

COMMENTS ON HALLUCINOGENIC AGARICS AND THE HALLUCINATIONS OF THOSE WHO STUDY THEM

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The recent publication by Jonathan Ott, *Hallucinogenic plants of North America* (1976) and two articles by Guzmán and Ott and Guzmán, et al. in *Mycologia* (68: 1261–1267 and 1267–1271) deserve some comment in view of the current low degree of accuracy developing in the literature on hallucinogenic mushrooms generally. Since the situation involves some basic aspects of the systematics of the higher fungi, and the ability of some people to say what they mean, I feel the present contribution is needed.

One item that attracted my attention, as an example, is a footnote on page 31 of Ott's book cited above. Pertinent statements from this footnote are as follows: (1) “. . . in honor of R. G. Wasson, who *discovered* (italics mine) the Mexican mushroom cults. . . .” Every one knows that Wasson did not *discover* them, and I am not aware that Mr. Wasson ever made any such claim. He certainly did bring them to the attention of the English-speaking world in a thorough and scholarly manner. Ott simply used the wrong word. He even cited Schultes' work in his book.

(2) Singer . . . “published a slipshod paper in *Mycologia* (163–164)” I checked *Mycologia* 50: 163–164. Page 163 is the end of an article by Johnson on marine fungi. Page 164 is the first page of an article by Bocobo and Curtis on *Trichophyton*. The only two-page article, the one containing the description of *Psilocybe muliercula* as a new species, is in vol. 50 on pages 141–142. Obviously this is the one Ott intended to cite. Ott attributed this paper to Singer, but the authorship is Singer and Smith. It is difficult to understand how an article containing only Latin descriptions of new taxa can be “slipshod” as Ott has described it—certainly it is not as slipshod as Ott's footnote!

I also checked the papers he and his co-authors published in *Mycologia* 68: 1261–1267 and 1267–1271. On p. 1264 it is stated that the authors do not regard the character of the presence or absence of a gelatinized cuticle (an ixocutis) over the pileus as a “valid taxonomic distinction” in *Psilocybe*—but they do not support their statement with any data or explanation. I have studied *Psilocybe* longer and in more detail than any one of them and challenge their statement. In the

typical ixocutis of the pileus in *Psilocybe* the hyphae are narrow and either gelatinized or are in a matrix of slime extruded by the hyphae. Slime is not evident in a simple cutis, and in the latter the hyphae are usually of greater diameter than those of an ixocutis. This difference holds for the majority of the species just as it does in *Pholiota*, but there are some intergrading species as one would expect.

The authors made a second statement that a species growing on dung and one on wood were synonymous (*Psilocybe subaeruginascens* Höhnelt and *P. aerugineomaculans* (Höhnelt) Sing. & Smith). To justify this they had to disregard both the details of the pilear cuticle and the basic ecology of the species. Dung inhabitants do use lignicolous material in the dung rather readily, but it must be remembered that such material has been "pretreated" in a sense by passing through the animal. The occurrence of these species on untreated wood to my knowledge, has not been demonstrated.

These same authors seek to maintain *P. venenata* (Imai) Imazeki & Hongo and *P. subaeruginascens* as distinct on spore and cystidial sizes that are admittedly overlapping. One of the reasons I postponed study of the genus temporarily was that before drawing conclusions as to the value of spore size in the taxonomy of the genus, I wanted some reliable data on variation occurring in individual naturally occurring populations: the same mycelium over a period of time if possible. Studies by Hesler and Smith on *Lactarius* (in press) support strongly the conclusions of others (Haard and Kramer, 1970; Tanghe and Hillhouse, 1973; Gilliam, 1975) that variation in spore size in a single species covers a wider range than most investigators have realized. Smith and Hesler (1968) may have used too narrow a range in *Pholiota*, though this remains to be documented. On the basis of the information used by Guzmán and Ott, I would consider *Stropharia venenata* Imai as only a minor variant of *P. subaeruginascens*—at least until better evidence can be presented to the contrary. This evidence might be in the form of studies of the fungus in culture or the study of a large series of collections.

I have studied a large number of collections of *Psilocybe* and find habitat a rather constant feature, but here one must evaluate each species separately by large numbers of collections. Conclusions based on bits of evidence are necessarily tentative. In *Psilocybe*, collections cannot be made at will since the genus is often very erratic in the fruiting habits of the various species.

In the second article, the authors are quick to mention that they are writing a world monograph of the genus. As a young man I too had

similar hallucinations in regard to a number of genera. In *Psathyrella* (1972) I finally managed a partial treatment of our North American species, over 400 of them, but I estimate that this at best is not more than three-fourths of the total number that will be found. A world monograph of *Psilocybe*, one worthy of the name, would require a life-time's effort. It is one thing to visit a region and quite another to collect the *Psilocybe* flora. If 50 years of collecting agarics intensively has taught me anything, it is that no area in North America has been sufficiently "collected" for agarics to justify the statement that the flora of that area is completely known for any large genus of agarics. Talk of a world flora for any large genus of agarics is simply extravagant language at the present time. I would hope that Dr. Guzmán would limit himself to the Western Hemisphere.

The authors further state that *Psilocybe semilanceata* (Fr.) Kummer and *P. pelliculosa* (Smith) Sing. & Smith are generally indistinguishable by macroscopic features. Since I have seen both and described *P. pelliculosa* in the first place, and have made numerous collections of it since, I can say that in my opinion they are readily distinguishable at sight. Ott's (1976) illustration of *P. pelliculosa* on p. 24 is an outright atrocity. The photograph (Pl. 2) could be either *P. pelliculosa* or *P. silvatica* (Peck) Sing. & Smith.

In regard to *P. semilanceata* and the reason why Singer and Smith did not include it in the monograph of the blue-staining group, it is to be noted that Fries (Epicrisis, p. 231) did not give any indication that the basidiocarps stained green or blue when injured. Any one who has worked with the psilocyboïd species of *Naematoloma* (which are *Psilocybe* species in the broad sense of Quelét's genus *Geophila*) knows that olive or greenish tints may be present in the gills, pileus or stipe, but that these do not necessarily indicate staining from injuries. In other words, Fries' description of the cap of *Agaricus semilanceatus* as "luteus virescens, etc." is not to be construed as indicating a color change from injury. On this basis Singer and I did not include it in the blue-staining section *Caerulescentes*. The authors of the second article state (p. 1268): "It is interesting to note that *P. semilanceata* an hallucinogenic European species, was not reported from America by Singer and Smith (15) and not even considered to be an hallucinogenic fungus." It is to be noted that our article was published in 1958; the one by Benedict, Tyler, and Watling which brought up this point was published nearly ten years later (1967). In this connection I do not accept the statement that *P. semilanceata* var. *caerulescens* Cooke is "really synonymous with the type variety of *P. semilanceata*." It has not yet

been demonstrated that blue-staining on injury must be abandoned as a useful character in *Psilocybe*. Thus Watling's suggestion that the section *Caerulescentes* must be designated section *Psilocybe* because of *P. semilanceata* being the type of the genus is simply not true. It may be that the section *Caerulescentes* will eventually be abandoned and its species distributed on other features such as degree of veil development, presence or absence of an ixocutis on the pileus, etc., to arrive at a more natural classification. This is a possibility, but it cannot be dealt with here. The authors of the two papers are apparently unaware of important aspects of the problem.

The authors of the second article state (p. 1269): "*Psilocybe semilanceata* is here reported from Washington for the first time." I have a collection, Smith no. 18020 from just south of Cape Flattery, made Oct. 19, 1941. True, this collection was not cited nor reported in the literature, but it was here at the University of Michigan Herbarium when one of them studied my collections of the Strophariaceae (especially *Psilocybe*), a fact not mentioned by them.

The authors also make the statement that they are reporting *Panaeolus foenisecii* (Pers. ex Fr.) Kühn. (in whatever genus one desires to place it—*Panaeolina*, *Panaeolus*, *Psilocybe* or *Psathyrella*) for the first time from Oregon. Smith (1972, p. 33) cited three collections from Oregon, two made by Wm. B. Gruber of Portland. This was just in passing, I made no attempt to include all Oregon records. The species is cosmopolitan and is known from California and Washington! It shows very poor judgment on the part of the authors to make such a claim for such a widely distributed and common species even if a search of the literature had been made.

In summary it seems only fair to warn professional biologists that the claims made by Ott relative to the strictly mycological aspects of his work, be carefully checked for accuracy before reliance is placed upon them.

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**FIRST REPORT OF VESICULAR-ARBUSCULAR
MYCORRHIZAE ON *ELAEOAGNUS*
*ANGUSTIFOLIA***

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Elaeagnus angustifolia L. (Russian olive), a native of Europe and western Asia, has become naturalized in the United States and is distributed from the Pacific Coast eastward to New England, and from Canada to Mexico (1, 3). In the Great Plains, *E. angustifolia* is used in farmstead and field windbreaks, and as a landscape tree.

There are few published reports on mycorrhizal associations with *E. angustifolia*. No mycorrhizal associations have been reported with this species in the United States (3). Khan (4), investigating the incidence of mycorrhizae with 89 plant species from Pakistan, did not find any mycorrhizae on *E. angustifolia*. Lobanov (5) made a survey of mycorrhizae of woody plants in the European part of the U.S.S.R., and reported that *E. angustifolia* has ectomycorrhizae with crumbly braided hyphae over the root ends. The hyphae were not organized into an orderly net within the root, and the symbiont remains unknown. Another survey carried out at the Forest Institute, U.S.S.R. Academy of Sciences, by Borisova (2) revealed that *E. angustifolia* had hyphae on its root ends (see page 879, number 2 of picture 2), and transverse sections of