25 August, 2005

Commenting procedures

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

Commentaries follow a format that is slightly different from previous mailings. Discussions are still grouped in three sections: GENERAL REMARKS (if present), PROPOSALS TO CONSERVE OR REJECT NAMES, and PROPOSALS TO AMEND THE CODE. Proposal sections are still organized numerically by proposal, with each proposal topic headed by proposal number, informal title, and reference citation. Where votes have been held, ballot tallies still appear in the second paragraph (see Ballots, below). When necessary, I will assign temporary CF numbers to topics or unassigned proposals; these discussions will stand at the end of the appropriate section.

Member comments are assigned alphanumeric codes that begin with the first four letters of the member's name and end with a date-based number in YMMDD format. This new method codes the dates each comment are submitted, particularly helpful in long-running discussions. For the same reason, each proposal's comments will now stand in chronological order according to submission date rather than alphabetically by member name. Smaller (10-point) font indicates text that printed previously; new text appearing for the first time is in 12-point font and, as before, with the member code flagged by an arrow (→).

Ballots will be held two or more times as year as needed. Commentaries include a running tally of all Committee votes, which are reported as YES : NO : MORE DISCUSSION NEEDED : ABSTENTION. A 60% majority (now **11!**) of the whole Committee must vote YES or NO to remove a proposal from discussion. Members are urged to return every ballot and vote on every ballot issue. Members "who, for three times in sequence over a period of nine months or more, fail to respond to requests for voting or subsequent reminders, are considered to have resigned" and will be replaced. Any qualified request for more discussion may outweigh a majority decision of the Committee and the respective proposal will remain open for further discussion.

GENERAL REMARKS:

Problems with Art. 37

GAMS (328). Paul Kirk has recently compiled a long list of names that were invalidly published during the last decade when Art. 37 is strictly applied. Several members of this Committee (including myself) have contravened this tricky article. Most of the cases detected by the compilers of Index of Fungi are straightforward and require correction. A few of them are debatable, such as *Trichoderma aggressivum* in Mycologia 94: 146-168, 2002. We plan to submit these cases to the CF for a binding vote about the validity, in order to avoid unnecessary supplementary publications and corrections. ... see previous mailings.

PROPOSALS TO CONSERVE OR REJECT NAMES

Prop. 1656, to reject *Lecidea flavocoerulescens.* Proposed by A.M. Fryday, Taxon 54: 181. 2005.

Voted May 2005: 9:4:1:1. Recommended, but this will not disappear from our discussion.

Committee comments:

JØRG (361) This concerns *Lecidea flavocoerulecens,* which is a well-known problem since no type specimen exists and the type is an illustration which is difficult to interprete (and have been interpreted differently by different lichenologists). A possible approach to solve this problem would be to designate an epitype. This is not a suitable procedure here, particularly since the name has been applied to several different taxa in the past, and it is therefore better rejected. I therefore support the proposal.

SIPM (364) This is another attempt to dismiss a dubious name from the early days of lichenology. The original description and illustration of *Porpidia flavocoerulescens* are not very informative indeed, and tell little more than that it is a crustose lichen with dark apothecia. Only a single specimen remains, which may have been seen by the author. However, it does not contain any apothecia, contrary to the protologue, and bears no annotation by the author, only a statement by Fries that it belongs to the original material. It is certainly preferable to use the name *P. flavicunda*, with a certain, fertile type specimen, instead. Therefore I will support this proposal.

PRIN (365) The situation seems to be more complex than apparent from the proposal. Buschbom & Mueller (Mol. Phyl. Evol. 32: 66-82, 2004) and Buschbom's thesis show evidence for several lineages. In Buschbom's thesis, one of the lineages seems entirely fertile, 1 entirely sterile, sorediate and 3 comprise fertile and sorediate individuals. This is certainly not the last word on this question, so I would rather like to see nomenclatural consequences postponed until the relationships within the group are more clear. With regard to the apparent importance of molecular studies in this group, wouldn't it be better to eventually conserve the names *L. flavicunda, L. flavocoerulescens* and *L. melinodes* with conserved types rather than reject a name that could still be useful in the future ? Another question: Is it absolutely certain that *L. flavocoerulescens* has priority over *L. flavicunda*? Both names were published in 1810.

SAMU (366) I do not support the proposal. It looks to me that the name *Lecidea flavocoerulescens* is in wider use than *Porpidia flavicunda.* Even though the former name is confused as it stands, designation an epitype to support the original illustration should resolve uncertainties surrounding the name.

DEMO (371) Like SAMU 366 I do not understand why the author as well as JORG 361 do not accept epitypification. The simple thing to do is accept Gowan and Ahit lectotypification and epitypify with a specimen allowing molecular characterization according to Buschbom and Mueller, which means the proposal is premature (PRIN 365).

GAMS (372) In view of the taxonomic uncertainties pointed out by PRIN I vote No.

PROPOSALS TO AMEND THE CODE

Article 59 [Limitation of dual nomenclature for pleomorphic fungi: pre-Vienna IBC Propositions 183–187]

Secretary comments: Of the five proposals below, only an amended Prop 184 was accepted at the 2005 IBC Nomenclature Section in Vienna. The CF comments from Secretary Gams's CF Mail 67 on this highly controversial article are included below to assist the Special Committee that will recommend possible amendments to by considered by the 2011 IBC in Melbourne. Comments are presented chronologically. Unless there are objections, text in red (which addresses the wording of now rejected proposals) will be summarized or dropped from Commentary 2. Authors of comments are encouraged to summarize or condense their own remarks to save space in Commentary 2 and successive commentaries. (Shorter text is generally more persuasive.) **[***See also Gams CF Report 13***].**

Props. 183-187, **Limitation of dual nomenclature for pleomorphic fungi.** Proposed by D.L. Hawksworth, Taxon 53: 596-598, 2004. Gams

Prop. 183: prohibit the introduction of new formal dual nomenclature from 1 Jan. 2008.

Voted May 2005: 3:8:2:2.

Prop. 184: Provision to avoid introd. of unnecessary teleomorph names: designation of epitypes.

Prop. 185: Provision for the use of informal designations for anamorphs.

Prop. 186: Recommendation of using informal designations for anamorphs.

Prop. 187: Authorize editorial Committee to amend rules on cross-referencing.
Voted May 2005: 5:7:2:1.

Secretary´s (Gams) comments: [SEC51223]: Being strongly involved in and having my own opinion on these problems, I cannot give you an objective guidance. As a first introduction, I recommend you to look into Mycotaxon 88: 493-508, 2003, where the Oslo debates about these questions are reproduced. For detailed suggestions concerning a gradual move towards a unified fungal nomenclature, see Hennebert´s texts on subsequent pages and at full length on the CBS website. Hawksworth's proposals do not offer a solution for dual fungal names coined before 2006. As a consequence, there will permanently be two intricated situations for fungal names, dual names introduced before and unified names after 2006.

My major point against such an approach that attacks the essence of Art. 59 is that this article has been introduced to the benefit of all mycologists, while its abandonment would serve only those using molecular identification. As editor of many mycological papers, contrary to Prop. 183, I still recommend authors to introduce separate epithets for teleomorph-linked anamorphs when these anamorphs are likely to occur and be isolated independently of the teleomorph and then their complete identity with the teleomorph is usually not ascertained without molecular work.

Anyhow, because these proposals are intended for mycologists, it will be crucial that the CF casts a vote before the Vienna congress, so that I can report there accordingly. I am not placing this on the present ballot while awaiting some discussion from the members.

[SEC50707]The arguments against these proposals are also summarized in my report 13. But for the moment I let the discussion go on. W.G.)

Committee comments:

JØRG (41200: 352) I feel torn both ways. I have myself taken actively part in restricting this practice to what is really necessary and maybe it would have been better to go a bit further in that direction, than do what is now proposed, which I cannot support as it stands. This will practically eradicate this use, which some workers in the field, for good reasons, still feel is necessary. I do not think it should be our role to make their important work more difficult - also these are names of economic importance since many are important pathogens. I believe the users will be horrified by such a drastic action, and mycologists come into disrepute in spite of the logics in giving up this system now that taxonomy has advanced, and the improvement towards a simpler nomenclature in this field which has proven difficult to handle. Somewhat to my own surprise, I find that these two arguments are weaker than the ones relating to practrical work.

CRAN (41200: 354) I would vote N0 on Proposal 183. More time is needed to refine this proposal. I believe in Principle IV and would like to see Article 59 modified to attain this. The informal system employed by Mason, Hughes, and Ellis is a step in this direction. Epitypification is another. I agree with Seifert -We need a system that prevents the description of a teleomorph genus when an anamorph genus is known. I am however bothered by a solution for dual nomenclature at the species level that simply elevates it to the generic level, e.g. polyphyletic groups. Personally, I would prefer the older generic name for the holomorph using epitypification. In any revision, Scott's objections summarized in Redhead, 11/25/04/9:29am must be considered.

MAY (41200: 355) I am not averse to changes in Art. 59, but given the preference of a good sample of mycologists (at IMC Oslo and ICPP Christchurch) against change, I would need to be convinced that proposals 183-187 are a change for the better. As Scott and Gary indicate, the proposals only partially solve the problems. In particular, there is no explicit solution to how best to assign one, and one only, generic name to monophyletic clades. I would prefer to see a set of proposals that deal more comprehensively with the situation.

Also of concern are a number of uncertainties as to how the new proposals would operate. So I would not support the proposals without more detail. Some questions have been raised about various situations already. Here are a few more.

What about a situation after 1 Jan 2008 where a new anamorph is discovered for a named teleomorph, but there is no appropriate existing anamorph genus (and presumably you could not introduce such an 'anamorph genus' since it is forbidden to introduce a binomial for the anamorph). Would this mean that a non-italicised term must be used when referring to the novel anamorph state (as hinted by Hawksworth's Note 2 under Proposal 185, where 'designations not regulated by this Code' are mentioned), and then you have *Penicillium* state of X and Thing state of Y where Penicillium could be italicised or not, but Thing would not be italicised.

After acceptance of proposals 183-186, what would be the situation for two competing species names found to be synonymous where one was a teleomorph, and another an anamorph name epitypified by a teleomorph (before the synonymy was realised). It would seem that the earlier epithet would have priority. That could well be fine (and conservation is available if the change is not desirable), but this implication needs to be spelt out (only examples for genera are mentioned in the discussion under Prop. 184).

As far as Gary's question about *Eidamia* (and Paul´s reading that the epithet cannot be transferred to *Hypocrea*) - if I understand Hawksworth's proposals correctly, and if they are in operation - then after 1 Jan 2008 I can´t see the impediment to epitypifying *Eidamia viridescens* with teleomorph material, and then transferring the epithet *viridescens* to *Hypocrea.* The proposed Art 59.7 stops introduction of separate names but does not seem to prevent transfer of a holomorph epithet (even if it is an epitypified anamorph name) to other genera - because the type (anamorph type + teleomorph epitype) travels with the epithet, and there is no 'separate name' with a different type. A clear answer is needed on this situation since Paul's scenario of mixtures of *Hypocrea* names and *Trichoderma* names with a known but unamable *Hypocrea* is not at all desirable!

Given that there seems some uncertainty as to exactly what are the ramifications of Hawksworth's proposals, I would prefer to see all the likely situations worked out beforehand, rather than discovering the full implications after the event, especially for such a major change.

An edit that seems necessary to proposal 183 is: delete 'publication and' in Art 59.5 Also, in the proposed Art 59.7 it would be better for consistency to use 'correlated with' (as existing in Art 59.6) rather than 'associated with'

As a PS, I wonder if Keith Seifert's point about names for environmental DNA ('6000 unnamed putative species') relates more to taxonomy than nomenclature. It would be bad taxonomy to name a new taxon just on a DNA sequence that was a little different from known sequences, without a very good understanding of variation within and between species of the group (based on several or more DNA regions), and without a much better sampling of the DNA of all known taxa. DNA is just another tool to add to morphology, cultures, interfertility etc that are used to delimit and classify taxa. There are thousands of collections of un-named agarics in herbaria, lacking full field notes and consequently important details necessary for proper characterisation as novel species. It is bad taxonomy to name these on material insufficient to allow proper comparison against known taxa, and it would be just as bad taxonomy to name a new taxon on a fragment of DNA without the same sort of proper comparison. Although we have the ICBN to regulate the nomenclature, there is no such document to regulate taxonomy. Encouraging good practices (such as not describing new species from fragments of DNA) will be up to referees and the scientific community.

GAMS (41200: 356) You will know me as being one of the most conservative figures in this debate. I do appreciate the elegance of David H´s proposal of epitypification of previously anamorphic taxa with a newly found teleomorph. The epithet then could be recombined into the appropriate teleomorph genus. This is much more elegant than the present CBS practice of just coining identical epithets for anamorph and teleomorph. But what then?

The proposed procedure can easily be applied and is desirable when a so far unispecific anamorph genus suddenly becomes holomorphic. When there are just a few, rarely cited species, I also do not foresee great problems. Thus the new genus *Conioscyphascus* might be superfluous for the teleomorph discovered in one out of 5 species of the anamorph genus. Problems really arise in large genera like *Trichoderma* and *Fusarium*.

According to present rules, the teleomorph name will prevail unless this is prevented by a set of additional rules, which have not yet been proposed. There will undoubtedly be a mycologist who in very good

faith coins *Hypocrea viridescens* comb. nov. and then the epithet is no longer available in *Trichoderma,* where we badly need it!

I continue to have the greatest difficulties with equivocal teleomorph connections. Not every *Trichoderma harzianum* will be *Hypocrea lixii.* The one-name approach that forbids the use of binomials for the alternate morph is quite risky in such cases. At least it will always require the application of molecular work for tricky taxa and we cannot demand this from all practicing mycologists. The rules of nomenclature must serve the whole mycological community.

Scott´s suggestion of postponing an effective date of change until after the post-Vienna congress is certainly wise. We will need a rather complex set of rules to avoid an abrupt change of the situation for pleomorphic fungi (not only those but in fact all so far anamorphic genera too) in spite of the unfortunate subtitle in the present Code.

Thus you will understand that I cannot yet endorse any of David H´s otherwise quite valuable proposals.

(41200: 357) Walter makes some interesting comments here but I believe there are some flaw.

First, in the second paragraph. As far as I am aware there is no concept in the Code of an anamorph genus - correctly there are genera where the type is anamorphic or genera where the type is teleomorphic - these may have been termed anamorph genus and teleomorph genus but the placement of species within these genera is a taxonomic decision and we are free, for example, to place species where the type is teleomoprhic in an genus where the type is anamorphic. In addition, a genus can be classified according to the opinion of the taxonomists - thus, Trichoderma can belongs to the Hypocreaceae. Nothing in the Code prevents this as it is pure taxonomy. The problem with the present Code is at species level in situations where Art. 59 applies, because priority is given to names whose type is teleomorphic. So, in a simple choice between a name in *Trichoderma* (with an anamorphic type) and a name in *Hypocrea* (with a teleomorphic type) the latter has priority. If the *Trichoderma* is considered monomorphic (i.e. no known teleomorph) Art. 59 does not apply and the name can serve as the basionym for a combination into Hypocrea. However, if this option is followed as soon as the teleomorph is found the name for this morph has priority for the holomorph and the rest is obvious, if the name exists it replaces the existing name in *Hypocrea* and if it has the same epithet a new name is required or a new eithet if it is undescribed - all of which is messy.

Under the Hawksworth Proposal 59.7, using the example cited by Gary, the following would happen. Gary selects an epitype for Eidamia viridescens representing the Hypocrea teleomorph and makes the combination into either *Trichoderma* or *Hypocrea* depending on which he prefers for the name of the holomorph. Whichever is chosen the proposed Article 59.7 will prevent the legitimate introduction of a name in the genus which was not chosen. The proposal does not require that the generic name whose type is teleomorphic must be selected. Please correct me if I am wrong in this analysis.

The next logical step from the Hawksworth proposals would be to add a voted example under Art. 9.7 to deal with existing name pairs applied to pleomorphic fungi so that the name of the anamorph can be epitypified with a teleomorph gathering and thus the name qualifies as the name of the holomorph under the existing Article 59. It appears to me that as currently worded it could be argued that Art. 9.7 does not specifically exclude this procedure. If I am correct the generic name *Trichoderma* would be the preferred choice for the *Trichoderma-Hypocrea* pleomorphic pair. A teleomorphic epitype should be selected for *Trichoderma viride* Pers. 1794 which then competes equally with *Hypocrea rufa* (Pers.) Fr. 1849 (basionym *Sphaeria rufa* Pers. 1796) as the correct name for the holomorph.

Walter´s 4th paragraph also has some statements/conclusions which I must question. First, there is only one *Trichoderma harzianum* - that gathering which is the type of the name. Whether that gathering is phylogeneticaly related to the type of the name *Hypocrea lixii* is for others to determine, not me. With respect, what Walter intended to say, I would suggest, is that not every collection identified as *Trichoderma harzianum* will be phylogenetically related to the type of the name *Hypocrea lixii.* So here we have misidentifications which are not a problem for the Code but for taxonomy. Is not the implication if we accept Walter´s comment that we should support the use of a name which applies to more than one species? And yes, I know, the Code serves taxonomy, but we have to accept that molecular work will always be required for tricky taxa. Many techniques are required to unravel the taxonomy of tricky taxa and not all mycologists have access to the necessary equipment to apply all these techniques. Take the simple example of recording lichens in the British Isles. Can everyone who has the latest 'state of the art' identification manual identify all taxa? The answer is a big NO because the use of TLC is required to identify 'cryptic' species. Do we reject such

classifications and names just because everyone does not have access to TLC? Quite clearly not; we never have demanded that the whole mycological community should be able to apply all techniques required for identifying all taxa and if this does not apply to all taxa why should it apply to that subset of taxa the correct name of which is determined by Article 59?

I repeat my support for the Hawksworth Proposal and solicit comments on the proposal to add a voted example to Art. 9.7 to deal with existing name pairs.

(41200: 356b) in reply to KIRK (357): What Paul calls flaws from my part, probably just reflects the fact that I am applying a generally adopted practice of sharply distinguishing between teleomorph-typified, i.e. teleomorphic, and anamorph-typified, i.e. anamorphic, genera. In other words, the effects of Art. 59 affect all anamorph-typified names, not only those of pleomorphic fungi, as the unfortunate subheading in the Code implies. Mycological practice is thoroughly entrechend with this spirit, and so is the acceptance of holomorphic genera as being superior to anamorphic ones. Before any major change can be enacted in Art. 59, this primacy will have to be modified somehow.

It would be a rather curious situation, if in *Trichoderma* the epithet *viride* would outweigh *rufa* by priority, which otherwise would have to be recombined into *Trichoderma rufum*. Which specialist of the group would have intended such a change?

Misidentifications may not be a problem for the Code. But they are a problem to the practitioner. Even in macromycete inventories, it is often necessary to distinguish between identifications of a taxon *sensu lato* and the same *sensu stricto* (e.g. in the Dutch inventory lists). For the identification of ambiguous anamorphs we have of course the possibility of melecular analysis, but if we have the exactly identified teleomorph at hand, this is also an important contribution to accuracy that can be expressed in the chosen name. (The same may also hold *vice versa*, when accurate identification is best served with work on the anamorph).

The inclusion of examples, voted or not, would certainly be of great benefit, whatever position we tak towards Art. 59.

SAMU (41200: 358) I write this a month or so after the internet discussion of these proposals. Over recent years we have talked so much about dual nomenclature and the desirability -- or not -- of doing away with Art. 59 but nobody has actually proposed anything concrete. I congratulate Dr. Hawksworth for making these proposals. I'm pretty conservative and would be happy seeing no change to the way that we name pleomorphic fungi and living within Art. 59. However, change will come and so-called unit nomenclature will rule. How that change takes place, the direction of that change, is important. The Hawksworth proposals proceed from the assumption that the proper name for the holomorph is that of the teleomorph. I disagree. In some cases, as judged by common useage, the more appropriate name will be that of the anamorph. In many cases the teleomorph phenotype is so generalized -- plesiomorphic -- as to leave it undiagnosable in the absence of the anamorph. In other cases the reverse is true. So, as regards the present proposals I support a ban on introducing a new morph name when the 'new' morph is part of a life cycle that includes a previously named morph (ana or teleo). The proposals need to go further. I suggest that members of user/taxon groups such as the ICTF *Trichoderma* and *Fusarium* subcommittees decide among themselves the appropriate holomorph name, whether it be that of the teleomorph or that of the anamorph, and then propose that name for conservation where appropriate. Species names in the rejected morph genus could be treated in the accepted morph name with the original authorship under which they were proposed. Various factors could determine priority including date and significance to the literature. There will be vast numbers of fungi that are not treated by special groups. These would typically be less commonly cited groups but, in any event, could be dealt with by volunteer or drafted committees or appropriate individuals. An approach like this would meet the need/desire for unit nomenclature while responding to the demands of stability of names.

DEMO (362) As may be clear from the Oslo discussions I am as conservative as the Secretary (It is not because I disagree on *Coprinus* that I do not agree with him on many things). The Rapporteur□s comments are also unbalanced in presenting the issue as well discussed among mycologists, without mentioning the majority in favour of statu quo. The dual nomenclature was introduced because it was useful for communication. For many years it should have been possible to reduce its use because of culture work, that it is even easier with DNA studies is not a fundamental argument. What users prefer is a name as short as possible for what they see that is *Penicillium* x not *Penicillium* stage of whatever else. For those who do not work routinely with DNA I should of course insist on the importance of Gary Samuels comment that the

sequence of a gene is only part of the global information. Sequences are useful part of descriptions, no more nor less than measurements colour and the like. Further think about the situation nicely described by McDonald in *Fusarium* where there is polymorphism of ITS and routine PCR techniques would place you in a group or another which have nothing to do with the real phylogeny. In basidiomycetes we also have the problem of dicaryons being made up of nuclei with different ITS.

NORV (370). Article 59 definitely needs modification and there is much good in the Hawksworth proposals. Unfortunately, I have not been able to devote as much time as I feel necessary to consider the full impact of the proposed changes would have on fungal taxonomy. Too many members appear uncertain as to the proper wording of the new paragraphs or even whether the modification is needed. More discussion is definitely needed before the Committee makes its formal recommendation.

DEMO (371). It would be best to vote on 183 and 184 first, because if (I hope not) they pass, then 185 and 186 may be useful.

Secretary´s comments: See also separate correspondence. An interim ballot on these proposals was voted in Dec. 2004: 4 Yes, 7 No.

REDH (396). No. I believe there might be better ways to change Art. 59 but we do not have time to discuss or rewrite them. This is best handled over time. Perhaps the Congress can establish a subcommittee to work on Art. 59.