1919**:

→**NORV 91215**: The authors are acknowledged morphological experts in the *Russulales* who understand the problems to be encountered in dividing an easily recognizable and popular mushroom genus into two based on DNA multigene sequence analyses. By opting to select a type that will preserve the name *Lactarius* for the much larger clade, they will keep the nomenclatural turbulence to a minimum. I support this proposal.

NEW: PROP. 1926, Proposal to conserve *Cladia* against *Heterodea* (Ascomycota).
Proposed by H. Thorsten Lumbsch, Teuvo Ahti & Sittiporn Parnmen. TAXON
59(2): 643 (2010).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1926**:

SUMMARY: Molecular data from 2000 show the genus *Heterodea* Nyl. (1868), currently represented by its type (*H. muelleri*) and another species added in 1978, nested within the *Cladoniaceae*, where it is currently classified.

In 1870, the generic name *Cladia* was introduced in a discussion in a *Ramalina* monograph for three species, the validly published *C. aggregata* and two others referred to only by epithets in the accusative and lacking basionyms and author citations. The epithets (*retipora*, *schizopora*), however, were “unique published fungal

epithets applied to lichens prior to 1870” that distinct indirect evidence ties to *Baeomyces retiporus* and *Cladonia schizopora*. In 1981 Filson was the first to lectotypify *Cladia* with *C. aggregata*; the combinations *C. retipora* and *C. schizopora* were validated later. Currently 14 species are accepted in *Cladia*, a genus that has been classified in *Cladoniaceae* or in the separate *Cladiaceae*.

A current (2010) molecular study shows that not only does *Heterodea* nest within *Cladoniaceae* but within the genus *Cladia* and indicates that the two genera are “best regarded as congeneric,” an inference also supported by morphological characters and chemistry. *Heterodea* is represented by two Australian species while the 14 species in *Cladia* are widely distributed throughout the southern hemisphere. As the name *Cladia* has been widely used in numerous publications and no authors have ever included the genus within *Heterodea*, the authors propose the well established *Cladia* for conservation against *Heterodea*.

Committee comments on Prop. **1926**:

→**NORV 100104**: Despite the nomenclaturally convoluted origin of the names of two of the three species originally placed in *Cladia*, the name *Cladia aggregata* was validly published. I tend to favor this proposal, particularly as it makes little sense to absorb a seemingly universally accepted larger genus within a more restricted smaller one. Nonetheless I await with interest Australian May’s and New Zealander Pennycook’s opinions on the matter.

NEW: PROP. 1927, Proposal to conserve the name *Agaricus rachodes* (*Basidiomycota*) with that spelling. Proposed by Else C. Vellinga & Shaun R. Pennycook. TAXON 59(2): 644 (2010).

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1927**:

SUMMARY: *Chlorophyllum rachodes* is a common, widespread edible mushroom species whose name has been challenged by the alternate spelling, *rhacodes*. When Vittadini described *Agaricus rachodes* in 1833, he provided no etymology but used that spelling consistently. Fries first introduced the variant spelling *rhacodes* in 1849 but alternated between *rachodes* and *rhacodes* during his lifetime. After Saccardo (1887) listed the species as *rhacodes*, the species was referred to as *rhacodes* in Europe until the late 20th century, while outside Europe *rachodes* was the more common spelling. After a brief discussion of potential etymologies for both spellings, the authors point out that although *rhacodes* is now used twice as often on the internet (due in part to the usage of that spelling by both Index Fungorum and MycoBank), this would probably quickly change once those “authoritative institutions” reintroduce the original *rachodes*, just as the usage changed over a century ago in Europe after Saccardo listed *rhacodes*. Vellinga and Pennycook propose to conserve *Agaricus rachodes* with the original spelling under Art. 14.11, noting that ‘rachodes’ has remained constantly in use throughout the names 175’s 175-year history and that a “philological justification” for a change cannot be sustained because of the unknown etymology, and Arts. 60.1 and 60.3 clearly support retention of the original spelling

Committee comments on Prop. **1927**:

→**NORV 100104**: Having swum in a sea of indecision regarding the spelling of this epithet for almost four decades, I regard this proposal as well overdue. Ignoring etymologies that support either ‘rachodes’ or ‘rhacodes’, the authors neatly cut the Gordian knot by invoking Arts. 14.11, 60.1, and 60.3. I firmly support Prop. 1927 and cheerfully accept the mushroom *rachodes* as one of my most favorite edibles.

NEW: PROP. 1945, Proposal to conserve the name *Thelephora comedens* (*Vuilleminia comedens*) with a conserved type (*Basidiomycota*). Proposed by Masoomeh Ghobad-Nejhad & Nils Hallenberg. TAXON 59(4): August–July 7_online_fast-track (2010). [Provided to Committee members in Mailing 11.]

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **1945**:

SUMMARY: The name *Thelephora comedens* Nees : Fries is currently applied to a “basidiomycetous corticioid fungus presently known as *Vuilleminia comedens* (Nees : Fr.) Maire, which serves as type for the genus *Vuilleminia* Maire. The epithet *comedens* is typified by a color drawing of a specimen in UPS that appears not to have been examined by anyone (including Fries) since publication of *T. comedens* in 1816–1817; the authors, who are unaware of any other Fries or Nees specimens representing *T. comedens*, regard the UPS specimen as the only extant material. Microscopically, the specimen does not conform to the current concept of *V. comedens* but rather represents a *Hyphoderma*. To preserve the stability of the species concept with the name, the authors have proposed as a conserved type a specimen collected by Petrak from *Quercus* that they consider represents *T. comedens* as currently recognized.

Committee comments on Prop. **1899**:

→**NORV 100709**: I tend to support this proposal.

Proposals to amend the code —

[only those directly pertaining to fungi (in the broad sense) are cited below for the present. Anyone wishing to comment on other proposals is invited to do so, and the proposal will be added to the Commentary.]

PROPS. (016–020), proposals to amend the CODE to make clear that it covers the nomenclature of fungi, and to modify its governance with respect to names of organisms treated as fungi. Proposed by David L. Hawksworth, Pedro W. Crous, José C. Dianese, Marieka Gryzenhout, Lorelei L. Norvell & Keith A. Seifert. MYCOTAXON 108: 1–4; TAXON 58(2): 658–659. (2009).

[To appear on Ballot 2010-1]

Background & Guest comments on Props. **016–020**:

SUMMARY: Hawksworth & al. propose necessary text changes to clarify that *Fungi*, now regarded as belonging to same superkingdom as *Animalia*, are still covered by the ICBN. Among several changes requested are **(016)** changing the title to the INTERNATIONAL CODE OF BOTANICAL AND MYCOLOGICAL NOMENCLATURE, **(017)** replacing “plant/s” by “plant/s or fungus/i” throughout, **(018)** providing for election of the Permanent Nomenclature Committee for Fungi by an International Mycological Congress, **(019)** permitting decisions on proposals relating solely to fungi to be taken at an International Mycological Congress, and **(020)** making such decisions binding on the subsequent IBC Nomenclature Section.

Committee comments on Props. **016–020**:

NORV 90423: As I favor evolution over revolution, I support this proposal, particularly given the historical presence of fungi within the botanical code. Despite the growing support for an independent mycological code, most supporters do not realize the time and effort it would take to write, edit, and govern. Likewise, there are still too many problems to consider launching a unified code at this time.

DEMO 90706: I find that some silly people are resurrecting the idea of a mycological code that I had hope had been abandoned at the International Congress in Tampa (1977, certainly a more presentative Congress than that in Cairns in 2006). What we

need is a unified Biocode, not a multiplication of codes. If we keep on that way, I will ask for a CODE FOR POLYPORES!

If, however, it is the price to pay for peace, I can go with Props. **16** [establish more clearly that the CODE covers mycology] and **17** [replacing ‘plant(s)’ by ‘plant(s) or fungus/fungi’ where needed]. I hope, however, that we will not end up with a CODE OF BOTANICAL, MYCOLOGICAL, ALGOLOGICAL, AND NON-ANIMAL PROTISTOLOGICAL (EXCLUDING MICROSPORIDIA) NOMENCLATURE.

I am more reticent to **18** [provision for electing the Committee for Fungi by an International Mycological Congress] because of the way the 2006 IMC was instrumented.

To **19** [to allow decisions on fungal proposals to be taken at an International Mycological Congress] I am definitely opposed. Nomenclature is a highly technical matter and people dealing with it should congregate at the nomenclature sessions. Mycologists were not that badly represented at the last sessions. Further suppressing institutional votes is opening the door to all kind of demagogic moves.

Something outside nomenclature, but the proposers should not have considered Opisthokonts supported by overwhelming evidence. Treat me to a beer at the next Congress I am able to attend and I shall explain why to you.

NORV 90706: For the time being, I include the discussion points for all proposals within one section. The proposals are, however, presented separately on Ballot 2010-1. If necessary, this commentary section will be reorganized in the next Commentary.

→**TRIE 91030:** I agree with Vincent Demoulin that the Botanical Code as it is should have a sub-title mentioning the groups of organisms it covers: that means the traditional names of the major organism groups as fungi, plants including algae, non-animal protists and the fossil counterparts.

I will not vote for an own mycological code, but looking forward to have a unified biological code.

Furthermore I do not see that there are many mycological experts from a number of countries who are on a long run really interested and engaged to realize nomenclatural sessions and discuss the paragraphs on the high level it is hitherto done within the last decades. So I like the way it is, in an one-week-session prior to the Botanical Congress. At these occasions there are fruitful discussions between mycologists and botanists as well as palaeobotanists regarding the same articles and nomenclatural problems.

Regarding institutional votes there is a long tradition and these votes are often the result of intensive and controversial discussions between the scientists/ curators at the major Natural History Institutions. These persons are daily involved with the handling of type material and with looking for nomenclatural problems in this context. Perhaps one might modify the concept and also include other botanical and mycological institutions with intra-structural tasks, as e.g. data repositories and other institutions that have missions with nomenclatural and taxonomic aspects. I would support this and would encourage the scientists from natural history collections, culture collections, DNA banks, data repositories, and centers etc. in developing countries to use this possibility to strengthen their institutions on the national level and the influence of their institutions in the international context.

→**PRIN 91223:** I am opposed to a separate Mycological Code. What is the purpose of the balkanization of nomenclature codes? Evolutionary relationships are one thing but what has nomenclature got to do with it? It is a purely legislative exercise that relies on established traditions.

(016) and (017): I am ok with these changes. The Code would have to be emended in ca. 30 places.

(018) and (019) Bryologists and especially algologists could argue the same way. I think this proposal overestimates the interest of general mycologists in nomenclatural issues (see Joe Ammirati's comment on Prop. 1769 "What a whole lot of ferment over one species"). Moreover, separate commissions working on the same Code will inevitably create confusion. Even if mycologists only vote and decide on fungal names it will in the long run introduce different standards of interpreting articles of the code. This is even evident within our small circle. For example, as a lichenologist I am astonished to see how often the general public is invoked as a reason to conserve names of basidiomycetes. With lichens this is not an issue. With separate committees on lichens and non-lichenized fungi I bet lichenologists would handle conservation more strictly than basidiomycologists.

PROP. (048–051), proposals to exclude the phylum *Microsporidia* from the CODE. Proposed by Scott A. Redhead, Paul M. Kirk, Patrick J. Keeling & Louis M. Weiss. MYCOTAXON 108: 05–507 & TAXON 58(2): 669. (2009).

[To appear on Ballot 2010-1; more discussion needed]

Background & Guest comments on Props. **048–051**:

SUMMARY: The consensus inferred from molecular phylogenies that the phylum *Microsporidia* is related to *Fungi* has resulted in recent papers treating the *Microsporidia* as fungi. Redhead & al. point out that carrying the taxonomy 'into the realm of nomenclature' may result in unwanted consequences, as virtually all microsporidian species and genera lack Latin diagnoses, making them invalid under ICBN rules. In view of their long protozoan historical association, the authors propose that microsporidian names be returned to their 'pre-2005' status by excluding them from governance by the CODE by altering the (048) Preamble, (049) Art. 13.1(d), (050) Art 45 (removing Ex. 10), and (51) Art. 54.1(a).

Committee comments on Props. **048–051**:

NORV 90425: I support this proposal.

DEMO 90706: I have not thought much about that but my reaction is to allow publication in another code as for algae. (I worked much on that, especially for the blue green algae at Sydney, and an experienced bacteriological nomenclaturist (Oren) told me that what we accomplished then was really excellent.)

→ **PRIN 91223:** (048–051) Supported.

NEW: PROPS. 172–174, Three proposals to amend Article 59 of the CODE concerning teleotypification of fungal names. Proposed by Walter Gams, Walter M. Jaklitsch, Roland Kirschner & Martina Réblová. TAXON 59(4): August_ July 7_online_fast-track fast-track (2010). [Provided to Committee members in Mailing 11.]

[To appear on Ballot 2010-1; discussion needed]

Background & Guest comments on Prop. **172–174**:

SUMMARY: Three proposals to clarify the effect of teleotypification are provided. Prop. 172 proposes to eliminate Art. 59.7 and thereby return the article to the pre-Vienna situation. The alternate Prop. 173 would modify the current Art. 59.7 so as to avoid including taxa with teleomorph-typified names within otherwise entirely anamorphic genera. If Prop. 173 is enacted, Prop. 174 would add Recommendation 59A.4, which specifies that newly discovered anamorphs should only be classified under teleomorph-typified generic names when no suitable anamorph-typified generic names are

available; Prop. 174 further species that a subsequent discovery of a teleomorph will require epitypification by a specimen exhibiting the teleomorph.

Committee comments on Prop. **1899**:

→ **NORV 100709**: I withhold comment until after the August IMC9 nomenclatural symposia.

Special discussions & questions —

Orthography recommendation 1

TO THE ROOT OF THE R~RR~RH~RRH PROBLEM/

[To appear on Ballot 2010-1; withdrawn from previous ballot due to confusing language]

KIRK 70727 [Orthography] ‘Correct’ spelling of epithet containing ‘...**riz**...’ or ‘...**rriz**...’ or ‘...**rhiz**...’ or ‘...**rrhiz**...’

VOTED 1 JUNE (EXTENDED TO 3 JULY) 2009: 1 : 5 : 7 : 0 (No consensus). The ballot proposal was poorly written, leading to understandable confusion by those who responded. Additionally, Vincent’s important and pertinent comment was inadvertently omitted from the April Commentary. This is retained for further discussion and will appear on the November ballot.

KIRK 70727: 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’?

GAMS (ex officio) 80204: Ancient Greek had the rule that in cases of composite words where the second part starts with letter rho (ρ) this rho is duplicated, the second one carrying a spiritus lenis and a spiritus asper (= h). This usage has been adopted in many names and terms (e.g. mycorrhiza). With some training in classical languages I am very much in favour of having general **rules** rather than memorizing the application or refusal of the rule with every name separately. Such a rule, if incorporated in the *Code*, would thus demand the spelling *arrhizus*. Contrary to this rule we have names like *Metarrhizium* and *Zygorhynchus*, which as generic names are not to be altered from the original spelling. With species epithets the indexing authorities apply rules of grammar much more liberally to correct originally misspelled names. So why not have a ruling that applies to *arrhizus* and similar cases.

There is now even a new **rule** that determines all *-botrys* names to be masculine, while the large fungal genera *Arthrobotrys*, *Blastobotrys*, *Stachybotrys* and some others have always been feminine in accordance with the similar genus *Botrytis*. We shall need proposals to treat these genera as feminine in the future, after my efforts to prevent acceptance of the masculine proposal at Vienna have failed.

PENN 80211: I am opposed to all such attempts to ‘correct’ an author’s original orthography, except where it can be clearly demonstrated that the original spelling was the result of a “typographical or orthographical error”. Otherwise ICBN (Vienna) Art. 60.1 + Ex. 1 expressly forbids any correction, including those intended to achieve a “philologically preferable” orthography.

Latinisation of Greek epithets is neither an exact nor a prescriptive process. Of course it is desirable that referees and editors persuade authors (while their names are still in manuscript) to follow a Latinisation standard such as that presented by Stearn, *Botanical Latin* 4th Ed., p. 254. But once a name has been published, ICBN Article 60.1 must be followed. Stearn, p. 255, commenting on ‘r-’ versus ‘rh-’, states: “the original spelling of an author should be accepted”; and on p. 261, commenting on ‘-rh-’ versus ‘-rrh-’, states: “as many reputable authors, Linnaeus among them, have omitted this additional *r*, it is best regarded as optional and an author’s original spelling . . . should be accepted.”

The *Pisolithus* example introduces additional complications:

(1) Assuming the Index Fungorum entry is correct that Scopoli’s original spelling was ‘arrizon’, this is a Greek neuter epithet that has NOT been Latinised — it has (I believe) been faithfully transliterated, letter by letter, from the Greek alphabet to the Latin alphabet. Following ICBN 60.1, the Greek spelling and grammar of such epithets ought to be preserved by subsequent authors, and an appropriate Greek termination should be used if the taxon is transferred to a genus of a different gender. The correct epithet for the *Pisolithus* combination based on *Lycoperdon arrizon* is ‘arriz-’ + the appropriate Greek masculine termination. {Could a Greek scholar please elaborate.}

(2) Persoon (1801) transferred the basionym, *Lycoperdon arrizon*, to *Scleroderma*. The epithet orthography should have remained as ‘arrizon’, but Persoon Latinised it as ‘arhizum’ (a correctable error, I would argue). {Persoon’s index entry ‘Lycoperdon arrhizon’ refers to both Batsch’s *L. arrhizon* (p.137; correctly transcribed but not sanctioned) and Scopoli’s *L. arrizon* (p.152).}

(3) I cannot find any reference in ICBN to “sanctioned spelling”; it is names that are sanctioned, not orthographies. In my opinion, the sanctioning authors should be under the same obligation as us lesser mortals to preserve the original basionym orthography, and their “typographical and orthographical errors” should be subject to correction (Art. 60.1).

(4) Proponents of “sanctioned spelling” need to explain why this putative principle is not uniformly applied. Many orthographies that Fries published in the sanctioning works are routinely corrected; e.g., *Agaricus* ‘cepastipes’, *Hydnum* ‘cinnabarrinum’, *Polyporus* ‘cinnabarrinus’, *Sphaeria* ‘cinnabarrina’, *Bulgaria* ‘inquians’, *Dothidea* ‘nervisequia’, *Polyporus varius* var. ‘numularius’, *Sclerotium* ‘pustulla’, *Peziza* ‘pustullata’, *Hydnum* ‘septemtrionale’, etc. What exactly is the distinction between a “sanctioned spelling” and a correctable error in the sanctioning works?

PENN 80212: I should make it clear that my comments of 12 February 2008 refer only to epithet stems, not to incorrect terminations and inappropriate compounding forms (both subject to correction; Articles 32.7, 60.8).

DEMO 80422 [This was inadvertently omitted from the previous

commentary]: In his *Synopsis Fungorum* (1801) which has been the starting point book for the nomenclature of GASTEROMYCETES until the Sydney Congress (1981), and now is the sanctioning book (ICBN Art.13), Persoon recognises two species for the northern hemisphere *Pisolithus*. One is named *Scleroderma tinctorius*, with reference to Micheli’s T.93, f.1, and the other *S. arhizum*, with reference to *Lycoperdon arhizon* (sic) Scop., *Delic. Insubr.* 1. P. 40.

Until Rauschert (1959) demonstrated that for homonymy reasons the name *Pisolithus tinctorius* (Pers.) Coker and Couch could not be used, this has probably been the most popular. In fact while the taxonomic literature has rapidly accepted the correct name *Pisolithus arhizus*, the community of mycorrhiza researchers has frequently ignored it. Given the ecological and economical importance of the fungus, if conservation of species name had been available in 1959, one could have argued then for the conservation of *P. tinctorius*. With more than 40 years of correct usage by taxonomists it is now too late for such an action and the only thing to do is convince the mycorrhiza workers to use correct nomenclature. This would also have a practical advantage insofar as by using *P. arhizus* authors show they are aware of nomenclatural actuality but hopefully also taxonomic actuality and only refer to the northern

hemisphere taxon, while *P. tinctorius* has usually be used in a broad sense including every species of the genus.

With the nomenclature of *Pisolithus* there is not only an unusual problem of homonymy, but also some orthographical problems. The original publication is to be cited “*Lycoperdon arrizon* Scop., Delic. Fl. Faun. Insubr. I: 40, tab. XVIII, 1786”. The sanctioning was however in the form “*Scleroderma arhizum* (Scop. : Pers.) Pers., Syn. Meth. Fung.: 152, 1801”. In the citation of the basionym, Persoon wrote *Lycoperdon arhizon*. It is probable that Persoon considered rho with spiritus asper could equivalently be transcribed rh or rr. He however overlooked that in compounding, when the first word element ends in alpha rr is the norm. The adjective *arrizos* existed in classical greek (Aristotle). The best transliteration would be *arrhizos* as in the homonym of Scopoli’s name *Lycoperdon arrhizon* Batsch, Elench. Fung. Continuatio prima: 239, 1786 (a synonym of *Bovista plumbea*, the priority between those names is uncertain but without importance given the sanctioning of Scopoli’s name).

The other difference between Persoon’s and Scopoli’s spelling that of the termination comes from the fact greek adjectives in –os are generally transcribed in –us in latin.

The most correct spelling would be *Lycoperdon* or *Scleroderma arrhizum* and indeed both Fries (1832) and Hollos (1904) automatically corrected Scopoli and Persoon variants to that spelling.

In the application of the code one should first decide if any of those spelling is an orthographical error to be corrected under art. 60.1. My opinion is that even if some of those spellings are more correct than others none is a true orthographical error and all can be considered orthographic variants(in the code the transcription of the spititus asper by h is for example just a recommendation, 60A/.2).

This means that without sanctioning one would have to write *Pisolithus arrizos*. Sanctioned names are however treated as if conserved (art.15.1) and the spelling used by the sanctioning author should thus be retained. This is not explicit in the code but is part of the logic of sanctioning which aims at using a 1753 starting point without disturbing the nomenclature of names which were part of the former starting point books. The jurisprudence of the Special committee for Fungi has indeed been to consider orthography is sanctioned. An explicit statement in the code or at least an example would however be good and I will propose it for the next edition.

The correct citation for the northern hemisphere *Pisolithus* is thus *Pisolithus arhizus* (Scop. : Pers.) Rauschert.

PENN 90527: I would like more clarity and precision in the wording of the proposition; the current wording is sufficiently loose/ambiguous for me to be unsure how to represent my opinion with Yes/No vote.

NORV 90527: I agree with Shaun and apologize for the ambiguous choice offered to the Committee. [Ballot 2009-1 offered the choice: The sanctioned spelling of a name containing the element ‘rhiz’ is recommended over a more common or botanical spelling.]. I was somewhat unsure of how to word the discussion on the ballot and invite Paul to send in a clearly rewritten sentence to be considered on our fall ballot. Should, for instance, we recommend that the spelling ‘rhiz’ be regarded as preferred (and thus warranting correction) over the other alternative spellings of the element meaning ‘root’, regardless of the spelling proposed by the original author of a taxon?

PRIN 90603: I don’t really understand the meaning of the recommendation as it appears on the ballot. What is a sanctioned spelling (spelling of a sanctioned name, original spelling?), what is a botanical spelling (grammatically correct spelling?)? Could this be re-worded to make it more clear for me how to cast my vote?

→ **PRIN 91223:** Re GAMS 80204: Although it is tempting to have a general rule, the case of botrys-names shows that general rules can have adverse effects. Regardless of the special case here, I think instead of introducing strict rules one should keep with the first [original] author.

Re PENN 80211: I don't think there are many editors now that would even recognize wrong latinization of Greek, let alone check the code for suggested new names of species. In case of a general rule it probably means that lots of names will be published that have to be corrected later on.

I think the fewer rules and articles the better.

→ **NORV 100710:** After rewriting the Ballot 2010-1 entry and re-reading PRIN 91223, I realize that there is no difference in choosing among rhiz (which I prefer) and four other variants and choosing between rachodes/rhacodes in Prop. 127. Where controversy exists, rather than agonize over the low state to which philology has fallen, I find it more pragmatic to follow the spirit of Art. 60.1 in accepting the element as spelled by the original author.

NEW! The correct name for “niu-chang-chih”

Ganoderma camphoratum* vs. *Antrodia cinnamomea

[The following discussion regarding the nomenclatural status of an important Chinese medicinal mushroom was originally sent by Sheng-Hua Wu (Taiwan) to Paul Kirk, who has forwarded it along with pertinent publications to the Committee for discussion. Background provided by Prof. Wu is followed by comments from Paul and Scott Redhead from Email correspondence. Background publications provided by Prof. Wu were sent to the Committee in Mailing 13.]

→ **Sheng-Hua Wu (Taiwan) 91229:** This fungus (niu-chang-chih) is a very expensive medicinal polypore known only from Taiwan, and is restricted on the tree species *Cinnamomum kanehirai* (also endemic to Taiwan).

The first publication of this species was a collaboration by Prof. Zang (China) and Prof. Su (Taiwan). Su consulted Zang identification about this medicinal specimen. Zang is actually specialist of agarics but may be not specialized in polypore taxonomy. They together (Zang & Su 1990, Acta Bot Yunnan 12: 395-396) published a new species: *Ganoderma camphoratum* (as '*comphoratum*').

The Zang & Su publication wrongly described basidiospores (with spines) not belonging to this species, and Zang also mistakenly using another trees species (...*C. camphoratum* [instead of] *C. kanehirai*). Su knew that the type specimen is a real "niu-chang-chih" and understood the mistaken description about the spores and host species made by Zang, after publication.

Dr. Chang studied polypores in Taiwan. He mentioned to me at the Vancouver Congress (1994) that he considered to present "niu-chang-chih" again because of the mistakes from the first publication. My response was the mistake (and its generic placement) can be corrected, but not necessary to present a new species again. Meanwhile Chang and Chou (Chang & Chou 1995, Mycol. Res. 99: 756–758) published it again as new species [*Antrodia cinnamomea*] without mentioning the first publication.

Later on I borrowed type specimens of both *Ganoderma camphoratum* and *Antrodia cinnamomea* and found both are good specimens with all necessary characters of the same species. I consulted Dr. Ryvarden and his opinion that the first publication is valid and new combination for *Ganoderma camphoratum* should be made (the same conception with me). I further talked with Dr. Chang, and he agreed that we together published this new combination, and to treat *Antrodia cinnamomea* as a later synonym (Wu et al. 1997, Bot Bull Acad Sin 38: 273-275).

The problem should be thus settled. But, afterward Chang regretted this collaborative publication and still uses his *Antrodia cinnamomea* for "niu-chang-chih". In the paper by Chang and Chou (2004, Bot Bull Acad Sin 45: 347-352), they regarded *Ganoderma camphoratum* M. Zang & C.H. Su as a *nomen confusum*, and proposed to stand a later synonym, *Antrodia cinnamomum* T.T. Chang & W.N. Chou, as the correct name for the "niu-chang-chih". In their opinion, "the holotype of *G. camphoratum* is composed of two heterogeneous elements belonging to two different fungal taxa: one is a *Ganoderma* species, and one is *A. cinnamomea*"; and the ICBN Article 9.12 (Greuter 2000) "gives a clear indication that *G. camphoratum* must remain attached to the part corresponding most nearly with the original description or diagnosis". They further interpreted that the protologue of *G. camphoratum* (Zang and Su 1990) clearly indicated that the *Ganoderma* element is what the naming authors intended to describe, and the *Ganoderma* element in the original material is entirely limited to the basidiospores attached to the surface of the *Antrodia* basidiomata. However, I think, it was misleading for Chang and Chou (2004) to quote ICBN Article 9.12 to deny the valid publication of *G. camphoratum*. Zang and Su (1990) never possibly intended to name the new species *G. camphoratum*, based on the part of "*Ganoderma* basidiospores" attached to the surface of the "*Antrodia* basidiomata". In the protologue, they provided all necessities for meeting requirements for validating publication of a new taxon, but they mistakenly described the alien spores, as its own. The mistake appeared in a sentence, only representing a small portion within the overall description prepared for all kinds of structures in the "*Antrodia* basidiomata". It is evident that nothing is "*nomen confusum*" in regard to the "good" type specimen of *G. camphoratum*, although the description in the protologue might not be so good due to mistakenly describing its spores. Another mistake indicated by Chang and Chou (2004) against *G. camphoratum* was that the host tree of *G. camphoratum* is *Cinnamomum kanehirai*, rather than *C. camphora* (source of epithetic name of the fungus) mistakenly cited in the protologue. However, this kind of mistake does not affect the valid publication of a new taxon. See ICBN Article 51.1, for explanation.

Above is my understanding that *Ganoderma camphoratum* should be the first valid publication for "niu-chang-chih", and my explanation is according to my knowledge of ICBN. How is your opinion?

Another generic placement problem is we found "niu-chang-chih" does not belong to *Antrodia*, but belongs to an independent genus (*Taiwanofungus* gen. nov.) together with another very allied species (*T. salmoneus*, also endemic to Taiwan, is restricted to another tree host) (Wu SH et al. 2004, Fungal Sci 19: 109-116). And a comprehensive phylogenetic analysis will be published in Bot. Stu. next Jan (2010? ms attached).

- KIRK 91229:** You are correct in your interpretation of the Code. If a type consists of two or more elements the name must be attached to that element which most closely matches the original diagnosis and in this case the majority of the diagnosis applies to the *Taiwanofungus* element. However, I will seek a second opinion on this by consulting colleagues on the IAPT Committee for Fungi in the next few days.
- REDH 91231:** Art. 9.12 definitely guides the process of choosing a lectotype, and it is backed up by Art. 9.17(c), or 9.17(b). In looking at the wording of Art. 9.12 I note that the word "specimen" is used and although the term "specimen"

is sometimes hard to apply for fungi, in this case the conk is certainly a specimen while the dusting of spores might not seem to be a “specimen”. There are other cases out there where contaminating spores have been described by mistake because they were more conspicuous. However, I note that in the 2004 publication Chang TT, Chou WN go through the effort to argue that the “*Ganoderma*” spores were the more important element and to try to actually “lectotypify” the name (somewhat poorly worded) by those elements. Therefore, there are arguments both ways and truly this is the sort of question that should go to the whole committee for a binding vote and not just by we select few.

→**REDH 100101**: ...My inclination is to support the idea that the first name for niu-chang-chih is *Ganoderma camphoratum* (*‘camphoratum’*) [seemingly first lectotypified by the authors who wanted to reject it], but it has been argued both ways by two sets of authors each supportable with some logic. Let’s have more people weigh in. Otherwise it could go on for a long time.

Additionally, as the type of the generic name *Taiwanofungus* is *Ganoderma camphoratum*, it is critical to know how to apply the type name. Also, I think you should proactively tell these authors that the name *T. salmoneus* is invalid. Somebody should correct it.

General remarks —

Article 59

The Special Committee on Names for Pleomorphic Fungi (as of November 4, 2009, comprising Chair Rossman, Secretary Redhead, and members Bischoff, Gams, Hawksworth, Kirk, Sampaio, Seifert, Simpson, and Spiegel) has been instructed to evaluate the impact of modifications (or elimination of) **Art. 59**, and forward recommendations to us, the permanent Nomenclature Committee for Fungi, and to the General Committee for further input before making recommendations to the Nomenclature Section at the 18th Botanical Congress in Melbourne in 2011.

REDH 81103 (7th & 8th circular cover letter & circulars) and **81105** (9th circular): During 2007 Cheryl Grgurinovic & Tsuyoshi Hosoya resigned, to be replaced by Pedro Crous and Jack Simpson, with current membership remaining at 12.

12 votes were returned for the special ballot held prior to the 2007 MSA Baton Rouge meeting with 21 % voting to return to the St. Louis Code version of Art. 59 (before epitypification by a teleomorph), 16.5% to delete Art. 59, and 62.5% to modify Art. 59. Hawksworth later polled MSA and two other meetings, where most returning votes preferred that mycologists work toward a unified nomenclature.

Work continues within the committee regarding status and modification of Art. 59. Please contact Scott Redhead for additional information.

REDH 90928 (10th circular cover letter) and **90930** (10th & 11th circulars): Since circular 9 was sent out in 2008, only one response was received within the deadline and a second received 2 months later. Circular 10 therefore presents functional changes to accelerate much-needed input. These changes include a revised timetable (with final committee recommendations due prior to March 2010) and procedures modified to encourage all responders to ‘reply all.’ Other interested individuals added to the mailing list include Aoki, Toni Atkinson, Graefenhan, Minnis, Pennycook, and Stalpers. The terms “teleotypification” and “teletype” are introduced in attached documents.

→**REDH 91011** (Prop. 183 ballot): “Add a new Art. 59.7: “59.7. A separate name proposed on or after 1 January XXXX, for an anamorph associated with a pre-existing named teleomorph, or for a teleomorph associated with a pre-existing named anamorph, is illegitimate (Art. 52.1) and to be rejected. Where the earliest legitimate name is that of an anamorph, an epitype exhibiting the

teleomorphic state is to be selected so that the anamorph name can be used as the name of the holomorph (see Art. 59.8).” Ballot – do you support this proposal as it is currently worded? _ Yes; _ No.

–>**REDH 91022** (Prop. 183 ballot report): “Proposal fails because of a 50% split vote. There were 12 official members when the ballot was mailed, meaning the 7 or 8 votes were needed to reach 60%, I would have accepted 7 for the sake of argument, but in this case all 12 members voted and 6 supported the proposal and 6 rejected it [as currently worded]. Hence I conclude for reporting purposes that the proposal is not supported, and this officially is what will be reported as a minimum to the General Committee.

“Obviously the mycological community is still deeply divided on this issue. Two unofficial members “voted” no, also. However, the issue regarding Art. 59 is not dead as some who voted “no” to the current wording would consider alternative wording. This will be discussed along with some of your comments in the next circular, and another ballot will be sent on this and other issues (e.g. teleotypification) shortly because time is pressing.”

Secretary Redhead also noted the resignations of Wingfield and Crous, thanking them for their contributions.

–>**REDH 91104** (Circulars 12 & 13, with correspondence summary and Crous publications): The two circulars briefly discuss the Prop. 183 ballot results and ideas raised by Crous in two publications (Crous et al. 2009, *Persoonia* 23: 99–118 & 119–146).

NOTE: Prop. 1854, proposal to conserve the name *Teesdalia* (*Cruciferae*). Proposed by Al-Shehbaz & Warwick. *TAXON* 57: 1357–1358, by

REDH 90428: It should be noted that this proposal to conserve a vascular plant name results from an unanticipated consequence of sanctioning of the fungal name *Guepinia* Fr. : Fr. (*Syst. Orb. Veg.* 1: 92. 1825 : *Elench. Fung.* 2: 30. 1828), which made *Guepinia* Bastard (*Suppl. Essai Fl. Dépt. Maine-et-Loire* 35. ante 31 Jul 1812) unavailable but did not prevent *Teesdalia* W.T. Aiton, *Hortus Kew.*, ed. 2., 4: 83. Dec 1812 from becoming illegitimate. A note from our committee that the Committee for Vascular Plants has our sympathy and support on this proposal may be appropriate.