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A CRITICAL APPRAISAL OF MODERN GENERIC CONCEPTS IN LICHENOLOGY

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Abstract: After a brief historical survey, some current trends in the delimitation of lichen genera are discussed. A widespread tendency is that of elevating any supposedly monophyletic group of species to genus rank. As the term ‘ monophyletic ’ has no lower limit, this is likely to result in an explosive inflation of new genera, in a severe loss of the information carried by generic names and in a high degree of nomenclatural disorder. Five criteria are proposed for the acceptance or rejection of new generic segregates: (1) DNA testing for monophyly, (2) phylogenetic analysis, (3) number of characters used, (4) number of species considered, (5) information content of the new splittings. Upon a critical analysis of several recent generic segregations, a more flexible approach to taxonomic ranks is recommended, and particularly, when most of the suggested criteria are not fulfilled, a more frequent use of the subgeneric rank, which does not imply name changes.

Introduction

Taxonomy has a twofold task: on the one hand it builds systems of classification that endeavour to reflect evolutionary affinities, on the other hand it gives names to the organisms (Davis & Heywood 1963). Its two faces, like those of the Roman god *Janus Bifrons*, point in opposite directions: one face towards progress and change, the other towards conservation and stability. Progress of biological knowledge is reflected in the creation of ever-changing classification systems, pushed forward by the flow of new data pouring from all branches of biology. However, names must remain reasonably stable if we want to guarantee an efficient communication system (*Nomina si nescis perit cognitio rerum*, Linnaeus 1737). The genus and species ranks have a special status in this context: they are an integral part of the binomial, and any modification above and below them has no serious consequences on the names we give to organisms. Although changes of species names affect one or a few taxa only, the International Botanical Congress of 1993 was persuaded to take drastic conservation measures against otherwise legitimate changes (Greuter & Nicolson 1993). Generic names have a much greater impact, as they might involve several specific and infraspecific taxa.

To appreciate the recent revolution in generic concepts of lichenized fungi, one has just to compare the list of genera accepted in any work published a few decades ago (e.g. Ozenda & Clauzade 1978), with those of some recent checklists (e.g. Esslinger & Egan 1995). This process is by no means exhausted, and many name-users, such as professionals, students, legislators,

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authors of textbooks, curators of herbaria or of databanks, already suffer from this state of things. Most taxonomists, however, agree that priority should not be granted to nomenclatural stability when stability conflicts with the main aim of taxonomy, that of producing more ' natural ' classifications. Although no serious taxonomist would ever disagree on this point, one might legitimately wonder how many of the recently proposed generic segregations have something to do with the main aim of taxonomy.

Historical Background

The ' generic revolution ' of the past decades appears as more than justified if considered in the light of the history of the genus concept in lichenology (Hale 1984). After the Acharian period, a major break in the delimitation of genera occurred around the middle of the 19th century. As early as 1837, the French cryptogamist A. L. P. Fée (1789–1874) stated that all species of a natural genus should have the same type of spores. Many of Fée's contemporaries in lichenology, however, objected to this thesis, as with the microscopes then generally available the observation of spore characters was considered too difficult for practical use. However, at the same time G. B. De Amici (1786–1862) was working on a new microscope with acromatic lenses, which was to open a new world for the astute observer (Nimis & Hawksworth 1994). Using De Amici's microscope, G. De Notaris (1805–1877), in his ' *Frammenti Lichenografici* ' (De Notaris 1846) paved the way to a thorough re-consideration of generic concepts based on spore characters, and on the structure and ontogeny of the ascomata (Krempelhuber 1867; Hale 1984; Nimis & Bartoli 1990). De Notaris published only a few papers devoted to lichenology, where he described such genera as *Bacidia*, *Biatorella* and *Buellia*, but his ideas were soon put into practice by several other lichenologists, such as A. Massalongo (1824–1860), V. Trevisan (1818–1897) and G. W. Koerber (1817–1885), who, altogether, described more than 200 genera.

In the same period, the Norwegian botanist J. M. Norman (1823–1903) was following an independent and parallel pathway by proposing 23 new genera, mainly based on spore characters (Norman 1852). Competition for the creation of new genera was as fierce as today, and several papers were published in haste to avoid loss of priority. The hasty description of new genera and the rapid demolition of old, well-established schemes produced—between 1850 and 1880—a state of confusion that threatened to end in nomenclatural chaos. It was at this point that W. Nylander (1822–1899), one of the leading lichenologists of that time, started a very harsh reaction against the so-called ' sporological ' or ' Italian–Silesian ' school (Krempelhuber 1867), by favouring much broader and, with hindsight, much more artificial general concepts. Nylander's approach found widespread acceptance among lichenologists in the second half of the 19th century, not only because of the authoritative position of this author, but also because his generic concepts were much more appropriate for that particular historical period.

In the second half of the century, due to the colonial politics of the main European powers, many new plants and animals were brought back from overseas to the newly created National Museums. This material needed to be

rapidly identified, and a few, broad, clear, albeit artificial generic containers were much more useful for filing the new organisms than the intricate plethora of names coined by De Notaris and his followers (Poelt 1991). This process culminated in the *Catalogus* of A. Zahlbruckner (1860–1938), where many artificial genera were adopted, whereas most of the genera created by the ' sporologists ' were subsumed into the list of synonyms. This monumental synthetic work was a kind of heavy sarcophagus lying upon the much deeper—albeit controversial—knowledge gained by the activity of the ' Italian–Silesian ' school: in fact, the Nylanderian approach, and its adoption in the *Catalogus*, conspired to hold back the recognition of monophyletic groups amongst lichen-forming fungi for over a century.

Attention to the creation of natural systems reflecting evolutionary relationships became eclipsed in mycology (including lichenology) just in the period when biologists generally were starting to embrace evolutionary concepts. Indeed, it is only after the Second World War, one century after De Notaris' paper, that the wind started blowing in the opposite direction. Several well-established genera proved to be an assemblage of wholly unrelated organisms, and have now been split into better defined monophyletic units, often being found to belong to different families and even orders rather than a single genus. During this process many long-forgotten names coined by the ' Italian–Silesian ' (–Norwegian?) school were resuscitated from oblivion. Examples are *Amygdalaria* Norman, *Bactrospora* A. Massal., *Baglietto*a A. Massal., *Brigantiaea* Trevis., *Celothelium* A. Massal., *Chromatochlamys* Trevis., *Dimelaena* Norman, *Diploicia* A. Massal., *Heterodermia* Trevis., *Lecidella* Körb., *Loxospora* A. Massal., *Parmotrema* A. Massal., *Placocarpus* Trevis., *Porpidia* Körb., *Psilolechia* A. Massal., *Pyrrhospora* Körb., *Sarcosagium* A. Massal., *Schadonia* Körb., *Scoliciosporum* A. Massal., *Speerschneidera* Trevis., *Sporastatia* A. Massal., *Thelygnia* A. Massal., etc. Examples from the lichenicolous fungi include *Cercidospora* Körb. and *Stigmidium* Trevis. A shortage of researchers has left this process of revision and rediscovery of past research far from completed. Such developments were a positive sign of lichenology waking up from an all too long sleep; this involved many name changes, but these changes reflected a real need for lichen taxonomy to become more modern and scientifically reliable, and were accepted without much argument by the lichenological community.

Many of the new segregations concerned some of the largest and most artificial crustose mega-genera such as *Lecidea* Ach. s. lat., and in these cases most of the segregated groups clearly had little to do with the rest of the genus. Differences in ascus structure, apothecial ontogeny, spore development and paraphysis morphology were considered as the best indicators of a polyphyletic origin. Such characters, however, are relatively uniform within most of the large foliose and fruticose genera. The separation of *Phaeophyscia* Moberg, *Hyperphyscia* Müll. Arg. and *Physconia* Poelt from *Physcia* (Schreb.) Michx. (Poelt 1965; Moberg 1977), of *Heterodermia* Trevis. from *Anaptychia* Körb. (Poelt 1965) extended the process of splitting to macrolichen genera. The large genus *Parmelia* Ach., however, remained almost untouched, both because of its large size, and because most of the characters usually considered as more relevant for generic segregations were quite uniform throughout the

genus. In the period 1965–1982 a number of authors, and especially the eminent American lichenologist M. Hale (1928–1990), segregated several groups from *Parmelia* s. lat. based on morphological and chemical, as well as anatomical characters (see Elix 1993). Further investigation, however, demonstrated that even these segregates were heterogeneous, which brought forth further new genera to description. A synopsis of the genera segregated from *Parmelia* by Elix (1993) listed 64 segregations, a number which has increased in the last five years. Some authors, especially European ones, were reluctant to accept such re-classifications. As stated by Elix (1993) the main disagreement was not in the grouping of taxa, but in their ranking: differences among the supposedly natural segregates were considered as insufficiently relevant to justify a separation at generic level. A typical example is that of *Neofuscelia* Essl.–*Xanthoparmelia* (Vain.) Hale. These two genera, and some other closely related segregates, differ only in the presence of a single type of pigment, all other characters, including ecology and distribution, being similar. Accepting them as distinct genera would imply something as recognizing genera such as ‘ *Flavobuellia* ’, ‘ *Phaeorhizocarpon* ’, etc. (see Kärnefelt 1997). Similar considerations were sometimes considered as a symptom of a typically European conservative thought fighting as a Don Quixote against more ‘ modern ’ and ‘ progressive ’ efforts to disengage lichenology from old, well-established, but non-natural classifications. Such a point of view, however, is questionable, and in the course of this paper I shall try to explain why. Combinations of slight differences, such as those used for splitting *Parmelia* s. lat. or *Cetraria* s. lat., are now increasingly used for separating new genera of crustose lichens. In fact, a situation in which a large, relatively homogeneous genus such as *Parmelia* s. lat. is subdivided into dozens of small genera, while a much more heterogeneous genus, for example *Caloplaca* Th. Fr., is maintained as a single, huge generic entity is illogical. Considering the high number of generic segregates within *Parmelia* s. lat., we can expect at least as many new generic names for *Caloplaca* alone. Are we ready to pay this toll?

Grouping versus Ranking

Nobody will deny that a group of species should be segregated from a genus when there is evidence that this is highly polyphyletic (e.g. *Lecidea* Ach., *Porpidia* Körb., *Clauzadea* Hafellner & Béllem., *Rimularia* Nyl., *Lecidella* Körb., etc.). In this case name changes really do reflect an advancement of taxonomic knowledge. However, when closely related groups are recognized as distinct genera just because they appear to be more ‘ homogeneous ’ among themselves, the question arises: where should we stop splitting? The problem can be settled only if we find some means of agreeing on the minimal acceptable distance among taxa that are considered to be distinct genera. Is it then possible to agree on a ‘ common generic concept ’? I anticipate that, in my opinion, the answer is negative.

The species concept is, at least in principle, based on an operational definition: a species includes all organisms that are able to exchange genetic

material. Such a definition might be questionable for several groups of vascular plants, and still more for fungi, but there is a general consensus that, in one way or another, ‘ species ’ refers to populations that work as reproductive–evolutionary units. Supraspecific ranks, apart from the obvious fact that they should be proved to be ‘ monophyletic ’, are much more difficult to define on an operational basis.

In the early years of numerical taxonomy (Sokal & Camin 1965; Sneath & Sokal 1973) some authors proposed operational criteria for establishing the limits of supraspecific ranks, the main criterion being similarity. The boundaries of supraspecific units were to consist of statistically measured similarity thresholds. Such an approach implies the recognition of ‘ genera ’ as a reality: large and small genera could be delimited in an objective way, as all species joining above a certain similarity threshold would belong to one and the same genus. If this were true, one could rightly maintain that large genera should not be split just because of their size. This assumption is still maintained, and mostly unconsciously, by several taxonomists, also because it is the nearest approximation to an ideal situation (see e.g. Goodall 1997). Reality, however, is likely to be different: evolution can produce more reticulated, multidimensional patterns than the simple one-dimensional hierarchical structures depicted by cladograms and dendrograms. Such patterns do not always fit into a rigid scheme formed by a few hierarchically arranged units, with those of the same rank said to be at the same ‘ level ’.

A biological classification is fundamentally different from a classification of physical objects, where we can use a periodical system with a few fixed criteria. In biology, it is even unpractical to consistently use comparable criteria for similar ranks, not even in related groups. Otherwise, according to Sipman (1997), we would be forced to attempt something like putting all insects in a single genus next to *Limulus* or some other genera of primitive Crustaceans. In lichen taxonomy this would lead, for example in the *Parmeliaceae*, to a single genus *Parmelia* with perhaps 1000 species next to dozens of genera for the 100 species until recently included in *Cetraria* s. lat. ‘ Genera ’ do not exist as such: what exists is the taxonomic structure, and we can use generic names to describe elements of that structure without worrying about the ‘ existence ’ of something called a ‘ genus ’ (Le Bois 1997). The lack of an operational definition of ‘ genus ’ renders the search of a common generic concept a vain and futile effort (Tehler 1997).

We have no way of deciding if a supposedly monophyletic segregate, as small as it can be, really deserves to be treated as a genus, and those who elevate any supposedly monophyletic group at the genus rank cannot be criticized on a sound scientific basis. The genus concept is a vague convention: if we decide for narrower genera, nobody can say we are wrong. This can easily bring us to consider grouping and ranking as one and the same operation, as happens today. However, if we fully accept this conclusion we should also accept its consequences: (a) blinding that face of our taxonomic *Janus* that points to the stability of names, (b) entering a very perilous sea: indeed, the term ‘ monophyletic ’ has no lower limit, and every single species could be recognized as an independent genus, which is obviously absurd. Again, the question arises: where should we stop splitting?

Some Criteria for Evaluating New Segregates

The impossibility of agreeing upon a common generic concept does not imply that lichenologists cannot agree, for the time being, on some principles regulating the adoption or rejection of new generic segregates, in order to maintain an equilibrium between the two faces of their *Janus*-like discipline. Common sense enough should prevent lichenology becoming a modern branch of Babylon: one should clearly distinguish between inescapable name changes based on relevant new knowledge of relationships, and more or less gratuitous changes in the ranks of admittedly monophyletic, but closely related, units. A separate discussion would be necessary for new genera established on the basis of one of the hundreds of possible ' objective ' cladograms based on one of the hundreds of possible sets of ' equally weighted characters ' (I am still waiting for a good explanation of why characters such as ' spore type ' and ' lower surface pale ' should be weighted ' equally ' . . .). In the following, five criteria to slow down the current inflation of generic names are proposed:

(1) *Testing for monophyly*. Some authors (e.g. Clerc 1997) suggest that any newly proposed genus, in order to be accepted, should be tested for monophyly on the basis of DNA data. This suggestion might sound rather ahead of its time—as it was Fée's suggestion to use spore characters to define genera—but DNA analysis is likely to rapidly become routine work in any taxonomic study (see e.g. Grube 1997; DePriest & Gargas 1997). Using molecular data, we are likely to obtain more reliable estimates of the evolutionary distances among taxa, which could even help settling the still unsolved problem of the operational definition of supraspecific ranks. Genetic distance could perhaps surrogate similarity–dissimilarity measurements based upon character states. We are still very far from that, but certainly a wealth of molecular data will soon emerge, which is likely to bring about radical changes in our understanding of currently accepted taxa. This is another reason for being more patient before proposing new generic names.

(2) *Phylogenetic analyses*. A somewhat similar suggestion (e.g. Mattsson 1997) is that a new genus, to be accepted, should be based on some kind of phylogenetic analysis. Actually, several new genera were proposed without any decent analysis of their mutual relationships, or of the relationships with other genera. When presenting a newly coined generic name, one should at least demonstrate or discuss the evolutionary distance between that genus and its closest relatives: are they more closely related to each other than to other groups? If so, treating them as subgenera would not require name changes (Sipman 1997).

(3) *Number of characters used for the segregation*. Lichenized fungi show a wealth of thallus characters, which are increasingly used for generic segregations. If only one character is involved, this is the weakest possible evidence. It could be easily in conflict with the next character to be discovered. Consequently, when a new genus is to be established one should ask: is there more than one character separating it from its closest relatives, or could any additional character provide an easy argument to make such a division futile? If so, users would be happy to keep the current generic arrangement (Sipman 1997).

(4) *Thorough analysis of the old genera*. Some large genera, such as *Caloplaca*, *Lecanora* Ach. and *Verrucaria* Schrad., probably are, indeed, very heterogeneous and deserve some generic segregations. Before this is done, however, one should acquire a thorough knowledge of the genus as a whole, which is a long and painstaking task. The hasty segregation of a few species from a large genus, without a general conspectus of the genus itself, is likely to result in unnecessary, and often ephemeral, nomenclatural changes. Before accepting new generic segregates one should check whether these are based on the analysis of a sufficiently representative number of species.

(5) *Information content of the splitting*. Names are used as carriers of information. Any classification of objects defines a hierarchical series of non-overlapping sets. When a given set is split into a series of subordinate sets, the information content of each splitting event can be easily measured on the basis of the total number of objects contained in that set, and the number in each subset. For example, a subdivision of six species formerly belonging to one genus into six different genera has a very low information content, because the information carried by the genus name is the same as the species name. In contrast, the information is higher if 60 species are subdivided into six genera of 10 species each, but it is lower again if the 60 species are subdivided into six genera, one with 55 species, and the others with one species each. This argument, which is unlikely to be accepted by all taxonomists, would justify the splitting of large genera ‘ just because they are large ’, even if the discriminating characters would not be recognized as such in other cases. The information content of new generic segregates could be used as an additional criterion for evaluating the opportunity of using either generic or subgeneric ranks for the segregated taxa when these appear to be closely related.

Some Examples

Some recent generic segregations were based on such ‘ heavy ’ characters as to render the adoption of the criteria suggested above as completely superfluous. In most of these cases, sexually reproductive characters, supposed to be highly conserved, were used as synapomorphies. *Absconditella* was created by Vězda (1965) to accommodate a few species formerly included in *Gyalecta* Ach., or *Dimerella* Trevis.; the number and the type of diagnostic characters are such as to clearly demonstrate monophyly within the new genus, and no close relationships with the genera in which *Absconditella* species were previously included. The same considerations apply for such recently described or resurrected genera as, for example, *Anisomeridium* (Müll. Arg.) M. Choisy (Harris 1973), *Clauzadea* Haf. & Béllemere (Hafellner 1984), *Gyalideopsis* Vězda (Vězda 1972), *Lecidella* Körber (Hertel & Leuckert 1969), *Ophioparma* Norman (Rogers & Hafellner 1988), *Pleopsidium* Körber (Hafellner 1993).

In contrast, several recently described genera have been based on one or few vegetative characters only, which are generally supposed to be more open to convergence and parallelism: examples are *Leproplaca* Laundon, segregated from *Caloplaca* (Laundon 1974), and *Fistulariella* Bowler & Rundel, segregated from *Ramalina* Ach. (Bowler 1981). These genera should, in my

opinion, be rejected, as they do not comply with most of the criteria specified above. *Leproplaca* is defined by the *absence* of ascocarps and by a more or less leprose thallus, but there is no evidence that these two related characters could have not been attained independently by different species of *Caloplaca*. The anatomical character distinguishing *Fistulariella* from *Ramalina*—the presence of a hollow medulla—proved to be in conflict with several other characters within the genus. A more tricky situation is that of the monospecific genus *Ingvariella* Guderley & Lumbsch, segregated from *Diploschistes* Norman on the basis of the structure of the excipulum (Guderley *et al*. 1997). The authors themselves, however, claim that the taxonomic value of this character within the family is still highly controversial. Pending a solution of this problem, I would be more inclined to treat *Ingvariella* as a subgenus within *Diploschistes* s. lat.

Catapyrenium Flot. has been recently split by Breuss (1997) into five supposedly closely related genera: such a re-grouping added enormously to our understanding of *Catapyrenium* s. lat., but there might be some reason to wonder whether the new segregates really deserve to be treated at the generic rank. In his introduction, Breuss (1997) expresses the modern lichenological philosophy concerning genera: ‘ the recognition of clearly defined monophyletic groups justifies, following the newest trends in Lichenology, to split earlier large genera into narrower groups ’. All of his generic segregates, however, are introduced: (1) without evidence for monophyly, (2) in the absence of phylogenetic considerations, (3) on the basis of one or a few characters only, (4) with a deep, worldwide conspectus of the genus, which, however, (5) contains a relatively small number of species: *Catapyrenium* s. str. as re-defined by Breuss (1997) includes only six species, while the other four new genera were introduced to accommodate 17 species only. In my opinion, such segregations, which fulfil only one of the five criteria suggested above (no. 4) are, at least for the moment, perfect candidates for good subgenera.

Parmelia s. lat. is a very large genus, and there may be some justification for splitting it into many new, small genera based on a combination of subtle anatomical and chemical differences. Due to the high number of species, the information content will remain high after some segregations, provided that not too many genera will be left with one or a few species only (which, however, is not the case today . . .). Other authors, however, tried to apply the same philosophy to much smaller genera. *Cetraria* s. lat., for example, has been subdivided into several small genera, on the basis of subtle, and not always clearly discriminating, combinations of characters (see e.g. Kärnefelt *et al*. 1992; Randlane *et al*. 1997). In both cases the new generic arrangements are based on a deep knowledge of the species worldwide, but most segregates need to be tested for monophyly, and, even if this were to be proved for all of them, the differences among many of these genera are so slight that they hardly justify segregations at genus level, especially if the resulting splittings contain only one or a few species. The information content of such splittings is becoming dangerously low, and this is the main reason why Sipman (1997) proposed maintaining supposedly primitive genera, where basic characters are variable, such as *Cetraria* s. lat. and *Omphalodium* s. lat.

The recent splitting of *Porina* Müll Arg. did not fulfil any of the criteria specified above, except perhaps no. 5. Two different rearrangements of this genus were proposed by Hafellner & Kalb (1995) and Harris (1995), who recognized several segregated genera. However, McCarthy & Malcolm (1997), from screening a high number of species, showed that the characters used to segregate the new genera—based on the analysis of a few ' representative ' species only—form a continuum within a single, variable genus. This controversial state of affairs shows what can happen when criterion no. 4 is not fulfilled. Similarly, before accepting the segregations of *Niebla* Rundel & Bowler, *Vermilacina*, *Trichoramalina* Rundel & Bowler and *Fistulariella* from *Ramalina* I would rather await a reconsideration of *Ramalina* s. lat. worldwide (see e.g. Rundel & Bowler 1978; Krog & Østhagen 1980; Spjut 1995).

If the criteria suggested here were considered by most authors of checklists and regional monographs, some unfortunate situations of the recent past could be avoided. For example, many ' modern ' genera were readily adopted in some checklists (e.g. Egan 1987), but were again rejected within a few years (Esslinger & Egan 1995). Among them we can cite: *Apatoplaca* Poelt & Haf., *Buelliopsis* A. Schneid., *Fistulariella* Bowler & Rundel, *Leproplaca* Laundon, *Pseudosagedia* (Müll Arg.) M. Choisy, *Zamenhofia* Clauzade & Cl. Roux, etc. Even the genus *Cladina* (Nyl.) Nyl., which, however, was not accepted in many European floras, seems to be better treated at a subgeneric rank (Stenroos *et al*. 1997), and this will necessitate several nomenclatural changes back to *Cladonia* P. Browne for those who accepted it.

Conclusions

Many of the new generic segregations arise from a very human, and fully understandable, expectation: that of seeing our personal names connected to the name of a real object. As stated by Mattsson (1997): ' The naming of things is such an important issue that mankind has been aware of this since the oldest times (Genesis, 1: 29–30, and 2: 19–20). Naming gives power over the named, and the spreading of '' our '' names gives us some power even on other persons to have to use them '. This is, in my opinion, the main reason why the subgeneric rank is rarely adopted by lichenologists, and this reason has, admittedly, little to do with science. It is all too easy to ridicule this attitude, but we cannot forget that science is made by people, and that behind a new *name* there can be years of hard work. However, the subgeneric rank has an enormous advantage: it does not affect the binomial.

When the taxonomy of a given group is not settled, and if there is no clear evidence that the segregates are unrelated to the old genus, the tentative segregation should preferably occur at subgeneric level; the new taxonomic information will be there, without causing unnecessary, and often provisional, name changes. Before accepting the segregates at generic rank, one should be certain that: (a) most species within the old genus have been taken into consideration, (b) the characters circumscribing the segregates are really discriminatory, (c) the new genera are tested for monophyly, (d) the phylogenetic distance among segregates is large enough, (e) that, at least, the

information content of the new splittings is high. Decisions based upon these criteria will always be subjective, but the author of the new genera should, at least, provide the lichenological community with the means to take them. Pending this information, I would suggest to be prudent in accepting new generic segregates.

The priority question, however, will still remain a problem: several genera are being proposed simply because ' if I don't do it, someone else will '. This problem could have an easy solution, that of adopting a less rigid approach to taxonomic ranks: if an author thinks that he can propose some segregations within a relatively homogeneous genus, he could well propose, at the same time and in the same paper, two different taxonomic arrangements, one in which the segregates are recognized as genera (and thus the priority questions are settled) and an alternative one, in which the segregates are ranked at subgeneric rank (and in this way prudent people can avoid name changes). This flexible and, in my opinion, much more practical approach is, unfortunately, not yet permitted by the current code of Botanical Nomenclature.

Janus Bifrons had two faces, but he was not the god of conflict and schizophrenia. On the contrary, he was a god who knew the future and the past, a wise god protecting doors (stability) and streets (movement and change), a god symbolizing the polarity of forces that maintains the equilibrium of Nature. Modern lichenology tends to identify herself in the face pointing to the streets, and this is a likely symptom of her renewed youthful vigour. However, it is not wise for lichen taxonomists to blind the other face of *Janus*, that pointing to the doors. The XV International Botanical Congress adopted the following resolution: ' Considering the great importance of a stable system of scientific names of plants for use in the pure and applied sciences and in many other domains of public life and economy . . . the Congress urges plant taxonomists . . . to avoid displacing well established names for purely nomenclatural reasons, whether by change in their application or by resurrection of long-forgotten names ' (Greuter & Nicolson 1993). Names are a very important and delicate matter, and whereas taxonomists must be free to experiment with classification, they cannot play with names. They can do their job without causing trouble for anyone, if they avoid touching the genus and the species rank when this is not strictly necessary. However, who has to decide when such changes are ' really necessary '? Certainly, neither a more or less democratically elected International Commission nor a more or less restricted group of ' prominent ' taxonomists, as somebody has suggested.

Taxonomy is science, and science is free. Anybody can propose any name change, if they think that there is a reason for this. Editors and reviewers of journals can certainly play some role in moderating the inflation of new names, but we cannot ask them to become a modern taxonomic branch of the Inquisition. If an author really wants to publish a new name, this can be easily done in local journals without peer reviewing. However, as Wetmore (1997) states: ' do we have to blindly follow anything that is published? Many do, but the best do not, they evaluate each one, as my older master taught me '. Then, the greater responsibility falls upon authors of checklists and regional

monographs: it is up to them to show taxonomists whether they deserve immortality, or whether their newly created names deserve, at least for the time being, to fall into the inferno of synonymy.

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