

NOMENCLATURE COMMITTEE FOR FUNGI
NOMENCLATURE CF COMMENTARY 14 — 14 FEBRUARY 2011

COMMENTING PROCEDURES —

Proposals requiring Committee action are numbered by IAPT and published in TAXON (see free download at <http://www.ingentaconnect.com/content/iapt/tax>). As Secretary, I will send whatever additional references are necessary to complement the formal proposals. COMMENTARIES contain the accumulated responses sent to the Secretary by CF members who comment on TAXON proposals and other items brought before the Committee. Members are asked to separate comments according to proposal number, edit for clarity and brevity, and forward pertinent publications or comments to me for forwarding to all CF members. I list the names of guest experts and proposal authors invited to comment at the end of the appropriate summary and integrate their comments chronologically. Commentaries (emailed to Committee members as PDF attachments) are cumulative, so that the most recent commentary contains all discussion on an open topic. Comments on recommended or rejected proposals are withdrawn from future commentaries only after they have been included in the Commentary sent out at the same time as ballot results are reported. All previous and current commentaries, important committee correspondence, and formal reports may be downloaded by members and non-members via the International Mycological Association website [<http://www.ima-mycology.org/CF>].

Comments are grouped 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). Unless otherwise stated, 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 assign temporary CF numbers to topics or unassigned proposals; such 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 or guest name on the first line.

Ballots are 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 fail to return a ballot and respond to repeated requests or subsequent reminders are considered to have resigned and will be replaced. Any request for more discussion may outweigh a majority decision of the Committee when the request for discussion is accompanied by a formal statement explaining why such discussion is asked for; such proposals will remain open for further discussion for at least one more commenting period.

Lorelei Norvell, *Committee Secretary*
llnorvell@pnw-ms.com

1. Proposals to conserve or reject names –

PROP. (1861) to conserve the name *Aspicilia farinosa* (Ascomycota: Pertusariales: Megasporaceae) with a conserved type. [Originally: *Aspicilia farinosa* (Flörke) Hue, Nouv. Arch. Mus. Hist. Nat. sér. 5, 2: 61. 1910; now updated to *Aspicilia farinosa* (Flörke) Flagey, Revue mycol., Toulouse 10: 131. 1888.] Proposed by originally by Anders Nordin & Claude Roux. TAXON 58(1): 292 (2009); amended by the same authors, MYCOTAXON 110: 493 (2009).

Two proposals, **(1861a)** and **(1861b)** appeared on BALLOT 2011-1;
See NORV (SECRETARIAL NOTE) 101218 for explanation.

BALLOT 2010-2 RESULTS: 2 yes : 1 no : 0 abstain : 11 discuss

PROP. (1861a) to conserve the name *Aspicilia farinosa* (Ascomycota: Pertusariales: Megasporaceae) with a conserved type. [Originally: *Aspicilia farinosa* (Flörke) Hue, Nouv. Arch. Mus. Hist. Nat. sér. 5, 2: 61. 1910; now updated to *Aspicilia farinosa* (Flörke) Flagey, Revue mycol., Toulouse 10: 131. 1888.] (Original, amended proposal)

BALLOT 2010-2 RESULTS: 2 yes : 1 no : 0 abstain : 11 discuss

BALLOT 2011-1 RESULTS: 3 yes : 5 no : 4 abstain : 2 discuss.

→ Prop. (1861a) is withdrawn at the request of the proponents. Discussion will be retained for at least one more round, pending receipt of Prop. **1861(c)** by one of our members. (Please refer to the three new comments below.)

PROP. (1861b) to conserve the name *Aspicilia farinosa* (Nyl.) Flagey (1872, non (Flörke) Flagey, 1888) with a conserved type. Developed in NCF Committee, 2009–2010.

BALLOT 2011-1 RESULTS: 4 yes : 4 no : 4 abstain : 2 discuss.

Background comments on Prop. (1861)

SUMMARY: *Aspicilia farinosa* (Flörke) Hue (1910; = *Urceolaria calcarea* ~~λ~~ *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 (see **Proposers Nordin & Roux 91016** below), who originally proposed conserving the name with the Nylander collection under the name *Aspicilia farinosa* (Flörke) Hue 1920 so as to continue the current concept for the species as originally presented by Hue and Arnold, later found that the combination had earlier been made by Flagey in 1888.

Guests commenting on Prop. 1861 include HARRIE SIPMAN [former NCF membe; lichen expert, Berlin, Germany: 91102, 101011], IBC Rapporteur-Général JOHN MCNEILL [Edinburgh, Scotland: 101212], and ANDERS NORDIN [Prop. 1861 co-author,

Committee & Guest 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?

PROPOSERS Nordin & Roux 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.

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.

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.

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.

(GUEST) Harrie Sipman 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.

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.

REDH 101001: There seems to be ample unease with this proposal that I would like it to go another round to see if there are responses by other committee members to the latest responses we just read.

DEMO 101012: I accept the argument of PRIN 91223 that an epitype is not the solution and as in that comment as well as that by Sipman, that conserving the Flörke name in a sense that does not fit a taxon occurring in the original area is also inadequate. I however would not like to see *Aspicilia farinosa* resurrected in a sense that does not answer current usage. I would thus suggest accepting the proposed conserved type but for *Aspicilia farinosa* (Nyl.) Flagey non (Flörke) Flagey. I am somewhat uneasy because one is dealing with a combination, a case not cited in Art. 14.9, and will welcome comments. Anyway, this means more discussions are needed on this proposal.

REDH 101027: Having read of the concerns over the well delimited geographic distributions that exclude the possibility of this species being in Germany, I no longer support the proposal even if authorship were changed to “(Flörke) Flagey 1888”. However, the solution suggested by Demoulin to conserve a type with a different authorship may be a solution [under Art. 14.9] that is worth consideration. However, I am not certain how this would be worded because I am not certain what the conserved Nylander basionym would be, or why Flagey (1888) alone should not be chosen, or in his exsiccata series (LICHENES ALGERIENSES EXSICCATI No. 244), or on what date that exsiccata set was distributed. We need some more information.

DEMO 101104: Like Scott I think more discussion is needed and the solution might be in conserving with a different authorship. I have one question to lichenologists. Why does Flagey (REV. MYCOL., Roumeguère 10: 131, 1888) cite first *Aspicilia farinosa* Nyl. and immediately below, for a second locality, [cite] *Aspicilia farinosa* Fl. Hepp ?

PRIN 101111: Proposal 1861 aims at conserving current usage of the name *Aspicilia farinosa*, i. e. application to a mediterranean taxon that differs from Flörke's type material collected near Berlin. Harrie Sipman is concerned that conservation would result in a record of a mediterranean species from near Berlin that would finally end

up on the German Red List, because it hasn't been found in Germany since. I do not share these concerns. If *Aspicilia farinosa* is conserved with a conserved type, then Flörke's type specimen is "detached" from the species, because it is no longer conspecific with the type of *A. farinosa*. But Flörke's original description of the species would then be something like a misidentification, which is also a bit odd.

I think there may be a solution without a conserved type. Vincent pointed out that Flagey (1888) cites *Aspicilia farinosa* twice, first as "*Aspicilia farinosa* Nyl." and immediately below as "- *farinosa* Fl. Hepp." Conserving *Aspicilia farinosa* (Nyl.) Flagey, as suggested by Vincent [DEMO 101012], is perhaps not the best solution. Nylander (1872) just mentions *Lecanora farinosa* in a footnote and gives no description.

BUT: Massalongo describes it as "*Pachyospora calcarea* Massal. γ *farinosa*" (Ricerche: 43, 1852). He does not cite Flörke as basionym, but only mentions two potential synonyms (both with a question mark): "*Urceolaria calcarea* v. *farinosa* Ach.? Syn. pag. 144. - *Urceolaria calcarea* a *concreta* b *farinosa* Schaer.? Enum. pag. 91." In 1856 he combines his variety into species rank as "*Pachyospora farinosa* Massal." (SCHED. CRIT. 8: 148), again omitting Flörke's name, but citing "*Pachyospora calcarea* γ *farinosa* Massal." and the two other synonyms (both again with a question mark). In (REV. MYCOL. 14: 72 (1892) Flagey makes the following combination "*Aspicilia farinosa* (Flw. L. E. 300). *Pachyospora farinosa* Mass. SCHED. CRIT. 148". According to Lynge's index, Flotow Lich. exs. 300 is *Lecanora contorta* var. *farinosa* Flk.

My suggestion would be not to conserve *Aspicilia farinosa* (Flörke) Flagey with a conserved type, but to conserve *Aspicilia farinosa* (Massal.) Flagey against *A. farinosa* (Flörke) Flagey.

What do you think?

(GUEST) **Harrie Sipman 101111**: Thanks for showing me the comment of Christian [PRIN 101111]. I am happy to read that he also considers it a bit odd, when Floerke is supposed to have misidentified his own type specimen. I do not know of any similar case, where a neotype gives a content to a taxon that is evidently different from what the author meant. I would prefer when the species epithet *farinosa* sensu Nylander became detached from the variety *farinosa* of Flörke. Evidently Nyl. misunderstood the Flörke taxon when taking over the name, but he defined his species *L. farinosa* well by his exsiccate LICH. PARIS. 127 (issued 1855). Conservation of *Lecanora farinosa* Nyl. with LICH. PARIS. 127 as type would clear the situation except for the combination into *Aspicilia*. However, here the discovery of Vincent Demoulin may help, that Flagey (1888) lists first *A. farinosa* Nyl. and second *A. farinosa* Fl. Hepp. With the first he evidently meant *Lecanora farinosa* Nyl., with the second most probably the different species now called *A. substerilis*, which is equally likely to occur in Algeria. Thus he intended the new combination in the first place for *Lecanora farinosa* Nyl. and *A. farinosa* (Nyl.) Flagey has priority over *A. farinosa* (Flörke) Flagey.

REDH 101112:

This proposal plan is now very messy. Please note that although Art. 14.9 allows us to conserve a different type, and gives us latitude to do this either for a name at its original place of validation or from a later publication using the same name, Art. 14.9 then goes on to say that in the latter case the names are to be treated as if they were homonyms, and the later usage need not have a description. However, in the case of proposal 1861 the original name is either a variety or a rankless subspecific taxon name.

INDEX FUNGORUM lists it as *Urceolaria calcarea* k *farinosa* Flörke, Mag. Gesell. naturf. Freunde, Berlin: 125 (1810) (What does the "k" stand for?)

Conserving a specific binomial such as "*Aspicilia farinosa*" with a different type would not create an earlier homonym at the rank of species.

We then must examine Art. 14.10 that indicates that earlier homonyms are available for combinations. In this case there is no earlier homonym but in principle the earlier name would still be available for recombinations. Which of two available names will be the basionym for each subsequent combination?

Additionally, we cannot conserve *Aspicilia farinosa* (Massal.) Flagey against *A. farinosa* (Flörke) Flagey because there is only one combination made by Flagey, not two. See Art. 14, Note 1 regarding “isonyms”.

I believe we need some guidance from John McNeill on what is allowable. For this we need a bit more time than the November 15th deadline allows. Also, we need to establish what “k” stands for.

NORV 101112: I agree that Prop. 1861 as originally published is in disarray and a thoughtful rewrite is needed to present a clear choice to the Committee. As noted in my introduction to Mailing 28, it seems that in light of the current confusion, we should probably vote to reject the current proposal (on the next, not the current, ballot) and recommend a repaired action based on our discussion, but we definitely need John McNeill’s counsel on how best to proceed. If a formal proposal is required, it is likely that the issue will not be resolved until after the upcoming Melbourne Congress, and so the proposal will remain in limbo for another six years (one reason that I support Prop. 18–19).

You may all be assured this Prop. 1861 will NOT be settled by Ballot 2010-2. Of the 8 ballots already received, there is one vote in support, one vote against, and 6 votes for continued discussion, so the issue will obviously remain open. As you know, only one well-supported vote for continued discussion (with ‘well-supported’ meaning a vote to discuss is backed up by a comment containing good reasons why it ought to continue to be discussed) is needed to hold a decision over to a subsequent ballot.

The “k” within *Urceolaria calcarea k farinosa* Flörke may well be a typo for ‘b’, but conjecture is not good enough. Thus we need Paul Kirk to provide accurate information on source of the ‘k’ currently displayed on INDEX FUNGORUM.

ITUR 101115: I [prefer to reject] Prop. 1861 as currently written and recommend a reworded proposal to be presented to the Committee

PRIN 101115: Regarding REDH 101112: If the description of *Pachyospora calcarea* var. *farinosa* A. Massal. were interpreted as a description of a new variety, the case becomes much clearer. One would only have to conserve *A. farinosa* (A. Massal.) Flagey (1892) against *A. farinosa* (Flörke) Flagey (1888) without any fuss about types etc. It can also be argued that there is more than one combination by Flagey. In 1892, he does not cite Flörke but only Massalongo among the synonyms. A type for *A. farinosa* (A. Massal.) Flagey could be selected from Massalongo’s herbarium in VER.

MAY 101115: I am definitely in favour of more discussion. I agree that typifying with material of the Mediterranean taxon causes problems for a name originally based on material from outside the known area of the Mediterranean taxon. Utilising Art. 14.9 to come up with an alternative proposal seems a good solution. However, I found it difficult to mesh together the names and concepts provided in the original proposal with the information provided by Harrie Sipman, and it would be good to have an alternative proposal to vote on that integrates the information.

Guest) **John McNeill 101212** [replying to a request from the NCF Secretary]:

BASIONYM OF ASPICILIA FARINOSA: First of all, on the technical matter of the publication of the basionym, I would defer to those who can read German more fluently than I can (see attached pdf of Flörke’s paper), but my impression is that if the name is validly published (see below) it should be cited as:

Urceolaria calcarea var. *farinosa* Flörke in MAG. NEUESTEN ENTDECK. GESAMMTEN NATURK. GES. NATURF. FREUNDE BERLIN 4: 125. 1810 (‘λ’).

There is no “k” involved; it is the previous variety that is prefixed kappa (κ).

Flörke does not set out in conventional fashion the name of the species under which he states (on p. 121) that “... wie Varietäten unter eignen Diagnosen nach einander folgen lassen.” – and then proceeds to detail 11 taxa under successive Greek letters, the last being;

“λ) *Urceolaria farinosa* ...”.

Interestingly on the same page (125) is: “κ) *Urceolaria calcaria*

..... *Urceolaria calcaria* Achar. meth p. 142. und tab. 4. f. 1.”

This recognition of 11 varieties is in his “BERICHTIGUNG” no. 12 (p. 118) dealing with “*Urceolaria calcaria*, *tessulata*, *Hoffmannii*, und *contorta* Achar. meth., so wie *Verrucaria tessellata* und *contorta Hoffm.* Flor. 1795.”

I mention all this because it is just possible that under Art. 33.1 these are not validly published varietal names. I don't have time to wade through the German to see if Flörke definitely associates these varieties with one particular species name but I assume the proposers or some members of the Committee have – or will!

GENERAL POINTS RE THE CODE: There are three general points relating to the Code that seem relevant.

1) The second option in Art. 14.9 (second sentence) is a hangover from the time when there was no provision for explicit conservation of a type and also before there was provision for conserving species names. At that time the only way to conserve a generic name in its current application, when different from the original, was to treat a later usage, in which the desired type was included, as though it were a later homonym (even although there was no suggestion of excluding the “original type” as required by Art. 48.1). I fail to see any need for this provision under the current rules and I am not aware of any use of it since at least the Tokyo Congress of 1993, and certainly none except at the rank of genus. Indeed it might well be deleted, with the result that a few conserved generic names would change their authorship and perhaps, being now conserved from an earlier date, some names rejected in their favour could be deleted. However that would require an amendment to the CODE, and no one has proposed it.

2) Although there are (essentially by an oversight) a couple of examples (one mycological – *Agaricus lycoperdoides*) in Appendix IV of the CODE of an epitype being designated simultaneously with a conserved type, under the provisions of Art. 9.18 this is logically tautologous. An epitype can normally be superseded only if the type it supports is superseded but that cannot apply to a conserved type. In that situation only if the epitype itself is lost or destroyed can another be designated and that applies equally to a conserved type – the CODE does not spell this out but another would have to be conserved. It is at least arguable to view as editorial a “correction” to the two entries of epitypes of conserved types so that they are listed as the conserved types. Certainly it is extremely bad practice (one might even say stupid) to select as a type an element that is “demonstrably ambiguous” when there is no restriction whatsoever as to the element that may be chosen (as is the case when a conserved type is being proposed). [I realise that this was not Vincent Demoulin's basis for suggesting epitypification but rather as an alternative to conservation, but that would seem to mean applying the name to a different species if Sipman's taxonomic conclusions are accepted].

3) This Nomenclature Committee for Fungi is to be congratulated on noting the problem presented by the proposed conservation of a species name with an infraspecific basionym, something that several other committees have overlooked. As the CODE stands, *Aspicilia farinosa* may be conserved but *Urceolaria calcarea* var. *farinosa* may not. Hence one has to suppose that conservation of the latter would disconnect it from its basionym, something that would be extremely confusing, not to mention contrary to the intent of the conservation proposal were there any question of the species being transferred to a different genus, because the name in that genus would almost certainly continue to be typified by the type of the basionym.

This is not hypothetical; it is actually the case with *Stipa robusta* (see p. 463 of the VIENNA CODE) Although the current entry in App. IV implies that its basionym, *Stipa viridula* var. *robusta*, has the same conserved type, this is not permissible under Art. 14.1. In fact the species concerned is now usually called *Achnatherum robustum* with the same basionym and so, technically, that name is not conserved with the same type as *Stipa robusta*! There is a proposal to amend the CODE in Melbourne to resolve this, permitting basionyms of conserved names (even if not in the rank of genus or species) to have their types conserved also. As the December TAXON is not published at the time of my writing this, I attach a copy of the proposal. There may be a slightly more elegant way of handling this, but I would think the intent of this proposal is so non-controversial that, one way or another, it will be accepted in Melbourne.

THE SPECIFICS OF THE PROPOSAL TO CONSERVE *ASPICILIA FARINOSA*: I think the Committee should address the case on the assumption that Proposals 243–245 (or some variant thereof) will be accepted in Melbourne, i.e. that by conserving *Aspicilia*

farinosa it will still be possible to cite its authorship as “(Flörke) Flagey” (assuming the basionym is validly published, that is).

I am afraid that I cannot take aboard Harrie Sipman’s concerns that somehow the record of *Urceolaria calcarea* var. *farinosa* Flörke from Berlin will still survive once the name is applied to a different species from that collected by Flörke. This is the situation with every proposal for a conserved type. In all such cases all the original material, or at least that which had been designated as type, will change its name and will no longer bear the name with which it had been associated. Conceptually, this is no different from any record based on a misidentification. It is simply the (to my mind, small) price to pay for avoiding much wider name changes.

It is not for me to get into the merits of the proposal, but if Harrie Sipman’s taxonomic opinions are widely accepted, then it would seem useful for the Committee to include in its report on this proposal (regardless of the outcome) what the names for these three taxa are with and without conservation. Of course, this would be easier if Sipman has published, or is in process of publishing, these taxonomic conclusions. To the outsider there is certainly the suggestion that the taxonomic situation is still not evidently resolved, which makes any conservation proposal open to question – although a rejection proposal perhaps less so!

DEMO 101215: I cannot help much on this proposal, for except for the REV. MYCOL. I have no quick access to the literature involved and hope the lichenologists will settle the matter among themselves. I however must draw attention on the recent paper "Phylogeny and taxonomy of *Aspicilia* and *Megasporaceae* by A. Nordin, S. Savic & L. Tibell, MYCOLOGIA 102(6):1339-1349, 2010". MYCOLOGIA would not be my first choice for a lichenological paper and its editorial and refereeing policy must not be very satisfactory for lichens. The paper is indeed quite unsatisfactory for it is mostly a rRNA (nucLSU and mitSSU) phylogeny, from which, despite the mention in the introduction that "a denser sampling is necessary, particularly from *Aspicilia*" that "it is not possible to draw any firm conclusions about the relationships within the family", a synopsis of accepted genera including new combinations is presented. There is hardly any real phylogenetic or taxonomic information outside the cladogram. In this, Nordin acts as if his proposal (only incidentally mentioned p.1341 for stating that *A. farinosa* is taken in the sense of Nordin and Roux 2009) was accepted and p.1346 the new combination *Lobothallia farinosa* (Flörke) A. Nordin, S. Savic & Tibell is introduced based on the Flörke variety with *Aspicilia farinosa* (Flörke) Hue cited as homotypic synonym. By the way, from this and the original proposal, I guess that in the Flörke description one has a lambda not a k and that Flörke was using Greek letters to designate what one can guess are varieties. This paper, where the sequenced specimen is from southern France, has perhaps been hastily prepared to force us to accept conservation for the southern taxon but does not help in finding the correct way to do it.

On this as well as on other problematic proposals like the *Cortinarius speciosissimus* one I have a consideration, that as chairman I would suggest to Lorelei to put in practice. It is to inform proposers of the problems we have and ask a return from them. We indeed consult occasionally experts outside of the committee and I do not see any reason not to include the proposers in the discussion. In this case for example it would have at least made them aware of the fact Hue was not the first to make the combination in *Aspicilia*.

NORV (SECRETARIAL NOTE) 101215: Re DEMO 101215 (last paragraph)—Nordin & Roux were, in fact, the FIRST to alert us that Flagey, not Hue, had made the first combination in *Aspicilia* (see **NORDIN & ROUX 91006** above; in previous Commentaries these were placed above member comments directly after the summary). Originally, I fully intended to copy all conservation proposal authors to our major mailings (especially when returning ballot results) but ran out of time. Be assured that all proposal authors will now receive both Commentaries 11 & 12; those whose proposals have been decided will be thanked, and I will invite the remaining to comment and note that if comments are received before January 10, they will be shared with all members prior to closure of the Ballot 2011-1 voting period.

PRIN 101216: Re REDH 101112: I do not think that we are dealing with isonyms here.

Does Flagey really make only one combination? He cites different “basionym authors” in different publications. Vincent already pointed out that in Rev. Mycol. 10: 131 (1888) he cites “*Aspicilia farinosa*, Nyl.” and “— *farinosa*, Fl. Hepp.” in subsequent lines of his list. In 1892, he cites “*Aspicilia farinosa* (Flw. L. E. 300). *Pachyospora farinosa* Mass Sched. Crit. 148”. Flotow is only mentioned as editor of an exsiccatum here. Isonyms have to be based on the same type. If we regard Flagey’s different combinations as independent, they are based on different types. It would then be possible to conserve one homonym over the other. Because Massalongo is the first who apparently uses “*farinosa*” for the mediterranean species, and because it would be easy to typify *Pachyospora farinosa* from his material in VER (perhaps even using an exsiccatum) I would still consider this option.

NORV (SECRETARIAL NOTE) **101218:** The original authors, who seek to preserve current usage, propose **(1861a)** to conserve the name *Aspicilia farinosa* (Flörke) Flagey based on a “mediterranean” specimen (now believed to differ from Flörke’s original German taxon) labeled as *Lecanora cinerea* var. *calcarea* (*farinosa* Ach.) [Lich. Paris. 127] by Nylander, who later cited the name as *Lecanora farinosa* (Bull. Soc. Linn. Normandie, ser. 2, 6: 307 (note), 1872).

After considering various alternative proposals that might answer problems raised by the revised 1861, I have placed the following alternative proposals (1861b) on our ballot. **(1861b)**, proposes to conserve the name *Aspicilia farinosa* (Nyl.) Flagey 1872 [NON (Flörke) Flagey 1888] as currently used, conserving as type a specimen cited by its author: the same Nylander specimen cited above.

At this time, I have not placed a third alternate (c) suggested by PRIN 101111—to conserve *Aspicilia farinosa* (Massal.) Flagey over *Aspicilia farinosa* (Flörke) Flagey—as there seems to be some question regarding its type. This option appears to tie *A. farinosa* to earlier, not current, usage of the name. If neither (a) or (b) is recommended, however, this alternative to stabilize the name *Aspicilia farinosa* might also be explored.

BASIONYMS: (a) *Urceolaria calcarea* λ . *farinosa* Flörke (1810) [to me appears tied convincingly to *U. calcarea* and intended as a variety by Flörke, but that should be confirmed by Printzen, Triebel, or another native German speaker]; (b) *Lecanora farinosa* Nyl. (1872) = *Lecanora farinosa cinerea* var. *calcarea* (*farinosa* Ach.) Nyl. (Lich. Paris. #127); (c) *Pachyospora farinosa* Massalongo (1855).

PRIN 110110: In the light of McNeill 101212 neither proposal will solve the problem. I once more like to point to my suggestion to regard Massalongo's description as a that of a new taxon and conserve that name against Flörke's.

DEMO 110115: There has been not enough recent response from lichenologists, including the proposers. Further I disagree with some of John McNeill’s statements. The present wording in 14.9 is not at all a left over from codes previous to the adoption at Sydney of his proposal allowing explicit type conservation. It dates from the same time. Further, the accepted proposal was strictly dealing with the typification of superfluous (Art.63) names. I will try to come to an agreement with John on what we remember of this story but the important thing is that there is not enough lichenologists’ input. Further John raised the possibility Flörke's names were invalid. For once I would have loved to see the PDF of the original publication. Maybe you only sent it to native German speakers, but I am a disciple of Donk, who convinced me that to be a good mycologist, you need to read Latin, French, German, and English. I anyway I live 40 km from the German border and German was my major foreign language in school.

DEMO 110116 (excerpted from message to Sipman, after receiving the Flörke original from Secretary Norvell): John McNeill communicated [to] us the protologue of Flörke... and he raised the question whether the varieties could not be invalid under Art.33.1. This I do not believe; the typography with Greek letters is in my opinion sufficient to indicate those are varieties of what at the bottom of p.119 he calls *Urceolaria contorta*. So why does everybody call this *Urceolaria calcarea* var. *farinosa*, while *calcareo* (*calcaria*) is for Flörke another variety of *contorta*, that happens to be the one above *farinosa* on p. 125?

NORV (SECRETARIAL NOTE) **110120**: This proposal is obviously not close to ready, and I do not have the time to write another alternative at this time. Therefore, I am not placing it in Ballot 2011-2 so as to give Christian, Dagmar, Vincent, and other interested members an opportunity to craft a reworded proposal that we could support; I have also sent a second message to the proposal authors in hopes that they might respond.

If I have not received a coherently rewritten proposal by March 1 for placement on Ballot 2011-3, we shall vote again on the original Prop. 1861. At that time those who do not like that option may choose to vote against it so we may retire it from our discussion list.

→ (GUEST) **Anders Nordin 110207** (responding to a query from the Secretary, having not received the first one, sent in November.): The intention with proposal **1861a** was to stabilize the usage of the name *Aspicilia farinosa* by attaching it to the only well delimited taxon of the possibilities at hand, which also reflected the current use of the name. We were aware of the different possible applications pointed out by Sipman, but we were also aware of the fact that the application of his recently introduced name *Aspicilia substerilis* was far from clearcut (unpublished ITS sequences from the type did not agree with sequences from other collections determined by Sipman, which instead agreed with sequences of *A. calcarea*!). In fact the *calcarea*-group is taxonomically far more complicated than generally assumed.

This is the whole business simplified: Flörke introduced the epithet *farinosa* at subspecific level (*Urceolaria calcarea* var. *farinosa*). No original material has been available until recently, but from the recently discovered material as well as the original description it is clear that the epithet refers to a taxon identical with or closely related to *Aspicilia calcarea*. In 1872 Nylander (Bull. Soc. Linn. Normandie, ser. 2, 6: 307, note) made the valid combination *Lecanora farinosa* (Flörke) Nyl., but the material he explicitly applied the name to (Herb. Lich. Paris, No. 127) is a small-spored species not at all closely related to *A. calcarea*. After the combination into *Aspicilia* (by Flagey) was made, the name was discussed by Hue, who apparently was unaware of Flagey's combination and who chose to apply the name sensu Nylander, although he was aware of the discrepancy between the original but unsettled application of the name and Nylander's clearly defined application (well, that is, defined by the material pointed out by Nylander, but by him only described as a species separate from *L. calcarea*). As this usage seems to have been widely adopted after that, we made this proposal in order to finally settle the matter. Apparently the members of the committee are uncertain whether the code permits this or not.

Since none of the possible solutions put forward by the committee members seems to be without objections, we think we had better withdraw our proposal and instead proceed along ways that make a conservation unnecessary.

As was pointed out by the chairman of the committee (in a surprisingly offensive way), the name was used in the proposed sense for a combination into *Lobothallia* (Mycologia 102: 1346, 2010). One of the reviewers pointed out that a conservation could be avoided if the name was regarded as a new name instead of a combination, but since the proposal was under treatment (and naively enough I thought it would pass without problems) I found it inappropriate to follow this recommendation. But now, when the situation has changed, his advice is well worth considering.

To conclude: herewith we withdraw **proposal 1861a** and apologize for having occupied the time and puzzled the heads of the committee members.

→ **TRIE 110210**: For proposal 1861 I meanwhile checked Arnold, Flora 67, 1884: 409. The "*" indicated there stands for a new combination by Arnold

(the same case as on page 406 for *Lecania sambucina*). Thus this is not a subspecific entity as assumed by Nordin & Roux in their proposal.

So I suggest that this place is the valid recombination on species level, as far as I see that means the following:

Aspicilia farinosa (Flörke) Arnold, Flora 67: 409 (1884)

The basionym is *Urceolaria contorta* var. *farinosa* Flörke, Mag. Neuesten Entdeck. Gesammten Naturk. Ges. Naturf. Freunde Berlin : 125 (1810). I carefully read the German text.

My current view concerning the publication of names in this case is documented in LIAS names <http://liasnames.lias.net/> under *Aspicilia farinosa* and *Pachyospora farinosa*.

Thus, perhaps we could manage to have two valid "*farinosa*" strings by selecting an appropriate epitype for the Flörke material (German material) and study the Massalongo type in VER? A new combination *Lobothallia farinosa* (or whatever) might be based on *Pachyospora calcarea* var. *farinosa* A. Massal., if the mediterranean taxon is concerned.

→ **DEMO 110210:** If the [*Aspicilia farinosa*] proposal is withdrawn, it is withdrawn. It however hopefully should be reintroduced (Sipman may be in the best position to do it) for I cannot understand how Nordin could keep using the name in the sense he has done without conservation. If something is a new name (what by the way?, this confirms me Mycologia does not use the best adequate referees, at least for lichens) the type cannot change (7.3). One should note Sipman (this might have been a mail only sent to me) agreed I did see correctly that Flörke had *Urceolaria contorta* var. *calcarea* and var. *farinosa* and not *U. calcarea* var. *farinosa*, as himself, Nordin and others had assumed. I support typifying by conservation with the Nylander specimen as proposed in 1861. The problem is how to cite the name. I maintain that the end of 14.9 is not a leftover and might provide the most clearcut solution. The fact one deals with a variety at the beginning is of course a complication but I would not object to have *Lecanora farinosa* Nyl., T. Cons. implying the link to Flörke is severed.

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

BALLOT 2010-2 RESULTS: 8 yes : 1 no : 2 abstain : 3 discuss

BALLOT 2011-1 RESULTS: 13 yes : 0 no : 0 abstain : 1 discuss.

93% recommend Prop. 1888, but **retained** for discussion at the request of Demoulin.

PROP. 1888a placed on BALLOT 2011-2 as an alternate proposal that *Glomus* serve as a voted example as an instance where the neuter gender is retained.

→ BALLOT 2011-2 RESULTS: **10** yes : **2** no : **1** abstain : **0** discuss. [1 ballot not returned]

77% returning Ballot 2011-2, or 71% of the full NCF, voted yes on the proposal. The Secretary will report that while 93% originally recommended conservation of *Glomus* as a neuter noun as proposed by Kuyper, a strong majority prefers that *Glomus* serve as a voted example in the Code. This will disappear from Commentary 15.

Background 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 & Guest 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-Linnaean (or pre-something else) tradition. That would mean that Kuyper has not misunderstood the CODE and that the proposal makes sense, wouldn’t it?

DEMO 101012: PRIN 91223 drew my attention on the strange sentence that “*Eucalyptus* lacked a botanical tradition”. I could not get an explanation yet and would have to contact P. Silva who authored the proposal accepted at Tokyo, which included the sentence. This was supposed to be editorial but I am sure the intent of the code had never been to deny a botanical tradition to *Eucalyptus*. I did not find comments in the documents of the Editorial committee which concentrated on other parts of the Silva proposal. Comparing the Berlin and Tokyo codes, I think that what Silva should have proposed is “*Eucalyptus*, for which botanical tradition is inconsistent, retains the feminine gender assigned by its author”. Only *Eucalyptus* specialists (or Tom May?) could tell us whether this is correct. If one reads Silva proposal (Taxon 42:189-190,1993) one sees his suggestion for botanical tradition is “a consensus of usage during an unspecified period, perhaps extending only until the middle of the last century”. The idea of T. Kuyper to restrict botanical tradition to pre-Linnaean tradition is to me totally new in my 35 years on the Ed. Com.

REDH 101105: *Eucalyptus* is another matter. We are free to decide what is best for *Glomus* for mycology. Please vote yes or no and then we can decide to have more discussion if the vote is split.

DEMO 101106: There is I think no doubt mycologists want to use the neuter for *Glomus* (in this kind of issue it might however be good to have a vote on that point and not on the proposal). The question is whether the proposal is technically necessary and this is why the *Eucalyptus* example is fully relevant. As pointed out by PRIN91223 it does not seem worded in agreement with the intent of the article. The question is whether constant usage in the last twenty-eight years can be interpreted as botanical tradition. This is a matter of general principle not peculiar to mycology. The technically best way to handle this is in my opinion to have a vote on whether we want *Glomus* neuter or masculine and then refer the issue to the general committee. Assuming we want the neuter the question would be, “Are 28 years of usage and respect of classical gender sufficient to overcome the original authors gender?” If yes, could the case be closed or should this be a voted example? If the answer is no, we must of course proceed with the proposal.

PRIN 101111: Re DEMO101106 and REDH101105—I do not think we are free to decide what is best for mycology. We can only vote on proposals that have been published. Kuyper invokes Art. 62, which says “the gender assigned by botanical tradition,

irrespective of classical usage or the author's original usage". The question I raised was: what is meant by "botanical tradition"? I have thought about it and think, the next sentence gives the clue. "A generic name without a tradition RETAINS the gender assigned by its author." I can only imagine that this means "tradition before the author assigned the name." It cannot really mean "tradition after the name was published by author X". This would explain the sentence on *Eucalyptus*. The old Greeks and Romans did not know the genus, so it had no tradition, before L'Héritier described it. This would also apply to *Glomus*, and hence the name should be treated as masculine (the gender assigned by its author), were it not for Kuyper's proposal. I believe the proposal is technically necessary and support it.

MAY 101115: I am voting for more discussion so we can clarify [whether] this can be dealt with as an example, rather than by conservation. If the specific question was asked whether the gender should be neuter or masculine, I would vote for neuter, because that is now well established. The key point is what exactly is 'Botanical tradition'. The wording of the last sentence under Note 1 to Art. 62.1 implies that botanical tradition can be after Linnaeus [*Phyteuma* L. (n), *Sicyos* L. (m), and *Erigeron* L. (m) are other names for which botanical tradition has reestablished the classical gender despite another choice by Linnaeus."] – but nowhere is it clear how much later. If we were being asked right now to decide on the gender (and Trappe had not changed it), then we would have to say that the masculine (as by the original author) is correct. The example would need to be carefully worded, so as not to imply that if users disregard the Code for long enough then this becomes botanical tradition.

For *Eucalyptus*, perhaps the intent was '*Eucalyptus*, which could be interpreted as masculine from its termination, lacked an existing botanical tradition when introduced, and therefore retains the feminine gender assigned by its author'. There does not seem to have been any inconsistency about the gender of the genus – it was introduced as feminine by L'Héritier in 1789 (with type *Eucalyptus obliqua*). The only possible source of confusion is that there is a well-known species *Eucalyptus globulus* (Tasmanian Blue Gum) that was described in the genus not long after it was first introduced (by Labill. in 1800). The epithet *globulus* is a substantive, and hence the ending is not altered to agree with the gender of the genus. This does trip people up from time to time, but it is more a case of the occasional incorrect use of *Eucalyptus 'globula'* (such as in FLORA OF CHINA 13: 321–328. 2007) rather than the whole genus being considered masculine on the basis of this one species. So, I don't think Vincent is right in his conjecture that the *Eucalyptus* example was included because 'botanical tradition is inconsistent' on its gender. For *Glomus*, it comes back to knowing exactly what is meant by botanical tradition.

REDH 101208: I continue to support the proposal especially after reading Christian's comment (PRIN 101111), but I also think there may be merit in having the General Committee look closer at what we in the Committee for Fungi recommend, as suggested by Vincent (DEMO 101106).

DEMO 101216: We are not theologians trying to interpret God's intentions in dictating a sacred book. The CODE has been written by scientists for scientists, hopefully to help them. If its wording is not clear it is essential to compare former editions and study synopsis of proposals published in TAXON and reports of the Nomenclature session published in ENGLERA. What counts is the intent of the session that accepted a modification. If the wording does not reflect that intent, it should be corrected.

I have tried to reconstruct the history of Art. 62, without, however, going attentively through the 5cm thick pile of documents of the orthography committee. This is something I wished I never would have to go through again, so painful it had been, but apparently is necessary.

Gender of generic name was an article (already 76) in the Cambridge rules but later became Re.75A before the Orthography Committee (of which I was Secretary) proposed to restore its status at Berlin [V Demoulin & Nicolson, (171-198) in Report of the Committee for Orthography with proposals to amend the CODE, TAXON 35(4): 794-803, 1986]. The Committee also proposed a rewording of the first part, which was one of the few proposals almost unanimously supported (7-1). The committee report was clear that "tradition" was used as equivalent to "usage". "Tradition" may

have been preferred in the text of the article because it conveyed more the idea of extension through time. The idea, anyway, was to oppose botanical tradition/usage to classical tradition/usage and reverse the Cambridge rule that classical tradition took precedence. In the Sydney CODE the expression was "general usage" and I do not remember anybody wanting to deviate from that. The reason "general usage" was not maintained was perhaps to make clear that it was the usage by botanists that counted. After all, the best wording might have been "general botanical usage". The only person who seemed to have wanted to refer to non-classical (but prior to valid publication) usage was Parkinson, whose proposal was not accepted and it is anyway not evident from his proposal, which itself used "botanical custom".

The committee proposal was well received by the Rapporteurs (Greuter & McNeill, TAXON 36(1): 250, 1987) and accepted at the Congress with Greuter's recommendation and comment that "The new text was perfectly clear, easy to understand, and listed the main examples. The Committee on Orthography had done a good job" (ENGLERA 9: 192-193, 1989). I have no trace of problems before and at the meeting of the Editorial Committee and the Berlin CODE follows well the accepted proposal. After the meeting there were, however, two adverse A POSTERIORI reactions. One by Voss, who before had only discussed the gender of *Lotus* and *Melilotus*, found that "neologism" in the *Eucalyptus* example was inadequate (I still wonder if this is true and how, if so, it had escaped the attention of everybody, including Larry Johnson, the best linguist among botanists who had proposed the example) and a virulent one by Paul Silva (p.3 of his letter of Jan.16 1988).

His late comments having not been taken into account, Silva presented them again as proposals for Tokyo (TAXON 42(1): 185-190, 1993, prop.319, pp.189-190). There he made the comment that "Unfortunately, botanical tradition is not defined, but the implication is that it is a consensus of usage during an unspecified period, perhaps extending only until the middle of the last century." The rapporteurs' comments (p.268 of the same TAXON) were that it could be referred to the Editorial Committee, which was done without discussion at the Congress (ENGLERA 14: 215, 1994). The Silva wording included the present rewording of the *Eucalyptus* example, rewording that was probably accepted because it got rid of "neologism," to which Voss had objected. How I failed to see it was in contradiction with what has always been the intent of the rule and did not oppose it might seem strange. One must, however, be aware that I devoted so much time and energy to orthography, with so little success, except for this Art. 62, that I did not want to deal with orthography anymore. The rapporteurs had said the Silva proposal was editorial, so why bother? Anyway the intent of the rule is made clear by the last voted examples, as noticed by MAY 101115, that states that *Sicyos* and *Erigeron* are other names for which botanical tradition has reestablished the classical gender, despite another choice by Linnaeus. This makes it clear that botanical tradition does not mean tradition before the valid publication, but general usage.

I have contacted P. Silva to try to understand why he proposed his rewording of the *Eucalyptus* example. It is clear now that he wanted to phrase the article so that it was stated first that "botanical tradition" was the decisive argument and that his "proposal dealt only with the logic of the syntax, in particular an attempt to bring the wording of the examples in accord with the wording of the rule". As one may see from the proposal, his concept of botanical tradition was not at all the interpretation by Kuyper, that this refers to use before valid publication, but is closer but not identical with that of the Orthography Committee. Silva had no special knowledge of the *Eucalyptus* situation and just thought he could rephrase the article the way he did. The *Eucalyptus* example was suggested by L. Johnson, director of the Sydney Botanical garden and specialist of *Eucalyptus*, with comments too lengthy to reproduce here. Those almost concur with MAY 101115, that is, that there is a strong tradition of feminine gender, but diverge on a point Tom is unaware of. It is that in the eighties, the masculine had been taken up in the "(bad) treatment (not by a professional taxonomist, though an old professor of mine) in FLORA EUROPAEA". At the time we were thus anxious to have *Eucalyptus*, a genus as dear to the heart of Australians as *Acacia*, listed in the CODE, for fear that the FLORA EUROPAEA use would spread. Silva failed "to see the problem with either the Berlin wording or the Tokyo

wording, both of which affirm that the name *Eucalyptus* is feminine in gender". He overlooks that even if *Eucalyptus* is not endangered, the example as he worded it, leads to useless proposals like that on *Glomus*.

One must be aware that the possibility to conserve gender of generic names was introduced at Berlin, through a proposal by Voss, who wanted to make sure he could conserve the genus of *Lotus* even if this was in contradiction to Art. 76 (now 62) as proposed by the Orthography Committee. The rapporteur general commented on Voss proposal's (see ENGLERA 9: 192-194, 1989) that "It was not really necessary to go through conservation procedures to conserve gender of generic names; it was equally possible that the Section, meeting every six years, did rule on proposals to add other generic names to the "voted examples" included in the new Art. 76. The time delay would be the same, and the Committee work would be less. He did not speak strongly against the proposal, but did not think it was absolutely essential." Seeing the present situation, I wish he had spoken strongly against!

In conclusion I strongly urge, as Chair, that we take a vote on what gender we want for *Glomus* (neuter I guess) and let the General Committee decide whether this should proceed through conservation or inclusion in voted examples.

DEMO 110115: You must have made a mistake in the ballot for one sees a 188a and no b. I suggested you split the vote on whether we wish the neuter gender (I do) and how to have it, my preference being to leave to the GC to decide whether it should be a voted example (what I think it should be) or a conservation. If a majority votes for conservation of neuter we may at least consider we agree on the gender and still leave the GC decide on the way to do it.

NORV (SECRETARIAL NOTE) 110201: For the time being I have retained this discussion, although I believe no further discussion is needed. To that end, I have placed auxiliary proposal on the ballot that *Glomus* be recommended to the General (or Editorial) Committee as a voted example, as suggested by Vincent, rather than serve as a name conserved with as 'neuter'. I agree that it makes little sense for us to continue to vote on orthographic "conservation" proposals, particularly in view of the controversy surrounding the rhacodes/rachodes questions (1927).

→ **DEMO 110210:** ... on 16th dec I wrote ... "In conclusion I strongly urge, as president, to take a vote on what gender we want for *Glomus* (neuter I guess). and let the General Committee decide whether this should proceed through conservation or inclusion in voted examples." [The Secretary] did not take this into account, thus people expressed their wish to have the neuter gender by voting for the conservation proposal. If one uses conservation this means one considers the neuter gender is contrary to the code. If one uses the voted example one assumes it is in agreement with the code but that the issue is delicate enough for it to be included in the voted examples. This is the situation I favor given my past involvement with Art. 62. I anyway consider that the role of a Committee on a taxonomic group is to give the General Committee an opinion on the favored use of a name. It is not to judge the work of the Editorial committee on an issue that is not specific to its group (all the problem stemmed from the wording of the *Eucalyptus* example).

→ **PENN 110210:** As long as *Glomus* is confirmed as neuter, I am happy with either mechanism. However, a voted example seems a less ponderous mechanism, than conservation.

PROP. 1927(a), Proposal to conserve the name *Agaricus rachodes* (*Basidiomycota*) with that spelling. [This would conserve Vittadini's original orthography.] Proposed by Else C. Vellinga & Shaun R. Pennycook. TAXON 59(2): 644 (2010).

BALLOT 2010-2 RESULTS: 7 yes : 6 no : 0 abstain : 1 discuss

Two proposals, (1927a) and (1927b) appear on BALLOT 2011-1, due 15 January 2011
See NORV (SECRETARIAL NOTE) 101218 for a discussion of (1927b).

BALLOT 2011-1 RESULTS: 7 yes : 7 no : 0 abstain : 0 discuss.

→ TO BE INCLUDED ON BALLOT 2011-3, to be sent out on March 1 2011

PROP. 1927(b), Proposal to conserve the name *Agaricus rhacodes* (*Basidiomycota*) with that spelling. [This would conserve the currently more common orthography.] Developed in NCF Committee, 2009–2010.

BALLOT 2011-1 RESULTS: 6 yes : 8 no : 0 abstain : 0 discuss.

Background 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

Guests commenting on Prop. (1927) include WALTER GAMS [former NCF member, Baarn, Netherlands; 100820, 101108].

Committee & Guest 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.

(GUEST) **Walter Gams 100820:** Because either way is open for conservation, I would rather have preferred the other choice of conserving the spelling *rhacodes*, which is linguistically plausible and still has a slight majority of usage.

DEMO 101012: I am in total disagreement with this proposal. In it and in the supporting paper (TAXON 59: 623-627, 2009), it is assumed on p.625 that transliteration is not a prescriptive process and is not regulated by the code. Having published several papers on orthography in TAXON since 1981 (30:233-248) and having convened for years a committee on orthography, I absolutely disagree. It is true the code does not impose to choose among possible competing transliterations. A transliteration that does not fit any existing tradition is, however, a correctable orthographic error. It is true one may accept *Raphanus* beside *Rhamnus* and *racodes* would not be correctable to *rhacodes*. *Rachodes* cannot, however, be otherwise than an inadvertent error for *rhacodes*. To pretend the name could come from *rachos*, a rare word for a spiny bush is absurd, and Vellinga herself wrote in 2001 (Fl. Agaric. Neerlandica V:72) that *rachodes* was without obvious meaning. The suggestion it could be related to the habitat is particularly impossible, the suffix *-odes* meaning “looking like”. The fact Vittadini did not specify the etymology of his name is irrelevant, since to anyone who understands Latin and Greek the only possibility is as universally accepted in the 20th century, that one refers to *rhacos*. This is of course what Fries, whose knowledge of classical languages was excellent, understood and why he made the correction *rhacodes*. That he was not constant with it is unfortunate but does not change the fact it [his correction] was well founded. To me *rhacodes* is an orthographic correction to be accepted under 60.1. I would thus consider the

proposal superfluous and would vote against for that reason. Worst however I could have sympathized with a proposal that in case my interpretation was not accepted would stabilise the most frequently used spelling "*rhacodes*" It is in total contradiction to propose for conservation a spelling that represents a minority usage with the astonishing claim to thus make it more familiar. If doubt has arisen and conservation is necessary, then it is the most used spelling *rhacodes* that should be conserved.

NORV 101012: Unfortunately, Walter Gams and DEMO101012 raise a number of very good arguments for not selecting 'rachodes' as the species epithet. I say unfortunately because I was looking forward to having this particular situation 'solved' without controversy. I have voted for the proposal on the preliminary ballot, but now would like others to comment regarding the fact that *rhacodes* is by far now the most commonly used epithet (with 48 100 immediate 'hits' on Google, including Wikipedia, vs. 7 160 immediate 'hits' for *rachodes*) and the likelihood of the balance turning once Wikipedia, MycoBank, and IndexFungorum accept the conservation of 'rachodes'.

REDH 101027: I knew this would be a controversial proposal, but a necessary one. It should perhaps have been presented as two proposals, one with one spelling, and the other with the alternative spelling. This issue needs to be officially decided.

With due respect to the statements by Demoulin and his obvious expertise on linguistics, it was successfully argued that the origin of the original spelling is ambiguous. The authors cite Art. 60.1 and Art. 60 Ex 1 in their case in support of retaining the original spelling. Demoulin stated, "It is true the code does not impose to choose among possible competing transliterations. A transliteration that does not fit any existing tradition is, however, a correctable orthographic error." However, I fail to see which article paragraph he is using to support this position. His statement would seem to be an opinion, one of several people's opinions that are debatable. Gams had in earlier email exchanges expressed the opinion that the origin of the name was ambiguous. Unfortunately, he did not (from my recollection and saved emails) express a preference that earlier would have been helpful. The debate could go on for another century in the absence of a ruling. Merely rejecting the current proposal will do a disservice to this Committee if mycologists see how ineffective we are in resolving problems.

The authors chose to invoke Art. 14.11 that would normally be used to conserve a non-original spelling but in this case it is to solidify the original spelling against changes based on differing opinions such as Demoulin's. I propose that we put to vote a proposal to conserve the alternative spelling (under Art. 14.11) if this current proposal fails. For now I support the proposal. What I do not want to see is it failing and the alternative not conserved. A report by this Committee will be buried in time and does not have the same effect as a successful conservation.

Let us make a decision and please conserve one or the other spelling and not leave it to opinion, no matter how scholarly. Even E. Fries wasn't consistent.

DEMO 101104: Scott disregards all my argumentation to demonstrate that the origin of the spelling *rachodes* is not ambiguous. It is strange but not ambiguous. Something is ambiguous if it is not clear which of two possibilities was meant. This would have been the case if an origin based on *rachos* would have been possible. I maintain this is absurd. He also does not seem to understand what an orthographical error is. Why does he look for an article defining an orthographical error? This does not exist anymore than articles defining other general notions, which are not special to nomenclature. The important thing is that 60.1 provides for the correction of orthographical errors. I repeat that a transliteration that does not fit any existing tradition is an orthographic error. I wonder who could argue the contrary. I agree with Scott that, even if rejecting the proposal would be enough, as it has been in the past to make clear *rhacodes* is the correct spelling; it might do no harm to have it as a voted example similar to Ex.5 (*Gluta renghas*). The voted example is the best technique in such a case. Prop. 1927 is inadequate because it assumes the original spelling is correct and not to be altered, in which case conservation is not needed. Proposing for conservation the alternative is also from my standpoint inadequate since I consider it the correct one. It thus might be best to vote on which spelling we

want to be used (and Lorelei gives us more arguments for *rhacodes*) and then see how to make it secure. [SECRETARIAL NOTE: Vincent noted the day after sending in his comment that ‘not’ had been omitted from the first sentence, which has now been corrected with the not shown in bold and highlighted in green.]

REDH 101105: I am glad that Vincent at least agrees that one way or another this case should be decided by this committee rather than let the situation languish another century. I have great respect for Vincent’s command and understanding of languages. However, exactly how absurd is it to ask for a specific article to back up a claim? If it were an absolute clear orthographical error then I would have expected other scholars (Walter Gams, Elias Fries) to have consistently noted it. There has been a long history of flip flopping on this epithet by many scholarly people. Vincent’s statement is merely one voice and one opinion. We should decide this matter. The example of an example (Art. 60, Ex. 5) that he gave is hardly covers the same situation as it was a case of correcting an error involving the first letter. If Vincent wishes to use a voted example he should provide us with the wording and logic within the wording of the example. Note the definition of “voted Example” as a footnote to Ex. 10 in Art. 7 (p. 11) in the Code.

DEMO 101106: There are strange things in REDH 101105. I will not comment everything but note that GAMS 100820 writes of *rhacodes* it is linguistically possible, which implies *rachodes* is not, and hence the latter must be an orthographic error. As to Fries he was probably the first to correct the mistake but he did not do it consistently. The important thing is that he did it at a time the name had recently been published. The philosophy of having this as a voted example is evident, it is the existence of a controversy. As for *Glomus* the clear way would be to decide what spelling is desired (I hope the traditional european one which is by far the most used) and then ask the Gen. Comm. how this should be secured. If I had to word a voted example, it could be "*Agaricus rachodes* Vitt. is a meaningless word that must be an orthographical or typographical mistake for *A. rhacodes*, this corrected spelling is thus to be adopted". As anybody who opens the code can judge, this is perfectly parallel to Ex.5.

REDH 101108: Vincent (DEMO 101106) draws conclusions that I would never consider, such as the statement “...that GAMS 100820 writes of *rhacodes* it is linguistically possible, which implies *rachodes* is not, and hence the latter must be an orthographic error”. I think this overstates what Walter Gams intended, and Walter should speak for himself, hence I asked him (see below, GAMS 101108).

I would be interested to hear what others think of the suggested wording of a voted example as drafted by Vincent (in the absence of comments by Nov. 15th, if the current proposal fails, perhaps to wait for another cycle): "*Agaricus rachodes* Vitt. is a meaningless word that must be an orthographical or typographical mistake for *A. rhacodes*, this corrected spelling is thus to be adopted."

(GUEST) **Gams 101108** (in response to Scott Redhead’s request for his current opinion) on 1927): In this case I am still with Vincent. Else Vellinga unearthed the original spelling *rachodes* and therefore continues to defend it. Since [someone], I do not know who, corrected this spelling according to his Greek knowledge (*rhakos* = ruggy material) correctly into *rhacodes*, I certainly would favour this, still more commonly used, spelling. As it is a case for conservation anyhow, I would have favoured this form.

PRIN 101111: I find Vincent's argumentation conclusive. If one argues that "*rachodes*" is correct the proposal is unnecessary. Why should one conserve a spelling that is correct and applied in the original description? For the sake of stability, why should one conserve a spelling that has not been used by the majority of authors?

Because "*rachodes*" does not seem to make any sense, I guess we must see it as an orthographical error to be corrected to "*rhacodes*". I also support Vincent's suggestion to include it in the Code as a voted example.

PENN 101114: I am disappointed at the style of many of the comments opposing this Proposal, which seem to be little more than bald assertions of entrenched dogma: “We know what the etymology is; we know the correct spelling of it; therefore the

original spelling requires orthographic correction.” To recap briefly: (1) the etymology is unknown, never indicated in the protologue, nor by contemporary authors — the derivation from Greek *ρακος* is an assumption (albeit a plausible one); but plausibility is not a proof, and alternative assumptions are available; (2) even if the *ρακος* assumption is accepted, its transliteration has no absolute “correct spelling” — transliterations from non-latin alphabets are matters of convention, with numerous orthographic variants extant [see, for example, Special Recommendation CF_1 in this ballot] — ICBN Rec. 60A recommends (but does not demand) that new names should be transliterated in conformity with “classical usage”, but ICBN does not define failure to follow transliterational conventions as an orthographic error [and see also Art. 60 Ex. 1]; (3) the original orthography was not “discovered” by the proposers — it has remained in frequent use throughout the 175+ year history of the name, and is in current use in many parts of the world (including some parts of Europe, e.g. Italy).

MAY 101115: While a very literal reading of Art 14.11 (and the Note 1 to Art 60.1) could allow conservation of an original spelling that is considered correct—from a quick look through Appendices III and IV, I could not find any such cases. In any case, I do not agree that 'rachodes' makes sense. I prefer 'rhacodes', which as Walter Gams points out is linguistically plausible (from *ρακωδης*) while the derivation proposed by [Vellinga &] Pennycook from *ραχος* is not plausible due to the -odes termination (as used in *petalodes*, *nephrodes* etc. which are all to do with resemblance, not to location). I only used 'rachodes' in my Australian checklist because that seemed technically correct at the time (being the original spelling) without going into the etymology. In addition, I don't consider that the reference to Art 60.3 about changes affecting the first syllable to be made using 'reserve' carries much weight when the changed form has been in use for so long, and is the most currently used spelling. So, I vote for more discussion, to allow an alternative solution to be reached.

The long running confusion about this name reminds me of *Battarrea* (original spelling *Batarrea*). An attempt was made to conserve what would have been a more linguistically correct spelling (*Battarraea*) according to the recommendations of the Code if the name was being newly introduced. This proposal was not successful (therefore only the correction of the double t in accord with the name of Battarra is OK, but not the changed termination), and hence there remains confusion as to what is the correct spelling because there is no mention in the Appendices of the Code to the correct name. In Google there are 14,500 hits for *Battarrea* (correct) but still almost as many hits (>11,000) for alternate spellings such as *Battarraea*, *Batarrea* and *Battarea*. The lesson here is that it would not be good to reject this proposal, on the assumption that *rhacodes* is an allowable orthographic correction, without getting some mention into the Code. The voted example proposed by Vincent seems the best way. There are actually very few conserved orthographies for specific epithets, so it is not a situation that comes up very much, and so by including an example it is not as though we will be having long lists of voted examples. The example is specifically that there is not an obvious etymology for the original name, but there is a plausible correction.

A tangential issue is that the Australian taxon *Chlorophyllum nothorachodes* Vellinga & Lepp was introduced in 2003 (Austral. Syst. Bot. 16: 363) using the 'rachodes' spelling. This species has only been reported from the type collection, but nevertheless, it would be best to have both *Chlorophyllum rachodes* and *C. nothorachodes* using the same spelling for the 'rachodes/rhacodes' element of the epithets. I assume that if we ultimately agree on *rhacodes* as being an orthographic correction, then *Chlorophyllum nothorachodes* would be correctable to *C. nothorhacodes*.

REDH 101208: I continue to feel very strongly that one way or another one of the spellings be conserved and not leave it to a presumed but obviously debatable correction. We should vote on two options and the option that receives the most support should then be put forward again in the hopes that we all will then support it to end the confusion.

NORV (SECRETARIAL NOTE) 101208: I agree and have placed BOTH alternatives on our Ballot.

DEMO 101216: GAMS 101108, PRIN 101111, MAY 101115 say the truth better than I could. Let us get rid of this bad proposal and I will propose to the Editorial Committee to include *Agaricus rhacodes* as an example of correction.

NORV (SECRETARIAL NOTE) 101218: In view of the two strongly opposing sets of comments surrounding the original proposal, I present two alternatives on Ballot 2011-1. Vellinga & Pennycook (**1927a**) propose to conserve for the name *Agaricus rachodes* the spelling 'rachodes' as written by Vittadini throughout his original publication; the NCF alternate (**1927b**) proposes to conserve the same name with the spelling 'rhacodes', as more 'linguistically plausible' and derived from the Greek.

As there are few new points to be made in discussion and there is (however remote) the possibility that one or the other alternate may receive the requisite 9 votes to remove this item from discussion, I ask everyone to mark their ballots on both 1927a and 1927b.

NORV 101218: Having carefully considered all the arguments, I realize I am consistently troubled by the belief that it is possible to decree what constitutes 'correctable' orthography, particularly when each Congress is flooded by so many orthography proposals that attempt to cover all aspects of 'correctability' in the CODE. Vincent (Prop. 1888, DEMO 101216) has provided a passionate exposition of the problems inherent in attempting to have to Code cover spelling adequately.

I favor correcting spellings where there is a clear etymological trail, but in this case, there are too many inferences for me to feel comfortable in changing the original spelling, particularly in view of the proposers' persuasive arguments persuasive. Therefore I support the original proposal (1927a) and will vote "no" on our alternate, 1927b.

PRIN 110110: [voted for 'rhacodes'] I would prefer to include it as a voted example.

DEMO 110115: I again would have preferred to separate what people want and how to obtain it, but since you provided two options, I voted, even if I consider this should be a voted example if rhacodes wins and only a conservation if it is rachodes.

NORV (SECRETARIAL NOTE) 110130: As the Committee is very obviously evenly divided regarding preference for one or the other spelling, I doubt that we will ever reach consensus on which of the two spellings to conserve. I agree with PRIN 110110 that this might serve as another good example for the General /Editorial Committee to consider.

For the time being, I withhold (1927) from Ballot 2011-2 but will place it on Ballot 2011-3 (due to be sent on March 1) so as to give time for the original proposers and members to craft an alternate proposal that might more likely to receive support by 60% of our members.

(By the way, I still favor 'rachodes'!)

→ **DEMO 110210:** I strongly disagree with continuing discussion. Everything has been said and it is clear we are evenly divided and have lost enough time. Let the GC take its responsibility and choose between correct orthography and majority usage on one side and respect of original spelling on the other. As to conservation or voted example, the situation is the same as with *Glomus*.

→ **PROP. (1989),** Proposal to conserve the name *Botrytis brongniartii* (*Beauveria brongniartii*) with a conserved type (*Ascomycota*). Proposed by Andrew M. Minnis, Stephen A. Rehner & Richard A. Humber. TAXON 60(1): 233. (2011)

TO BE INCLUDED ON BALLOT 2011-3, to be sent out on March 1 2011

Summary of Prop. (1989)

When Saccardo (1892) introduced the name *Botrytis brongniartii* for a pathogen of the desert locust, he "cited no specimens, provided no illustrations, and his diagnosis was based apparently on a previous work by Brongniart (...1891)" but he did refer the name to a fungus with ellipsoid or oval conidia. Brongniart also provided no illustrations, and Minnis & al. are unaware of any existing type or original material that might serve as a lectotype. Their recent multigene phylogenetic study of *Beauveria* (to which Petch

transferred the species in 1926) revealed that the species currently represented by the name does not infect *Orthoptera* nor does it produce the same shaped conidia as originally described, "...problematic since the current widely used and economically important application of *Beauveria brongniartii* as a primarily beetle-pathogenic species conflicts with the original circumscription." Under Art. 9.17, the proposers cannot designate a neotype using a preferred element as it seriously conflicts with the protologue (and probably represents a different species.) They thus propose to "conserve *Botrytis brongniartii* with a conserved type (Art. 14.9), using an element that otherwise would be in conflict with the protologue and is different from that determined by application of the CODE."

Committee & Guest comments on Prop. (1989)

Part 2: Proposals to amend the code

SECRETARIAL NOTE 110214: Commentary 14 subdivides the proposed amendments into two— **(2-A)**, covering proposals not related to Art. 59 [i.e., (19–20), (138, new!), (220–221), (223–232)] and **(2-B)**, covering ONLY Art. 59 related amendments [i.e. (173), (298, 300, 303), (307–313)] proposals. For discussions of our previously recommended or rejected proposals, please refer to Commentaries 12 and 13. This entire section will be removed from Commentary 15.

The proposals below are presented according to their newly assigned numbers.

2-A: NON-ART. 59 ASSOCIATED PROPOSALS

Art. 7, Prop. I (224) would amend Art. 7.8 to read “Typification, termed sanctiotypification, of names lacking a holotype adopted in one of the works specified in Art. 13.1(d), and thereby sanctioned (Art. 15), may be effected based on any element associated with the name in that work” [(223–232) proposed by Redhead & al. TAXON 59(6): 1911–1913. 2010]

BALLOT 2011-1 RESULTS (on 224–232 together): 8 yes : 3 no : 3 abstain

➔ BALLOT 2011-2 RESULTS: **9** yes : **3** no : **1(2)** abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

69% returning Ballot 2011-2, or 64% of the full NCF—a percentage that does not change when combining the vote from the non-responding member’s 2011-1 ballot, voted yes on the proposal. The Secretary will report that a 64% majority supports Art. 7, Prop. I.

Committee & guest comments on Art. 7, Prop. I (224)

NORV 101116: ... Although the coining of a new ‘type’ term might strike some as unnecessarily pedantic, remember that most names sanctioned by Fries or Persoon were originally proposed in works written well over 200 years ago and that many types may now be lost or useless. Clarifying the process for selecting a type from the original or sanctioning works is needed.

I should note that only after final submission did we receive the following observation regarding the use of the terms ‘sanctiotype’ and ‘sanctiotypification’ from Walter Gams, who wrote Scott, “I have a very strong preference for the [‘sanctotype’]. Latin *sanctus* (gen. *sancti*) is ‘holy,’ *sanctio* (gen. *sanctionis*) is ‘the act of making it holy’. Thus properly speaking you should call the new thing a sanctionitype, if you so wish. But sanctotype and sanctotypification is perfectly sufficient.”

It is slightly ironic that when Scott first developed this proposal, we originally used the term ‘sanctotype’, which I still prefer. Gams etymological note arrived after the proposals were in the galley stage, however, and so ‘sanctiotype’ will appear in the published proposal. Nonetheless, we will recommend the ‘sancto-’ spelling, should the Committee prefer that spelling and recommend Props 224–232.

PRIN 101216: I would also support the second alternative (224). A small pedantic comment: Shouldn’t it read: “Typification, termed sanctotypification, of names lacking a holotype AND adopted in one of the works specified in Art. 13.1(d), ...”?

DEMO 101216: I am extremely surprised by the way those proposals appear. I understood in IMC 9 that despite the fact the official deadline for submitting proposals was due, it still could be possible to submit some that would result from ongoing discussions, especially those at the congress. I thus hurried to prepare Prop.185-190, that related to issues discussed in the Committee and at the Congress, and was glad to make it for what I thought would be the last possible issue, that is the October one. Those proposals still in press refer to a problem that was not discussed at the Congress and had not been discussed in the Committee for almost a quarter of a century. In the eighties the issue of the typification of sanctioned names was a highly divisive one. We thought the conflict was settled by adopting at Berlin what is now Art. 7.8. This was a compromise allowing those like me who wanted to typify primarily according to the original author and those like Singer who wanted to typify

according to the sanctioning author, to do whatever they want. Like any compromise it is liable to be criticized by utopists but has served us well during 23 years. So where is the urgency? The matter is delicate and people need time to see what benefit there is in the new concept. Especially it is necessary to have at least two rounds of discussions and I formally request more discussion.

NORV 101216: Time of publication by TAXON is beside the point and has nothing to do the merits of a proposal. In fact, our proposals, in preparation for over two years, are hardly an exception, as they are among the 135 new proposals to appear in the current TAXON. All these proposals are currently available from the TAXON Ingenta site for free download.

MAY 101217: I am in favour of clarifying typification of names in sanctioning works by introducing and carefully defining the term 'sanctiotypification', rather than deleting Art. 7.8. Scott, Lorelei, and Shaun have done a great job sifting through all the articles that are affected by their proposed changes to ensure clear application of sanctiotypification.

For definition of 'protologue' in relation to sanctioning there has indeed been a lot of confusion. Even though Korf (1983) treated the 'protologue of the original author' as independent from the treatment in the sanctioning work, he nevertheless (Korf 1982: MYCOLOGIA 74: 252) pointed out that: '... typification of names used in the sanctioning works "is based on the protologue" of the sanctioning author' and on p. 253 referred to "Fries's protologue". [SECRETARIAL NOTE: original text removed; now in comments after Prop. (221) and Prop. (227, 231)]

.... Some other comments: (1) If indeed typification of sanctioned **species** names is nearly always linked to the original protologue (as suggested in the last paragraph of the background text of the Redhead et al. proposal) then we should consider an **alternative 3**, which is to limit sanctiotypification to ranks above species. I would be in favour of this because I think it simplifies things. If the modern concept and the sanctioning author's concept for a species are in accord, but differ from the original concept, there is the simple expedient of conservation available, and this brings fungi into line with other organisms treated under the CODE, which I think is generally a good thing. What would persuade against excluding ranks of species and below from sanctiotypification is examples where typification has been based on elements not in the protologue. Without a list of types of sanctioned names, it is hard to track these, but we at least need an idea of the potential for such situations.

It is quite a different matter for genera because of the much expanded concept often provided in sanctioning works, where often more species are accepted compared to the original descriptions.

(2) Proposal (224) seems to go further than just removing ambiguity about typification of sanctioned names, with the addition of the phrase 'lacking a holotype' to Art. 7.8. Firstly, I take this to strictly refer to the holotype of the original name (and not a situation where there is only one element cited in the sanctioning work). Clarification of what is the protologue of a sanctioned name (i.e. the original protologue) also helps avoid ambiguity about what is the holotype.

Again, without a list of types of sanctioned names, it is not possible to see whether forcing use of a holotype as the type where one exists could alter the typification of some names. At present there is potential to typify with an element from the sanctioning work even where there is a holotype for the original name, and the holotype could be in conflict with the elements mentioned in the sanctioning work. Nevertheless, I am in favour of the restriction because again it is a simpler approach and brings fungi more into line with other organisms treated under the Code.

It is important to note that the Perry proposals such as (220) and (221) are not in accord with 224 that only allows sanctiotypifying if there is no holotype.

(3) For 9.6bis. Ex 3bis. This example specifically relates to the situation where a sanctioning author introduces a new name, with coincident publication and sanctioning of the name. It might be better to have examples involving sanctioning of pre-existing names, although we do need to allude to this situation. For names newly introduced by sanctioning authors, is there any difference here between lectotypification and sanctiotypification? If not, worth mentioning that this is so.

DEMO 110115: You do not provide for further discussion in those cases but I believe we need it. I voted according to my present feeling but will send you as soon as possible a detailed commentary. Further you forgot to provide for a vote on prop. 221 by Perry. She is good in nomenclature but is no mycologist and this proposal must be opposed, while I could live with either 223 or 224. A vote of opposition by the committee would be an important thing. We may discuss this when you have synthesized those votes, as well as the procedure last question. It would have been fair to let me explain my reasons, but we may come to a compromise on this. [SECRETARIAL NOTE: Vincent's additional comments, received during the Ballot 2011-2 voting period and shared with the Committee at that time, are below.]

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 247): PROP. I (**224**), as indicated, is part of a series of proposals designed to provide a less extreme approach to the typification of sanctioned names than Prop. H. Comments on this approach are provided under Art. 9 Prop. J.

→ **DEMO 110210** [pro parte]: I am sorry I have not the time for commenting at length. Those issues were the object of long discussions in the committee years ago and finally things were left with the compromise adopted at Sydney. At that time I felt like David Hawksworth (see the report) that a special provision was not necessary. If the sanctioned concept was incompatible with the original one then it could become a sanctioned later homonym. I however aligned with Korf in the fear that not providing for maintaining typifications made according to the sanctioning author would draw more opposition from people like Singer to the abolition of later starting points. Seeing, that even if he does not favor it, Scott (who is Singer disciple) did accept the idea one could suppress the special status made me think one should have given more attention (examining consequences) to that option.

Art. 7, Prop. J (221) (out of the proposal set encompassing Props. 215–221) on original material. Proposed by Gillian Perry. TAXON 59(6): 1909. (2010, in press)

→ **BALLOT 2011-2 RESULTS: 3 yes : 10 no : 0 abstain** [1 ballot not returned]
77% returning Ballot 2011-2, or 71% of the full NCF, voted no on the proposal. The Secretary will report that a 71% majority does not support Art. 7, Prop. J.

Background: In its entirety, Art. 7, Prop. J (**221**) reads: Add a new Note after Art. 7.8 to read as follows: “*Note 1.* For sanctioned names all references to ‘protologue’ (cf. Art. 9.4, 9.5, 9.17, 10.2, 10.5, 10.4, 59.2, 59.3 & 59.7 and Rec 9A.2, 9A.3, 9A.4, 9B.1) are taken as referring to everything associated with the name in the sanctioning work.”

Committee and guest comments on Art 7, Prop. J (221)

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 247): Art. 7, PROP. J (**221**), as indicated, is also part of a series of proposals designed to clarify the typification of sanctioned names. Comments on this approach are provided under Art. 9. Prop I.

MAY 101217: (from comments on Prop. (223–232): The Perry proposals that relate to Art 7.8 (**217, 220, 221**) offer an alternative solution, but I think it best to use sanctiotypification, rather than continue to extend lectotypification to the situation.

NORV 110131: I also oppose Art. 9, PROP. I (**220**) and Art. 7 Prop. J (**221**). I feel the proposals are simplistic and do not take into consideration the problems encountered by mycologists who must deal with the problems arising out of the sanctioning proposals.

→ **DEMO 110210** [pro parte]: ... one certainly must oppose 220,221.

Rec. 8B, Prop. A (138), on the designation of cultures of fungi and algae as types. Proposed by Takashi Nakada. TAXON 59(3): 983. (2010)

→ BALLOT 2011-2 RESULTS: **10** yes : **2** no : **1** abstain [1 ballot not returned]
77% of the votes returned (71% of the NCF membership) supports Rec. 8B, Prop. A.

Summary of Rec. 8B, Prop. A (138):

Add a new Recommendation 8B.3 to read:

“8B.3. When a culture is designated as a type, the status of the culture should be indicated, including the phrase ‘permanently preserved in a metabolically inactive state’ or an equivalent.”

Guests commenting on Props. **18 & 19** include JOHN MCNEILL [IBC Nomenclature Section Rapporteur-général, Edinburgh, Scotland UK] & Nicholas Turland [IBC Nomenclature Section Secretary rapporteur, St. Louis, MO USA; 110126]

Committee & Guest comments on Prop. (138):

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 248): PROP. A (**138**) seems to be a useful addition to Rec. *b. The Nomenclature Committees for Algae and for Fungi have been asked to give their recommendations on this proposal; those of the latter will be published in the April issue of TAXON.

NORV 110130: I support this proposal.

Art. 9, Prop. I (220) (out of the proposal set encompassing Props. 215–221) on original material. Proposed by Gillian Perry. TAXON 59(6): 1909. (2010, in press)

→ BALLOT 2011-2 RESULTS: **3** yes : **9** no : **1** abstain [1 ballot not returned]
69% returning Ballot 2011-2, or 64% of the full NCF, voted no on the proposal. The Secretary will report that a 64% majority does not support Art. 9, Prop. I.

Background: In its entirety, Art. 9, Prop. I (**220**) reads: Add a further new Note after Note 2ter to read as follows: *Note 2 quarter*. For names falling under Art. 7.8, the original material comprises only those specimens and illustrations associated with the name in the sanctioning work and can be an element not associated with the protologue.”

Committee and guest comments on Art. 9, Prop. I (220)

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 252): Art. 9, PROP. I (**220**), along with Art. 7 Prop J, proposes that for sanctioned names all references to “protologue” are taken as referring to everything associated with the name in the sanctioning work, while this proposal (Art. 9. Prop I) reflects the implications of Art 7.8 for the definition of original material of sanctioned names. If deletion of Art 7.8 (Art. 7 Prop. H) is deemed too extreme a solution, Art. 7, Prop. J and Art. 9, Prop I provide relatively simple modifications of existing terms and definitions to accommodate the typification of sanctioned name and appear to present a workable alternative to the proposals on sanctiotypes, as discussed below (Prop. J–M, **223–226**). [NC Fungi to report in April TAXON; our deadline Feb. 12!]

NORV 110131: I also oppose Art. 9, PROP. I (**220**) and Art. 7 Prop. J (**221**). I feel the proposals are simplistic and do not take into consideration the problems encountered by mycologists who must deal with the problems arising out of the sanctioning proposals.

→ **DEMO 110210** [pro parte]: ... one certainly must oppose 220,221.

Art. 9, Prop. J–M (225, 226, 228, & 232) would add articles as necessitated by adoption of Art. 7, Prop. I. [from Props. 223–232 proposed by Redhead & al. TAXON 59(6): 1911–1913. 2010]

BALLOT 2011-1 RESULTS (on 224–232 together): 8 yes : 3 no : 3 abstain

→ BALLOT 2011-2 RESULTS: **8** yes : **2** no : **3**(4) abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

62% returning Ballot 2011-2, or 57% of the full NCF—a percentage that does not change when combining the vote from the non-responding member's 2011-1 ballot, voted yes on the proposal,. The Secretary will report that a 57% majority supports Art. 9, Prop. J–M.

Committee & guest comments on Art. 9, Prop. J–M (225, 226, 228 & 232)

MAY 101217: [SECRETARIAL NOTE: Text originally contained within comments on Prop. (223–232) (4) A few minor corrections are suggested:

Prop. **(225)** For 9.6bis. Ex 3bis. For the *Agaricus* 'trib.' *Clitocybe* example, need to state that there is not a holotype in order to then proceed to sanctiotypification.

Prop. **(225)** A very minor correction is that for 9.6bis. Ex. 3ter. it is fine to cite 'Weber (in Wiggers ...)' as full citation for place of publication of *Patella*, but for author citation of just the names, should omit 'in Wigg.' (as used in *Patella stercorea* Weber in Wigg.), following Art. 46.2 note 1.

Prop. **(232)** Art. 9. Ex 8ter. I wondered whether the designation by Hallenberg could be considered an epitypification under Art. 9.8. The answer is no, because the 'holotype, lectotype, or neotype that the epitype supports' would have had to have been 'explicitly cited' (Art 9.7). It could be worth indicating at the end of the example that: ', and nor is it an epitype because there is no explicit statement of the sanctiotype that it supports'.

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 252): Art. 9 PROP. J–M (**225, 228, 232, 226**), along with Art. 7 Prop. I, provide an alternative approach to the typification of sanctioned names. The proposals adopt the premise that the process of typification of such names is so different from that of other names that establishing a special term, "sanctiotype," is desirable for what would otherwise be the lectotype of a sanctioned name. Those most involved in such typification should advise on whether the simpler solution of Prop. I above deals adequately with the situation or whether the special terminology of this set of proposals is needed. Prop. J (with Art 9 Prop I) is the core proposal. Prop K is editorial, consequent on its acceptance. Prop. L and M provide examples that could well be incorporated in the CODE (with suitable editorial modification) regardless of the outcome of the other proposals (except Art. 7 Prop. H to delete Art. 7.8) and may be referred to the Editorial Committee.

Rec. 9C (new), Prop. A & C (231 & 227) would add recommendations as necessitated by adoption of Art. 7, Prop. I. [from Props. 223–232, proposed by Redhead & al. TAXON 59(6): 1911–1913. 2010]

BALLOT 2011-1 RESULTS (on 224–232 together): 8 yes : 3 no : 3 abstain

→ BALLOT 2011-2 RESULTS: **8** yes : **2** no : **3**(4) abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

62% returning Ballot 2011-2, or 57% of the full NCF—a percentage that does not change when combining the vote from the non-responding member's 2011-1 ballot, voted yes on the proposal,. The Secretary will report that a 57% majority supports Rec. 9C (new), Prop. A & C.

Committee & guest comments on Rec. 9C (new), Prop. A & C (231 & 227)

MAY 101217: [SECRETARIAL NOTE: Text originally contained within comments on Prop. (223–232) Thus, in order to clarify protologue, the idea introduced by Perry of explicitly treating the sanctioning work as a protologue is appealing because this gets around use of the somewhat imprecise term 'any element' in prop. 224. We know already the difficulties of being specific in Art 7.8 about the 'elements'! However, I would not call it a protologue, but rather pick up on suggestions in prop. **(227)** for the sanctioning treatment to be treated as equivalent to a protologue for the purposes of sanctiotypification, while being clear that it is not **the** protologue (unless the name is newly introduced in the sanctioning work). This is because we still need to talk about **the** protologue when discussing sanctioning, especially because of the proposed restriction of sanctiotypification to cases where there is not a holotype (see 2 below).

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 254): The rationale for Rec. 9C(new), Prop. A (**231**) is that, except when a type is “otherwise chosen” under Art. 10.2, no eligible type of the name of a genus or subdivision of a genus can be in conflict with the protologue because, by definition, it must have been “definitely included” in the protologue. In the situation when no type of a species name is definitely included, the proposal might seem to permit a type of the name of a genus or subdivision of a genus that is in serious conflict with the protologue to be superseded by another element that is also in conflict with the protologue. However, Art. 10.2 already requires supersession “if it can be demonstrated that the selected type is not conspecific with any of the material associated with the protologue” and so supersession is already mandatory in the event of any conflict with the protologue. Consequently it would be logical to delete not only the words proposed but the whole of Art 10.5(a), which is an accidental relict of the division in the Tokyo Code of the detailed rules on typification into three sets, general matters (Art. 7), those dealing with names of species and infraspecific taxa (Arts. 8 and 9), and those dealing with names of genera and subdivisions of genera (Art. 10). An “ed.c.” vote will be interpreted as favouring the deletion of clause “(a)” in Art. 10.5. PROP. C (**227**) is a part of a series of proposals on the typification of sanctioned names for which comments are provided under Art. 9, Prop. J.

Div. III, Prop. G (019), to permit decisions on proposals relating solely to fungi to be taken at an International Mycological Congress [and] **Prop. H (020)**, to make such decisions binding on the subsequent IBC Nomenclature Section.

BALLOT 2010-2 & 2011-1 RESULTS: (019 & 020)—6 yes : 8 no : 0 abstain

→ BALLOT 2011-2 RESULTS: **6** yes : **7(8)** no : **0** abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

54% returning Ballot 2011-2, or 50% of the full NCF—a percentage that increases to 57% adding votes from the non-responding member’s 2011-1 ballot, voted no on the proposal. The Secretary will report that a 50–57% majority does not support Div. III, Prop. G–H.

Summary of Div. III, Prop. G–H (019–020):

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 ... (**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 & Guest comments on Prop. (019–20):

DEMO 90706: [I am] definitely opposed [to Prop. 19]. 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.

TRIE 91030: ... 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: [see Comments under (018)]

(GUEST) **Walter Gams 100820: Prop. 019** is much more radical and will affect fungal nomenclature. So far such proposals were handled by the CF, then ratified (or not) by the General Committee and subsequently by the Botanical Congress. Acceptance of the proposal would eliminate the valuable scrutiny by the General Committee in which the Secretary of the CF automatically participates. Therefore and because no effort was demonstrated at Edinburgh to improve the nomenclatural infrastructure among mycologists, I give this vote a **resounding No**. This proposal was not further discussed in the first session at Edinburgh.

DEMO 100824: Experience of IMC9 reinforces my opposition. If big advertisement may have drawn tens of attendants from among the 1700 attending the Congress, I do not believe those with direct involvement in nomenclature were much more numerous than at a Botanical Congress. Further at a normal nomenclature session, those not attending can be represented through the members of their institution that carry the institutional votes. Since this proposal does not provide for institutional votes, I will anyway strongly oppose it.

Note that "maintaining a presence on the Editorial Committee" is rather superfluous. Because of the editing of lists of conserved and rejected names the Ed. C. has always tried to have at least a mycologist, an algologist and a paleobotanist. At the moment we are two mycologists and those who may complain are the bryologists and pteridologists. Writing down the practice as a rule may have as consequence that those groups may want to also have their guarantee and we lose one of our mycologists.

ITUR 101115: Gams 100820 comment on proposal **(019)** worries me, so for the moment I keep the conservative side.

MAY 101115: ... I do not agree at this stage with **(019)** and **(020)**, mainly because of the lack of provision for institutional votes in voting at IMC, and other formalities about the voting that are well established for IBCs. Among votes at Botanical Congresses there is a substantial proportion of institutional votes – see for example the tabulation by McNeill (2005: *TAXON* 54: 1057) where for IBC from 1975 to 2005 the number of individual votes was between 95 and 297 but the number of institutional votes ranged from 296 to 494 (representing 116 to 231 institutions). Inclusion of institutional votes helps to overcome bias in those actually attending the Congress due to the geographic location of the Congress and also, to some extent, due to resources available at different institutions to support staff attending Congresses. It is true that there is some debate about how to apportion the institutional votes (see for example, Applequist et al., 2010, *TAXON* 59: 1567-1570). Whatever way this is resolved, any feeling that mycologists are shut out of the voting can be addressed by utilising the existing processes of Div. III.4 (b) (2) footnote 2 for adding institutions to the list of those permitted to vote. In any case, there are plenty of mycologists already represented: a quick look at the list of institutions provided by McNeill & Turland (2009, *TAXON* 58: 971-973) shows that predominantly mycological institutions are included, such as BPI (3 votes), CBS (4), DAOM (3), DAR (1), HMAS (5), IMI (4) and PDD (1), as are plenty of institutions with mycologists on staff such as K (7 votes), MEL (4) and NY (7). In my experience of preparation of institutional votes at MEL (where there are two mycologists and about half a dozen vascular plant taxonomists), other staff are only too happy to take on board the view of mycologists when deciding on proposals specific to fungi. The longer times between IBCs as compared to IMCs is not really a factor retarding changing the Code in respect of provisions important for fungi. The main problem in addressing the key areas where change is needed (such as electronic publishing and Art 59) is getting people to agree on workable changes in the first place.

NORV 101115: MAY 10115 makes excellent points regarding institutional votes that I had not previously considered. I would note, however, that the time between IBCs can be extremely trying with respect to Conservation/Rejection proposals. For the most part, the General Committee DOES follow our lead with respect to the nomenclature of Fungi, but not always. As Secretary, I must frequently inform proposal authors that nothing is final UNTIL it has been passed at a Congress and ALSO finds its way into the CODE. One has only to look at the recent upheavals with respect to *Acacia* to see that nothing is settled until after the CODE is published.

DEMO 101216: My experience entirely concurs with that of MAY 101115. The demagogy of opposing institutional votes (see again the October TAXON) is pure folly.

(GUEST) **McNeill & Turland 110126** (TAXON 60: 281): Division III PROP. F–H (018–020) represent the mechanism by which it is proposed that the outcome of amendments to the ICBN relating solely to fungi be determined by mycologists alone rather than by the Nomenclature Section of an International Botanical Congress that an increasingly small number of mycologists attend. Although in practice such decisions have generally been taken by Nomenclature Sections on the advice of the permanent Nomenclature Committee for Fungi and of the mycologists present, it does seem desirable that a larger and more representative group of mycologists make these decisions. The mechanism proposed does not seem to create any insurmountable difficulties, although given the different frequency of Botanical and Mycological Congresses, there may come to be practical difficulties in the timing of implementation of agreed changes. The view of the Nomenclature Committee for Fungi on these proposals has been sought and will be presented in the April issue of TAXON [our deadline Feb. 12!].

NORV 110130: I now support Division III PROP. F–H (018–020).

→ **DEMO 110210:** I would answer to NORV 101209 that one should put the ten mycologists at the last nomenclature session in perspective with the total number of attendant. Unfortunately I am in such a hurry I cannot find my report with the number. On the reverse I believe that they were not much more than ten persons experienced in nomenclature at the mycological congress sessions.

→ **NORV 110210:** I repeat: attendance during the 2010 International Mycological Congress by 91, 65, and 145 mycologists at three two-hour long sessions devoted exclusively to nomenclature (not stand alone as at an IBC Nomenclature Section but competing with poster and other sessions) demonstrates an interest in nomenclature equal to or greater than attendance by only 10 mycologists at the Viennese Section in Vienna. The same number of mycologists had the opportunity to attend both.

We should never assume that our Committee comprises the ONLY nomenclaturalists well trained enough to provide informed advice regarding mycological matters, but we do have an obligation to encourage an appreciation of the CODE within the mycological community. I feel that this would be better achieved within the confines of an IMC where nomenclatural decisions and their impact on mycology could be discussed at length.

→ **Secretarial clarification 110214:** Vincent later clarified that he was interested in the number of total participants voting in the 2005 Nomenclature Section at the Vienna Congress, which turns out to have been around 200. This would mean that our ten voting mycologists represented 1/20 (or 5%) of the total. Obviously, those voting at the Edinburgh 2006 IMC Nomenclature Sessions were all mycologists, and thus represented 100% of the total attending.

2-B: PROPOSALS TO AMEND ART. 59

MAY 110114: [commenting on all Art. 59 proposals]: I strongly support all the Redhead and Hawksworth proposals to amend Art. 59 that are included in Ballot 2011, and the clarifications by introduction of the term teleotypification.

I rarely work specifically on fungi that have pleomorphic life cycles, but in the broader context of my checklist and catalogue work I do frequently deal with the names of such fungi. Trying to integrate the ramifications of Art. 59 into databases for names of biota in general is a real problem - whether in on-line portals such as the Atlas of Living Australia or in reference collection databases.

For no other biota do we have different code-governed names for different morphs. In other biota the morphological differences can be great—whether between male and female or among different stages such as caterpillar and butterfly or biphasic marine organisms with pelagic larvae and demersal adults. When it was not possible to link different stages of the life cycle of fungi as characterised by morphology (or place fungi known only by the anamorph), it was quite OK to have different names, but now that molecular techniques are widely available and inexpensive it is possible to place all fungi within a phylogenetic classification. Our nomenclature should reflect such a classification, not introduced obstacles to prevent it.

Adopting a phylogenetically based classification is essential, but does not preclude anyone using morphology for identification, or (as allowed under props 185 and 186) informal names for morphologically circumscribed taxa. I think that user groups will adapt to names as provided by taxonomists, whether those names are formal (code compliant) or informal.

The fact that morphology may be used in identification of fungi does not mean that the classification must be tied also to morphology. I personally use morphology preferentially for identification, because I am very familiar with often quite obscure morphological characters. However, I suggest that morphology will be less and less used as molecular identification becomes cheaper and more accurate than paying for the time of a highly trained mycologist to look down a microscope.

It can be annoying to 'lose' morphological indications in the name of a taxon when a phylogenetic classification is adopted. I find this so for *Cortinarius*, where previously we could readily spot sequestrate (truffle) species because they were placed in genera such as *Thaxterogaster*. However, we cannot maintain these genera because they are just not phylogenetically supported. In looking for names of truffles I now have to wade through thousands of names in *Cortinarius*, which is indeed a drag. However, the solution is to start to include morph information in global databases, not to insist on the retention of taxa that are not monophyletic.

Once we see how the CF has voted on this set of proposals, a few 'friendly' amendments may be required to enhance them. For example, Art 59 Ex. 1 is there to show that Art 59 does not apply to lichen forming fungi. It might be useful to have a new example that demonstrates application of Art 59 to the fungi that it DOES cover (to in effect replace the deleted examples 3 and 4).

Also, it is a little confusing at present as to whether there is any difference in the treatment of genera and species under Art 59 and it would be good to spell this out. This is because 59.1 covers holomorphs (with holomorph defined as a 'species in all its morphs') and 59.2 covers binary names, and then 59.3 just talks about names (which presumably is names of species or names of genera).

→ **DEMO 110210** [commenting on all Art. 59 proposals]: I have no time to comment on Art.59 and hope I will be able to study the matter somewhat before voting on Saturday. I would however reiterate my strong opinion, as somebody who is not a taxonomist of imperfect fungi, but who constantly has to provide identification and advises on biodegradation and health issues implying those fungi, it is essential for practical reasons to retain dual nomenclature.

→ **KIRK 110211**: with respect to Art. 59, my opinion is that what we are trying to do is equivalent to rearranging the deckchairs on the Titanic – I am not convinced that the chaos we have now will be any worse if we deleted the entire article, so I vote ‘No’ on any proposal to amend the article. [Others] will continue to revise (split) anamorphic genera based primarily on results from analyses of molecular data (because that is what [their] collaborators are interested in) and that is contrary to the reason why I suspect Vincent supports dual nomenclature – name stability.

Art. 59, Prop. B (173), to modify Art. 59.7 so as to avoid including taxa with teleomorph-typified names within otherwise entirely anamorphic genera. (172–174) proposed by Gams & al. *TAXON* 59(4): 1297. (2010).

BALLOT 2010-2 RESULTS: 1 yes : 7 no : 6 abstain

BALLOT 2011-1 RESULTS: 3 yes : 8 no : 3 abstain.

→ **BALLOT 2011-2 RESULTS: 5 yes : 7 no : 1(2) abstain** [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

58% returning Ballot 2011-2, or 50% of the full NCF—a percentage unchanged when combining the vote from the non-responding member’s previous ballot, voted no on the proposal. The Secretary will report that a 50% simple majority does not support Art. 59, Prop. B.

Art. 59, Prop. E (298), to delete Art. 59.5, Art. 59 Note 1, and Art. 59 Ex. 3 & 4; **Prop. G (300)**, to replace in Art. 59.3 “that of a form-taxon and is applicable only to the anamorph represented by its type, as described or referred to in the protologue” with “anamorphic with restricted priority with regard to names for teleomorphic types or teleotypes (Art 59.1).” (294–306) proposed by Redhead, *TAXON* 59(6): 1927–1929. 2010.

BALLOT 2011-1 RESULTS: (298)—8 yes : 4 no : 2 abstain; (300)— 8 yes : 3 no : 3 abstain.

→ **BALLOT 2011-2 RESULTS: 9 yes : 2 no : 2(3) abstain** [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

69% returning Ballot 2011-2, or 64% of the full NCF— a percentage that does not change when adding the vote from the non-responding member’s 2011-1 ballot, voted yes on both proposals. The Secretary will report that a 64% majority supports Art. 50, Prop. E & G.

Background & comments on Art. 59, Prop. E (298) & G (300)

SUMMARY: (298–301) would add two additional paragraphs in Art. 59 to address instances where simultaneous publication of synonymous anamorph and teleomorph names permit selection of a teleotype so that an anamorph can be used as the name of a holomorph or as a replaced name for a nomen novum and amend other Art. 59 sections as needed to conform to passage of (297).

NORV 101115: I am somewhat hesitant about the ramifications resulting from passage of Props. 296–301 and so wish to consider other comments before making my final decision.

(GUESTS) **McNeill & Turland 110126** (*TAXON* 60: 277): Art. 59, PROP. C–K (**296–304**) comprise a set of proposals by the Secretary of the [Spec. Comm. on Art. 59] in the absence of agreement to any proposals by the Committee as a whole. They are designed to limit dual nomenclature. ... Prop. D is the core proposal to limit dual nomenclature by making illegitimate later names for anamorphs or teleomorphs considered to belong to the same taxon as a morph with an existing legitimate name. The qualifications in the proposed new rules “considered to belong to the same taxon ... at the time of publication” and “considered at the time of publication to be the same taxon” might be difficult to establish objectively, whereas “in the publication” rather than “at the time of publication” would be more readily assess. Prop. E–H represent necessary changes to the existing rules, consequent on acceptance of Prop. D. Prop. I–

K seek to increase clarity in Art. 59. The Spec. Comm. on Art. 59 voted as ... E–4 :4 : 2; ... G–8 :1 :1; ... J– 6+2 (subject to the deletion of the date) :1 :1....

Art. 59, Prop. J (303), to modify Art. 59.1 as needed to agree with passage of Prop. D. (294–306) proposed by Redhead, TAXON 59(6): 1927–1929. 2010.

(303) BALLOT 2011-1 RESULTS: 7 yes : 4 no : 3 abstain.

→ BALLOT 2011-2 RESULTS: **8** yes : **4** no : **1(2)** abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

67% returning Ballot 2011-2, or 57% of the full NCF— a percentage that remains unchanged when combining the vote from the non-responding member’s previous ballot, voted yes on the proposal. The Secretary will report that a 57% majority supports Art. 59, Prop. J.

Art. 59, Prop. L–N (307–309) to modify Art. 59 to raise the status of anamorph names. (307–313) proposed by Gams & al., TAXON 59(6): 1929–1930. 2010.

(307) BALLOT 2011-1 RESULTS: **8** yes : **4** no : **2** abstain.

→ BALLOT 2011-2 RESULTS: **9** yes : **3** no : **1(2)** abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

69% returning Ballot 2011-2, or 64% of the full NCF—a percentage that does not change when combining the vote from the non-responding member’s previous ballot, voted yes on the proposal. The Secretary will report that a 64% majority supports Art. 59, Prop. L.

(308–309) BALLOT 2011-1 RESULTS: **5** yes : **7** no : **2** abstain. [reflects one retroactive vote change (yes > no) on (308) noted on Ballot 2011-2]

→ BALLOT 2011-2 RESULTS: **4** yes : **8** no : **1(2)** abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

67% returning Ballot 2011-2, or 57% of the full NCF, voted no on both proposals, a percentage unchanged when combining the vote from the non-responding member’s previous ballot. The Secretary will report that a 57% majority does not support Art. 59, Prop. M–N.

Rapporteurs’ comments on Art. 59, Prop. L–N (307-311)

(GUESTS) **McNeill & Turland 110126** (TAXON 60: 277): Art. 59, PROP. L–P (**307–311**) represent a series of proposals designed to avoid usage of redundant dual names by attributing more weight to anamorph names while still preserving, as far as possible, the usage of teleomorph-typified generic names for species with teleomorph typified names and vice versa. The Spec. Comm. on Art. 59 voted as: Prop. **L–2** :7 :1; Prop. **M–1** :8 :1; Prop. **N–1** :8 :1; Prop. **O–1** :8 :1; Prop. **P–1** :8 :1.

Art. 59, Prop. O–P (310–311) to clarify the situation of teleomorph-typified and anamorph-typified genera. (307–313) proposed by Gams & al., TAXON 59(6): 1929–1930. 2010.

BALLOT 2011-1 RESULTS: (310)—6 yes : 7 no : 1 abstain; (311)—5 yes : 7 no : 2 abstain.

→ BALLOT 2011-2 RESULTS: **5** yes : **7** no : **1(2)** abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]

54% returning Ballot 2011-2, or 50% of the full NCF—a percentage unchanged when combining the vote from the non-responding member’s previous ballot, voted no on both proposals. The Secretary will report that a 50% simple majority does not support Art. 59, Prop. O–P.

Rec. 59, Prop. A–B (312–313) to specify that “teleomorph-typified genera should (as far as possible) be reserved to teleomorph-typified species and vice versa for anamorphs.”. (307–313) proposed by Gams & al., TAXON 59(6): 1929–1930. 2010.

BALLOT 2011-1 RESULTS: (312 & 313) **5** yes : **7** no : **2** abstain.

- BALLOT 2011-2 RESULTS: 5 yes : 7 no : 1(2) abstain [(#) = counting Ballot 2011-1 vote cast substituted for missing Ballot 2011-2]
 54% returning Ballot 2011-2, or 50% of the full NCF—a percentage when combining the vote from the non-responding member’s previous ballot, voted no on both proposals. The Secretary will report that a 50% simple majority does not support Rec. 59, Prop. A–B.

Rapporteurs’ comments on Prop. (312–313)

(GUESTS) McNeill & Turland **110126** ((TAXON 60: 278): Rec. 59A PROP. B and C (**312–313**) are adjuncts to Art. 59 Prop. L–P. The Spec. Comm. on Art. 59 voted ... Prop. **B–1** :8: 1; Prop. **C–2**: 7: 1.

Part 3: Special recommendations —

None now in discussion (CF-4 next to be assigned). NOTE: Post-Melbourne special recommendation topics will be numbered anew as needed, starting with NCF-1.)

Part 4: General remarks & discussion —

- SECRETARIAL NOTE: After considerable discussion, the NC for Fungi voted on Ballot 2011-2 [10 yes, 2 no, 2 abstain] to recommend **Prop. 1896** to conserve the name *Lichen lichenoides* (*Leptogium lichenoides*) against *Lichen tremelloides* and *L. tremella* (lichenized *Ascomycota*). Scott Redhead feels that we may not have considered all ramifications surrounding our approval of Prop. 1896, and so provides the following comments.

- **REDH 110211**: I am disappointed that the Committee has opted to recommend the adoption of a conservation proposal for a name that is unnecessary. I maintain that the name *Tremella lichenoides* L. (1753) is valid and therefore the name *Lichen lichenoides* (**L.**) **Wulfen in Jacquin** is not threatened by *Lichen tremelloides* Weiss (1770) nor by *Lichen Tremella* Roth (1788) because it has an older basionym (1753). Spencer, Irvine & Jarvis (58 (1): February 2009: 253. 2009) erred when they accepted Donk’s supersession of an earlier lectotype for *Tremella* L. I raised this issue here and the Committee has not resolved it. I will therefore raise the issue at another level. It surprises me that this Committee would not consider the effects on mycology of accepting an algal solution. Why is this Committee in such a rush for one lichen name?

Look how Index Nomina Genericorum treats “*Tremella*”, dating “it” from 1901, 1822, 1821.

Tremella Linnaeus ex Arthur, Proc. Indiana Acad. Sci. 1900: 133. 1901 (non Persoon ex Saint Amans 1821 (*nom. cons.*), nec Persoon ex E. M. Fries 1822).

T.: *T. juniperina* Linnaeus ex Arthur
 UREDINALES (83) 9 Feb 1996

[C] *Tremella* Persoon ex E. M. Fries, Syst. Mycol. 2: 209, 210. 1822 (non Persoon ex Saint Amans 1821 (*nom. cons.*)).

LT.: *T. frondosa* E. M. Fries (vide F. E. Clements et Shear, Gen. Fungi 342. 1931)

FUNGI ['CAET.]-TREMELLACEAE (75) 9 Feb 1996

Tremella Persoon ex Saint Amans, Fl. Agen. 535. Apr 1821 (*nom. cons.*).

T.: *T. mesenterica* Persoon ex Saint Amans (*typ. cons.*)

FUNGI ['CAET.]-TREMELLACEAE (110) 9 Feb 1996

Compared to Index Fungorum

Tremella Dill. ex L., (1753)

Tremella Pers., Neues Mag. Bot. 1: 111 (1794) [conserved]

And MycoBank:

Tremella Pers. 1794
 1794, Neues Magazin für die Botanik, Römer 1: 111
 Type taxon: *Tremella mesenterica* (Schaeff.) Retz. **1769**
 [IN WHAT GENUS!]

Tremella L. 1753
 Species Plantarum: 1157
 Status of the name: basionym; unavailable
 Type taxon: *Tremella mesenterica* Schaeff. 1774
 [HOW CAN THIS BE?]

The solution is to accept *Tremella* L. (1753) as valid, typified by *T. juniperina* L. *Tremella* Pers. remains conserved and it does not matter if it were also sanctioned because conservation overrules sanctioning.

I fail to see how an earlier lectotype can be superseded by an invalidly named type.

A name must be valid or it is not a name in the Code

Art. 6.3. In this *Code*, unless otherwise indicated, the word "**name**" means a **name that has been validly published**, whether it is legitimate or illegitimate (see [Art. 12](#)).

Art. 7.1. The application of **names** of taxa of the rank of family or below is determined by means of nomenclatural types (**types of names** of taxa). The application of names of taxa in the higher ranks is also determined by means of types when the names are ultimately based on generic names (see [Art. 10.7](#)).

Art. 10.1. The type of a **name** of a genus or of any subdivision of a genus is the type of a **name** of a species (except as provided by Art. 10.4). For purposes of designation or citation of a type, the **species name** alone suffices, i.e., it is considered as the full equivalent of its type.

Art. 10.2. If in the protologue of the **name** of a genus or of any subdivision of a genus the holotype or lectotype of one or more previously or simultaneously published species name(s) is definitely included (see Art. 10.3), the type must be chosen ([Art. 7.10](#) and [7.11](#)) from among these types unless the type was indicated ([Art. 22.6](#), [22.7](#), [37.1](#) and [37.3](#)) or designated by the author of the name. If no type of a previously or simultaneously published species name was definitely included, a type must be otherwise chosen, but the choice is to be superseded if it can be demonstrated that the selected type is not conspecific with any of the material associated with the protologue.

Art. 10.5. The author who **first designates a type** of a name of a genus or subdivision of a genus **must be followed**, but the choice **may be** superseded if (a) it can be shown that it is in serious conflict with the protologue and another element is available which is not in conflict with the protologue, or (b) that it was based on a largely mechanical method of selection.

Donk (1958) operated under a much earlier Code where the starting date was 1821 for fungi, when he proposed to supersede the earlier selected lectotype. By selecting a type, no matter how it was done, Arthur (1901) ensured that *Tremella* L. was valid. How could Donk then select as "type" an invalid name (non rite publ.)? It makes no sense because the chosen "name" (which is not a name under the Code) "*Tremella nostoc* L." was not available! It violates Art. 10.1. Art. 10.5 only allows for supersession but does not insist upon it. It is fundamentally wrong to allow supersession of a valid generic name with a valid type species by an invalid name that can have no type because it has no protologue because a protologue is where it is first validated.

Then, superseding the “lectotype” makes all the names invalid, falling like dominoes. This is basically wrong and it should be challenged.

Look at all the partial list of *Tremella* names published prior to 1794 (as extracted from MycoBank). Combined with what generic name were they published? These were all pre 1886, the starting date for *Nostocaceae Heterocysteeae* (Art. 13.1(e) second paragraph).

Result of query (click on items for details; 526 record(s) found):

8. *Tremella adnata* J.F. Gmel. 1792 [LEG; MB496606]
 9. *Tremella adnata* L. 1755 [LEG; MB528958]
 10. *Tremella agaricoides* Retz. 1769 [LEG; MB496607]
 14. *Tremella albida* Huds. 1778 [LEG; MB231878]
 15. *Tremella allii* Holmsk. 1781 [LEG; MB496608]
 17. *Tremella alveolata* Scop. 1772 [LEG; MB496609]
 18. *Tremella amethystea* Bull. 1791 [LEG; MB231673]
 19. *Tremella ancephaloides* Willd. 1788 [LEG; MB496610]
 20. *Tremella annulata* Willd. 1788 [LEG; MB496611]
 23. *Tremella arborea* Hoffm. 1787 [ILL; MB121953]
 24. *Tremella arborea* Huds. 1778 [LEG; MB218056]
 25. *Tremella arborea* var. *fusca* With. 1792 [LEG; MB528943]
 29. *Tremella atra* O.F. Müll. 1782 [LEG; MB217978]
 30. *Tremella atra* Schrank 1789 [ILL; MB496614]
 33. *Tremella atrovirens* Bull. 1784 [LEG; MB497506]
 39. *Tremella auricula* L. 1753 [LEG; MB355933]
 41. *Tremella auricula-judae* Bull. 1789 [LEG; MB218233]
 47. *Tremella auriformis* Hoffm. 1787 [LEG; MB224790]
- etc. etc.

This Committee needs to oppose the notion that *Tremella* L. is to be typified by *T. nostoc* in order to leave in chronological order the publication of such names. What comes of *Tremella* “Adans.” (1763) mentioned by Donk?

→ **REDH 110214** [responding to Secretarial query as to whether he wishes to submit a new proposal]: I am not going to redraft a proposal, but rather write criticizing the assumption that *Tremella* has been “retro-lectotypified.”