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ON STAUDE'S NEW GENERIC NAMES FOR AGARICS

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SUMMARY

The author concludes that the generic names for agarics first introduced by Staude (1857) have been validly published.

When I drew attention to Staude's forgotten "Die Schwämme Mitteldeutschlands" (*1857*), I felt obliged to conclude that the new generic names for agarics appearing in that work were validly published (Donk, *1949:* 319-320), and I still am fully convinced that this conclusion must be upheld. Rogers (*1950:* 22) submitted that Staude, although referring to *Collybia* and other genera as G[attungen], did not definitely accept them as genera, since he continued to refer species under them to *Agaricus*, thereby implying that they were only subgenera or sections; and that Staude, therefore, dit not validly publish these generic names.

Rogers's conclusion is untenable for several reasons. If his argument became generally accepted, quite a lot of generic names might be murdered by it: the number of generic names established without simultaneously published new combinations with the new generic name, like those without any mention of species, is considerable. Such an attitude would not only be undesirable from a practical point of view, but would also disregard the declared object of the Code to promote stability of nomenclature.

The Code has deliberately and carefully avoided the stipulation that a new generic name requires simultaneously published new combinations; it has even refused to rule that new generic names ought to be associated with binomials, as was stipulated by the former American code! Certainly this was not done solely to accomodate such special cases as, for example, Tournefortian generic names published after 1753 as an overflow from the pre-binomial period: the decision was for general application.

Staude emphatically marked the names in question as generic ones; not only in the introductory pages, but also throughout his more detailed treatment of the agarics he preceded these generic names with "G[attung]" and added generic descriptions. He supplied the information that, "the genera . . . to follow are all according to Fries. Cp. Systema mycologicum

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etc." [1] That he did not make any new combination is incorrect: I already pointed out that (as regards agarics) a couple of recombinations were made under *Pleuropus* and one under *Crepidotus* (Staude, *1857:* 71). Under the generic name *Coprinus,* already established and generally accepted in his day, Staude is inconsistent in exactly the same manner as under *Pleuropus:* some specific denominations are combinations with the corresponding generic name, others, with *Agaricus.* Under *Gomphidius,* another previously established generic name, all three specific denominations are combinations with *Agaricus,* but I should feel guilty of deception if I stated that I really believed that Staude did not definitely accept that genus. In any case, Rogers may have to admit that *Crepidotus* was validly published by Staude, for the only species treated by that author bears a duly recombined name. Yet Rogers may still want to argue that by not having 'definitely accepted' the other equivalent taxa as genera, Staude thereby also implied that *Crepidotus* and the earlier published generic names *Coprinus, Russula, Gomphidius,* and other ones were names for subgenera and sections, too. One reason why Staude did not make recombinations may be that the agarics were popularly known as species of *Agaricus,* and another that his plates bearing the combinations with *Agaricus* may have already been finished when he prepared the text.

Rogers has forgotten to tell us of which *genus or genera* Staude intended his 'subgenera or sections' to form a part. Perhaps of that author's "10. Fam. Agaricoideae. Blätterschwämme" or of the subdivisions thereof, "1. Abtheilung. Dermophylli," "2. Abtheilung. Pleurotinae," "3. Abtheilung. Cantharellineae," and "4. Abtheilung. Agaricini"? But then must Staude not be corrected once more? By accepting the view that Staude implied that his genera were only subgenera or sections, must we not also accept the further implication that the family and its *Abtheilungen* were genera or subgenera? Or was Staude after all quite consistent in the ranks he assigned to his groups? Might it not be Rogers who was inconsistent when he magnified something quite unessential and irrelevant into a major objection to valid publication, an omission deliberately permitted by the Code? The Code keeps the two elements Rogers confused distinct. Art. 42 declares Staude's new generic names unequivocally validly published, while Art. 43 (1) which he invoked deals with quite different matters as may also be gathered from a study of its text and examples. Moreover, Rogers failed to state why, with his interpretation of the facts, these

[1] "Die folgenden Gattungen . . . sind sämtlich nach Fries. Vergl. Systema mycologicum etc."—Staude (*op. cit.* p. xxv). In his bibliography on pages 139-140 he fully cited both Fries's "Systema mycologicum" and "Epicrisis." For an "Extract from Staude's summary of the classification of agarics," see Donk (*1949*: 385-387).

generic names would not stand as validly published alternative names to the implied subgeneric ones.

If Rogers were correct, I fear that the next victim would be Quélet's "Les Champignons du Jura et des Vosges" (1872), and that such statements as the following would become standard practice. Quélet, like Staude, although dividing the agaric "F[amille]" into "Series" and "Genres," did not definitely accept the latter as genera, since he continued to use in connection with *Tricholoma, Clitocybe, Collybia,* and so on the masculine terminations for his specific epithets in both the text and the index, thereby implying that they were only subgenera or sections of *Agaricus*; and that he, therefore, did not validly publish these generic names. May I also direct attention to Bataille's "Les Bolets" (1908) and Gilbert's monograph "Amanitaceae" (1941)? These two publications may also prove fruitful subjects for elimination of *bona fide* published generic names!

I do not at all admire Staude's book, but I will not pretend to disbelieve in the valid publication of his names when I honestly believe the opposite to be true. Roger's solution asks mycologists to agree upon an unusual convention in connection with one particular case only. As this proposed convention is not supported by the Code, it will for that reason certainly not appeal to all!

Staude's names hardly form an inconvenience, and, apart from the dubious *Pleuropus-Pleurotus* case (cf. Donk, *1949*: 302, 348), there in only one generic name that endangers nomenclatural stability, viz., *Derminus* (Fr.) Staude, hence I have already proposed it for rejection in favour of *Pholiota* (Fr.) Kumm. (Donk, *1949:* 324, 394).[2] There are no inconveniences at all as to specific names, because the new combinations he made have no undersirable consequences. The fact that such household names in mycology as *Tricholoma, Clitocybe, Collybia,* and others appear to have been published much earlier than was commonly believed, is a gain.

Recently the Special Committee for Fungi approved by ballot the conservation of the fungus name *Tricholoma* with '(Fr.) Kummer' (*1871*: 25, 129) as the author's citation. If its members were genuinely convinced from a taxonomic point of view that this citation is preferable to '(Fr.) Staude' (*1857:* xxviii, 125), the name *Tricholoma* (Fr.) Staude must be added to nomina rejicienda as an earlier homonym. It may well be however, that they acted under the erroneous impression that Staude's generic names were not validly published. Since Staude's concept of the taxon is

[2] This proposal still stands.

the same as Fries's (1838) and Kummer's and includes nothing objectionable, to ignore *Tricholoma* (Fr.) Staude is to misrepresent the facts. Before the proposal is finally accepted, therefore, this error should be corrected and '(Fr.) Kummer' be replaced by '(Fr.) Staude.'

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