

THE CLAVARIOID RAMARIA SUBGEN. ECHINORAMARIA

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This article investigates the re-arrangement in classification proposed for this subgenus in the monograph by R. H. Petersen (1981). It is found to be wanting in many ways.

The recent monograph by R. H. Petersen (1981) deals with 35 species from all parts of the world and, in his introductory words, has 'brought to order' the subgenus. As the only one who could be held responsible for the previous disorder, I perused the work with growing wonder that has led to this criticism. Many differences between the author and myself are glossed over unexplained if, indeed, mentioned; there are many misleading directions, careless mistakes, omissions, and idiosyncracies. Among the tropical species I find what I can only call colloquially a rare old muddle. The specialist may feel bound to tussle with the work but the attentive student will be bewildered. My advice is to consult original articles, wherever cited, to ascertain the facts.

The approach is not prepossessing. Nowhere are we told how the subgenus is to be recognised. A lengthy subgeneric description relays features which refer to *Ramaria* as a whole and it is not until line 15 that the decisive character appears without indication. The author makes 6 new species, 1 new variety, 7 new combinations, and 6 'stat. nov.'. This information is not in the author's nomenclator or in the index; it can be gathered only by turning one by one 144 pages of the text. During this labour, I met variations in type-setting for equal taxa and in the citation of authors, e.g. Petersen and Pet., as well as species without author's name. There have been two previous systematists of this surname, J. B. Petersen (1887–1961) and O. G. Petersen (1847–1937); strictly, the citation should be R. H. Petersen. Then I noticed that all of the four colour-plates covered more than one species or variety and that, while the legends were numbered for distinction, the individual pictures were not. The expert may puzzle them out but the student will have great difficulty.

The descriptions are full but very confusingly prolonged with trivialities. Italics are not used to emphasize salient points which are hard to trace through the diagnostic, yet faulty, key. Thus it is difficult to learn the author's concepts of the species. The reader is left to work them out and, in doing so, will wonder why details in one case are characters in another, mostly in the lesser understood. Indeed, criteria in the key are often ignored or distorted in the ensuing text. Under every species there are references for

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nomenclature but few, if any, for biological or floristic information. Some mycologists who have contributed much to the subject, such as Petch in Ceylon, Pilat in Czechoslovakia, and Maas Geesteranus in Holland, are practically forgotten.

The text opens with a historical account. The first sentence leads us to infer that Persoon began the study of *Echinoramaria* in 1797. We are, then, taken through the rigmarole of what I call the dark age of mycology, in growing wonder that the history of *Ramaria* and *Lachnocladium*, so much confused with *Echinoramaria*, has no bearing on the subject. The history ends on p. 6. We read on and come, almost incidentally, on a line of nomenclatural reference (p. 35) which implies that the subgenus was not created until 173 years later. There is no mention of a type-species here or anywhere in the monograph. Is it Persoon's *Clavaria abietina*? If the reader refers to the original account of the subgenus in 1970, he will find that it is a species unknown to Persoon who had, in fact, no thought about such a subgenus. It was first mooted as *Echinospora* (Corner, 1950) but not instated as *Echinoramaria* until twenty years had elapsed and provided satisfaction. The real, if very brief, history of the subgenus is omitted. Misgivings multiply as we read such generalisations that the north temperate species have relatively small spores, and find later in the text that the north temperate *R. americana* and *R. broomei* have very large spores. The misspelling of the Linnean Society of London (p. 64, 71, 182) seems unscholarly.

In the section on taxonomic characters and in several specific descriptions it is often not clear whether the author refers to living or dried fruit-bodies; it is a common and tiresome failing in many monographs. Agglutinated hyphae in these clavarias must refer to the dried specimen; darker or paler branch-tips may refer to the living or the dead; we are not sure. There is no explanation how branching alters as the fruit-body grows, how the hymenium may thicken, and how the sterile hymenium may form; it is as if no one had cared. The last sentence on p. 13, about the colour of the fruit-body in tropical species is far from the truth; it was through the study of these species that the importance of the feature was realised.

Spores become a main consideration. The author supplies SEM photographs of the spores of all available species. The trouble is that these pictures, however excellent, seem to numb understanding. In this monograph they are in such disorder that comment is difficult. The spores are shown from different angles, in different positions, and at some thirty different magnifications ($\times 5400$ to $\times 22500$). Different examples of one species are on different plates where they are mixed up with others at different magnifications. The author distinguishes five sorts of spines or warts but we are not shown how the spines develop, whether sharp from the first or as warts that sharpen, at what spacing, how they may join, and whether or not spine-tips are broken off in the course of preparation. There is no systematic co-ordination of the details. When the pictures have been sorted out, it is still uncertain whether those with short or obtuse spines or warts represent mature or immature spores. In this connection a paragraph (p. 19) debates whether the spores of *R. suecica* refer to the species to *Echinoramaria* or not, without noting that it has already been done (Corner, 1970).

In a section on 'Interpretation of Spore Statistics' the author considers the sporograph and has a graph of a 'sporogram' (relation of length to width of the spore). In not understanding, however, that this relation is hyperbolic and refers to the volume of the spore which refers, in turn, to the volume of the basidium, the author overlooks spore-number, basidium-width, and the ampoule-effect. The result is the confused information in his uncritical Table 1 and his faulty conclusions. He alters for no reason the notation for these relations which I introduced in 1947 and have used consistently (Corner, 1972) in order to emphasize the most important relation d/w (spore-width to basidium-width). On such uncertain ground he complains that I erred in comparing the spores of *R. botrytis* with those of *R. formosa*, though both belong to subgen. *Ramaria*; and he misleads by stating that the spore of *Echinoramaria* may be boletoid.

In the subgeneric description it is not mentioned if the spines are coloured, if the spores have oil-drops, or if the spore may be in any way amyloid. 'Odor none to mild' is not what others have written.

The introductory part of the monograph concludes with some meagre remarks on phylogeny. The careful reasoning and thoughtful conclusions of others are not permitted in this prerogative where *Gomphus* is just another spore.

In the ensuing descriptive part of the work I find that the three series which I had offered purposely and comprehensively on the same page, had clearly distinguished, and had provided with types, have been brushed aside without comment, not even historical. In their place the author puts two sections, *Dendrocladium* (Pat.) Peterson on p. 44 and sect. *Flaccidae* (Corner) Petersen in different type on p. 100. There is no direct comparison between them. The first is given a lectotype, the uselessness of which is at once apparent, and the second, which is a plural adjective and not a sectional substantive, has no type. As I turned page after page to discover what the author intended I came upon his confession that he did not know himself, that he could not distinguish the sections, and that for this lack of reason their species were mixed up throughout the one diagnostic key for their determination. By taking botanical names published by others, altering the position or status of these names, and adding his own, the author avoids a critical latin diagnosis and saddles botany in a way that neither he nor another can ride.

For sect. *Dendrocladium* Petersen makes *Lachnocladium giganteum* Pat. a lectotype, and gives as an exact synonym the genus *Dendrocladium* (Pat.) Lloyd (1919). Lloyd, however, did not include *L. giganteum* in his genus for which a totally different species had been chosen for the type by Stevenson & Cash in 1935. Petersen omits these points which vitiate his intervention, though he could hardly have been unaware of them (Corner, 1950, 1970). What, then, is *L. giganteum*? In the works just cited, I referred it either to *R. cyanocephala* (Berk. et Curt.) Corner or to *R. zippelii* (Lév.) Corner because, in the absence of essential colour-notes, *L. giganteum* could not be identified more exactly. Now, *R. cyanocephala* is the type of subgen. *Echinoramaria*, which this monograph is at pains not to mention. Its author splits the type-species into *R. grandis* f. *cyanocephala* (Berk. et Curt.) Petersen (listed as var. *cyanocephala* in the index) and *R. zippelii* f. *aeruginosa* comb. nov. in different print and without the symbol (Pat.) Pet.

So the type of the subgenus is degraded to the minimum of nomenclatural status as two forms of different species. Whether this has brought the subgenus to order needs examination.

R. cyanocephala is a wide-spread and common, tropical and subtropical, species extremely constant though habitually altering in appearance, living and dried, as it grows. I have studied it from many living specimens in Asia and tropical America. It produces some of the largest fruit-bodies in the genus and these, with slow growth, are long-lived. My reasons for holding it apart from *R. zippelii* were clearly given in 1950. Petersen, in disregard, states 'Corner apparently assumed that only one blue-tipped fungus existed' and infers (p. 99) that Corner was misled by his own illustrations. Yet, Corner had studied the growth of individual fruit-bodies and recorded their change in colour, form and texture. What, then, are the two blue-tipped forms of the two species to which Petersen refers? They are distinguished in his diagnostic key (p. 37, item 5) by features of the branch-tips and dried appearance. Both features are simply the difference between young and old fruit-bodies of *R. cyanocephala*. The young fruit-bodies are *R. grandis* f. *cyanocephala*; old fruit-bodies are *R. zippelii* f. *aeruginosa*; I have watched the one grow into the other. Neither belongs with the species to which Petersen refers them. The mistake arises from that failure to understand the branching of the fruit-body, which I have mentioned, and failure to follow the course of development.

Now, *R. grandis* dates from *Clavaria grandis* Peck (1902) and *R. cyanocephala* from *Clavaria cyanocephala* Berk. & Curt. (1868). By the rules of nomenclature, one should write *R. cyanocephala* f. *grandis* if this reduction is correct. However, according to the author's records, *R. grandis* f. *grandis* lies outside the distribution of f. *cyanocephala*, which would not be expected of a forma. Thus it happens that an elementary error induces the author to split a species into two, to associate the halves wrongly, to break the nomenclatural code, and to maintain this course as the restoration of order.

I return to *L. giganteum*. Petersen's description is compounded from four sources, one of which is *R. zippelii* var. *gracilis* Corner; it supplies the only information on living colours. In recording this variety from the Malay Peninsula, the Solomon Islands, and Brazil, I published colour-notes and the fact that the spores were consistently smaller than those of var. *zippelii* and *R. cyanocephala*, as well as those of the types of *L. giganteum* and *L. echinospora*; they may be identical with those of *L. olivaceum* as given by Petersen (Table 4, p. 68). Petersen overlooks this difference as well as the occurrence of var. *gracilis* in the Malay Peninsula, as type-locality, and the Solomon Islands. Therefore I do not consider that var. *gracilis* belongs with his *R. gigantea*. A critical point for him is whether the basidia bear 2 spores (actually 1-2) or 2-4 spores (actually 2-3-4, rarely 1). Var. *gracilis* has 2-3-4. Concerning the type of *L. giganteum*, and therefore the type of sect. *Dendrocladium*, Petersen is oddly silent on this point just as he must be on the colour of its branch-tips. The large spores of *L. giganteum*, indistinguishable from those of *R. cyanocephala*, suggests that its basidia were (1-)2-spored and not 2-3-4-spored as Petersen gives in his compounded description. He does not explain the issue. According to his key, the species nearest to *R. gigantea* is *R. pancaribbea* Petersen. Two distinctions are given, the first in spore-ornamentation and the second in the colour of

the fruit-body, but as this is not known for *R. gigantea* except as var. *gracilis* which is to be excluded, there is only the first difference. Examination of Petersen's plates of the spores shows more difference between the two examples of *L. olivaceum* (a synonym of *R. gigantea*) than there is between *R. pancaribbea* and the other synonyms of *R. gigantea*. It may be that *L. olivaceum* is *R. zippelii* var. *gracilis* and that *R. gigantea* (from French Guyana) is *R. pancaribbea*, but *R. gigantea*, as compounded in this monograph, is unrecognisable. It is no wonder, therefore, that the whole section *Dendrocladium* is unrecognisable. I note that *pancaribbea* is the spelling in the text, the index, fig. 28, Plate 7 and Plate 12, but that it is *pancarribea* in the key and the colour-plate. This resurrection of *Dendrocladium* reveals the dead hand of that necrotic nomenclature from whose annihilating grasp biology must escape. We are returned in this monograph to the dark age of mycology.

As regards *R. zippelii*, Petersen chooses a neotype in the apparent type of *Clavaria phaeocladia* Pat., but he does not say what was the colour of the living fruit-body (so essential for distinction from *R. cyanocephala*) or whether the basidia are 2-spored, or why Patouillard described the spores as smooth. This neotype is equally unidentifiable. Omitted, too, is the habitat of *R. zippelii* and its occurrence in tropical Africa. Whereas I reduced *Lachnocladium hookeri* to *R. zippelii* on account of its large spores, though with doubt because the colour of the branch-tips was not known, Petersen refers it to *R. mutabilis* of sect. *Flaccidae* with much smaller spores, and omits reference to my conclusion. Original work must be consulted.

Next, I enquired into *R. fragillima* which, as *Clavariella fragillima*, van Overeem had taken up. Petch and I followed him because there was no denying his contention. In this sense the name has been used for over fifty years. Petersen states that our work was based on folklore (p. 21) and, unfamiliar himself with this wide-spread tropical fungus, makes it part of *R. cokeri* Petersen (1976); he discards *R. fragillima* as a *nomen dubium* and seeks justification in the falsified remark that I had said that my description of *R. fragillima* was 'seriously at odds' with the original description. I never wrote that, but pointed out two discrepancies of little weight. If *R. cokeri* were identical with *R. fragillima*, I would reduce *R. cokeri* accordingly and note that the purpose of nomenclature is the conservation of names, not their frivolous debunking. When it comes to comparing the two species in detail, it will be noticed that Petersen's colour-plate of *R. cokeri* does not show the red branch-tips invariably present in *R. fragillima* and that there is considerable difference between their spores as shown in Petersen's plates. Their identity has yet to be proved.

In several places, especially in the specific key, Petersen insists that only 2-spored basidia (not those with 2-3-4 spores) have yellow or yellow-brown contents. Reference to my description of *R. fragillima* will show that its living basidia with 2-3-4 spores have yellowish brown contents. Likewise I noted this for the (2-)4-spored basidia of *R. invalii*, which Petersen reduces to *R. eumorpha* in his dubious sect. *Flaccidae*. He overlooks these unsolicited facts which prove that his dictum is wrong.

R. zippelii var. *crystallospora* becomes *R. crystallospora* (Corner) Petersen with the false remark that Corner gave no description. Its fruit-bodies agree with those of var. *zippelii*

in all points, as I did explain, except for the markings on the spores. I added that in one collection several spores had scattered spines as in var. *zippelii*; hence I saw no reason for supposing a specific difference. Petersen has a SEM illustration of one spore and a line-drawing of two others. None shows the typically cristate marking, as illustrated by me and corroborated by Perreau, but all show the intermediate character of var. *zippelii*. Petersen refers to a sporograph but, without analysis into spore-number, it is not helpful. Modern mycology tends to deny specific variation; splitting, such as this, prevents it. A similar case is *R. campestris*, described originally as *R. zippelii* var. *campestris*. It is extremely close to var. *zippelii* and differs mainly in its habitat in open grassland; Petersen, as noted, fails to give the habitat of *R. zippelii*. He compares his *R. campestris* with his *R. cokeri* but forgets its different habitat in forest soil and that it should have red branch-tips. For *R. campestris* he gives the tips as concolorous with rest of the fruit-body in the key and as white in the description where the fruit-body is not white.

As regards *R. broomei* which seems to take over from the error of *Phaeoclavulina nigrescens* introduced by Coker and Donk, I note that in discussing its distribution Petersen overlooks Imai's record from Japan and omits from his description the all-important spore-number. This species introduces the difference between Petersen's treatment of his first nine species and mine. I preferred the character of the fruit-body; he prefers, without mention of my point of view, the spore-number of the basidium. That most of his 2-spored species have their 2–4-spored counterparts in fruit-body character escapes his notice; thus, *R. apiahyana* – *R. fragillima*, *R. broomei* – *R. americana*, *R. cyanocephala* – *R. pancaribbea* (? *R. gigantea*), *R. guyanensis* – *R. longicaulis*, and *R. zippelii* – *R. grandis*, with *R. zippelii* var. *gracilis* and var. *crystallospora*. In many homobasidiomycetes 2-spored states occur as variants or close allies of the 4-spored. I still hold this opinion for subgen. *Echinoramaria* which these pairs of species re-inforce, and continue to uphold the red, white, and blue of *R. fragillima*, *R. zippelii*, and *R. cyanocephala*.

Sect. *Flaccidae* untypified and indefinable, sets off with one of those endless nomenclatural wrangles that are so useless and frustrating for the progress of science. It is over the correct botanical name for the common little virescent clavaria of northern coniferous forest, which Petersen says is rare. He calls it *R. abietina* with a prolonged and one-sided account without reference to the alternative standpoint which advocates *R. ochraceovirens*; he dumps this name as a *nomen dubium* because he cannot find an original specimen, though the original description has left no doubt about the identity of the fungus. This is in contrast to his procedure with *R. zippelii* which he upholds though it, also, has no type-specimen and was accompanied by a much poorer description; as noted, he denies this privilege also to *R. fragillima*. So pundits vacillate, and what does it matter? What we need is stability that will not cloud knowledge with synonyms. The code, or rules, is inadequate. I am reminded of the breadfruit tree which has three apparently plausible botanical names without decision; we use the vernacular. It would serve science better if these disputes were settled by an international referee tossing a coin or drawing names from a hat.

The dichotomy of the key which begins the species of sect. *Flaccidae* has 'Fruitbodies lignicolous' versus 'Fruitbodies terricolous', which means also humicolous. Anyone who

has collected in primeval forest will know how difficult it may be to distinguish very rotten wood, of which there is plenty, from humus. However, by this uncertain means, Petersen extracts *R. articulata* and *R. camellia* as lignicolous. For the first there is Singer's field-note 'rather superficially on dicot wood'. For the second, Petersen overlooks the type-habitat 'inter folia ad humum in silvis'. At the outset the key does not work; it goes on faltering. *R. ochracea* is put among the terricolous though the type-collection has the note 'ad truncos putridos', as Petersen quotes but fails to enter in his specific description. He would refer to this species some collections from South America without mentioning their habitat. It seems that he would treat as other synonyms *R. flaccida* var. *chilensis* (terricolous) and *R. intricatissima* (on fruits of *Eucalyptus*). The criteria in the key are nullified in the ensuing text and it becomes impossible to understand what the author means by a species in a group where species-mongering is rife. There is the naïve distinction between under bamboos in contrast with under trees which, of course, may grow over or under bamboos. Thus a bamboo-species is extracted as *R. capucina* (Pat.) Pet., which is a superfluous name (Corner, 1950, 1970). The equally unreliable feature of the unilateral hymenium is introduced without explanation of its fallibility. *R. eumorpha* is said to have dull-coloured fruit-bodies which does not agree with the well known *R. invalidii*, reduced as a synonym. For *Clavariella subspinulosa* and *Clavaria spinulosa* no author is given; neither is in the index or nomenclator; their synonymy is confused. *R. flaccida* is thought to be rare; it has also escaped the nomenclator. *R. incognita* has no habitat. The vinescence of *R. myceliosa* is confused. *R. subdecurrens* has no author. For the last species in sect. *Flaccidae* (*R. tropicalis*) the only other with which it is compared is *R. pancaribbea* of sect. *Dendrocladium* thus finalising the futility of these impositions.

Another dichotomy of the key is between spores over or under a mean (average of ten measures) length of 5.6 μm . As this implies averaging spores from 2–3–4-spored basidia, I am not convinced that ten measures are enough. I compared Petersen's results for *R. cyanocephala* and *R. fragillima* with the measures of fifty spores given by me (Corner, 1950), and found discrepancies of 7–8%.

The text ends with the section 'Nomina Miscellanea Deposita' where some twelve species or specific names are, through sundry pretexts, deposited; perhaps 'Disponenda' was intended. Thus, the first, as *Clavaria acutissima* Berk., is deposited because Peterson is puzzled. He refers this fungus with yellow-brown spores to *Clavulina* without a reason. The last is *Clavaria tubulosa* auct., which is in fact *C. tubulosa* Fr. with a type-specimen that Petersen overlooks. It is the common pantropical *Lachnocladium tubulosum* (Fr.) Lév. (Corner, 1970). I note that *R. angustata*, *R. flaccida* var. *longiramosa*, and *R. fuscobrunnea* have escaped deposition as they have escaped this wayward monograph entirely. I close the book with thorough distrust.

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