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#### *About the Institute*

The Hunt Institute for Botanical Documentation, a research division of Carnegie Mellon University, specializes in the history of botany and all aspects of plant science and serves the international scientific community through research and documentation. To this end, the Institute acquires and maintains authoritative collections of books, plant images, manuscripts, portraits and data files, and provides publications and other modes of information service. The Institute meets the reference needs of botanists, biologists, historians, conservationists, librarians, bibliographers and the public at large, especially those concerned with any aspect of the North American flora.

Hunt Institute was dedicated in 1961 as the Rachel McMasters Miller Hunt Botanical Library, an international center for bibliographical research and service in the interests of botany and horticulture, as well as a center for the study of all aspects of the history of the plant sciences. By 1971 the Library's activities had so diversified that the name was changed to Hunt Institute for Botanical Documentation. Growth in collections and research projects led to the establishment of four programmatic departments: Archives, Art, Bibliography and the Library.

September 17, 1969

Dear Mr. Wasson,

During my reading of your latest book "Soma ...", a small linguistic point concerning the interpretation of a Chinese vernacular name, came to my attention. My knowledge of Chinese is extremely limited (I entertained myself with some months of study during my Army service about 25 years ago) but I believe there is at least a moot question involved in a sentence that appears on page 82. The sentence reads: "The early commentators gloss this as meaning an edible mushroom growing on trees, probably what is known in recent centuries as the tree-ear, mu erh 木耳, familiar throughout the temperate zone to those versed in mushroom lore as the oyster mushroom, Pleurotus ostreatus (Fr.) Quel."

The mu erh (might not "wood ear" be a more appropriate translation than "tree-ear"?) has been known to me for many years as the common name (Peking dialect or gwo yü) of edible species of Auricularia, including A. fuscosuccinea and A. polytricha which are widely used in China and elsewhere in the Orient and exported today chiefly from Hong Kong to various parts of the western world. When I wrote my monograph of Auricularia in 1952 I was aware of this Chinese vernacular name but at the time thought it too abstruse a point to mention. In a work on Neotropical Tremellales now in press, I include it. To the best of my knowledge, mu erh is a designation not applied to a gilled fungus (Agaricaceae sensu stricto) and this would include Pleurotus ostreatus. I have recently consulted various Chinese students as well as a professor here, all of whom agree that mu erh refers to a non-gilled fungus and more specifically to Auricularia spp. I think this might also be confirmed in any Chinese restaurant where mu erh is commonly served.

Knowing of your interest in even the smallest matters that touch mushroom lore, your opinion on this question would be of the greatest interest to me even though the consequences of the interpretation may be of something less than world-shaking importance.

With kindest regards,

Sincerely yours,

Bernard Lowy

(Sapper & Brinton attacked)

LOUISIANA STATE UNIVERSITY  
AND AGRICULTURAL AND MECHANICAL COLLEGE  
BATON ROUGE · LOUISIANA · 70803

College of Arts and Sciences — Agricultural Experiment Station

DEPARTMENT OF BOTANY AND PLANT PATHOLOGY

DEPARTMENT OF PLANT PATHOLOGY

April 21, 1970

Dear Mr. Wasson:

It certainly may be argued whether Sapper understood the full implications of his own terminology (I am convinced that he did not) but concerning the stones, to question that "the idea that they truly represented fungi never crossed his mind," seems to me to give him much less credit than he deserves. I would be inclined to accept his terminology as prima facie evidence that Sapper interpreted the stones as mushroomic symbols and not as phalli or any other objects. While Barberena "hält das Ganze für einen Phallus" and Sapper replies that "diese Deutung erscheint mir aber unhaltbar ...", preferring to call these artifacts "Pilzförmige Götzenbilder", then it seems to me that Sapper has clearly committed himself. He did not guess that the stones might represent hallucinogenic mushrooms, but we are not on absolutely firm ground regarding this interpretation even today, in spite of the evidence now at hand which tends to support this hypothesis. That Sapper considered the stones to represent sacred objects, I think cannot be doubted. His description of them as "Götzenbilder" - false dieties or idols - is a value judgement, but it is also an admission of his belief that these idols were sacred to someone nevertheless. I would say that Sapper, in his brief note, came remarkably close to what we consider at present to be the best interpretation. I am not aware of anything else that Sapper had to say on the subject, but I would not agree with your statement (in Bot. Mus. Leaf. 20(2a): 42. 1963, entry 88) that Brinton makes "historically ... the first known, published reference to mushroom stones as mushroom representations."

I find no merit in Brinton's cursory note which is in fact, completely misleading. After accepting Sapper's interpretation that the stones are not "phallic emblems", Brinton goes on to say, quite incredibly I think, that Sapper "...advances, however, no other explanation". Sapper's terminology embodies his explanation and Brinton's "suggestion" is superfluous. Brinton's further comments that "the night growth of the fungus would strengthen the mythical alliance" (ie the Maya word "hu" being "sufficiently near" to "uh or yuh", meaning moon) would itself appear to be based on a myth.

Were you at Carnegie Hall on April 17 for the premier of Cela and Balada's "tragifonia", "María Sabina"? Sorry I could not be there, but I was thinking of you -- and of María.

Best wishes, as always,

B. Lowy  
B. Lowy

This interpretation was upheld by Dr. Rudolph Heberle, whom I consulted on May 5, 1970, showing him Sapper's notes which he read, as well as pertinent correspondence between Wasson and myself. B.L.

Unsere Abbildungen zeigen die Coremonie des Verbrennens, und zwar stellt Fig. 1 den Beginn des Vorganges dar, Fig. 2 den Moment des Vollbrandes und Fig. 3 das Ende der Handlung, wo die Überreste der Verbrennung in einer Urne gesammelt werden, um sie

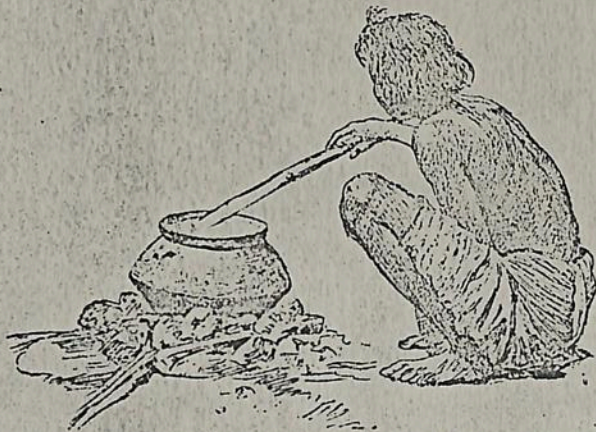


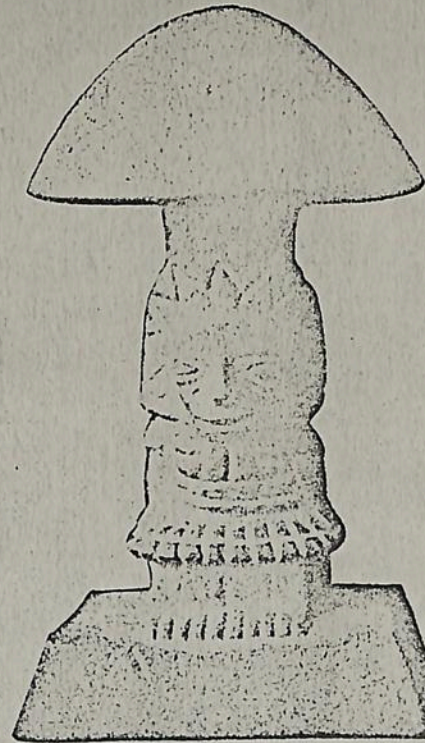
Fig. 3. Sammeln der Leichenasche und Knochenreste. Gezeichnet von N. Samokisch.

dann in das Wasser, womöglich in den Ganges, hinein zu werfen. Wird der Ort, an welchem der Scheiterhaufen gestanden hat, mit Wasser gewaschen, so wird ein kleiner Graben gemacht, damit das Wasser in den Flufs abläuft.

In dem russischen, von Gustav Radde herausgegebenen Prachtwerke „23 000 Meilen auf der Jagd Tamara“, das die Reise des Großfürsten Alexander und Sergej Michailowitsch nach Indien und Ostasien behandelt und dem auch unsere Abbildungen entnommen sind, finden wir noch die Bemerkung, daß die Leichenverbrenner in Indien eine besondere, niedrige Kaste bilden. Aber sie sind alle reich, weil sie den Gold- und Silberschmuck der Leichen — der nicht abgenommen zu werden pflegt — erhalten, wenn auch im geschmolzenen Zustande.

### Pilzförmige Götzenbilder aus Guatemala und San Salvador.

Auf der mittelamerikanischen Ausstellung in Guatemala waren aus verschiedenen Gegenden der Republik Götzenbilder ausgestellt, welche in ihrer äußeren Form lebhaft an Pilze erinnern. Die Figuren bestehen gewöhnlich aus drei besonderen Teilen, einem Sockel, einem ungefähr cylindrischen Mittelkörper, auf welchem ein Gesicht und Teile der Gestalt eines Menschen in sehr schematischer Weise eingegraben sind, und einem hutförmigen, meist rundlichen oberen Abschnitt. Auch aus San Salvador sind derartige Götzenbilder bekannt.



Pilzförmiges Götzenbild aus Guatemala.

Beistehend ist das besterhaltene dieser Idole (in  $\frac{1}{3}$  natürlicher Größe) nach einer Photographie von Dr. Santiago F. Barberena, dem Direktor des Museums in San Salvador, wiedergegeben. Der obere Abschnitt ist hier flacher als bei den guatemalteckischen Götzenbildern, die ich kenne; das Gesicht ist von einer Zickzacklinie umrahmt, welche neun volle Biegungen und noch eine einfache Linie zeigt. Dr. Barberena hält das Ganze für einen Phallus und glaubt, daß die neun Biegungen sich auf die neunmonatliche Schwangerschaft beziehen. Diese Deutung erscheint mir aber unhaltbar, da die mittelamerikanischen Indianer seiner Zeit zwanzigtägige Monate benutzten, und wenn sie auch sicherlich daneben die Zeit von Vollmond zu Vollmond (x ji lipó in Kekchi) beobachtet und berücksichtigt haben, so ist doch unwahrscheinlich, daß sie die Zeit der Schwangerschaft gerade nach der ungewöhnlichen Zeiteinteilung gezählt haben sollten. Auch ist die breite Ausladung des Hutes der Deutung als Phallus entgegen. Immerhin aber sind diese pilzförmigen Götzenbilder so eigenartig, daß sie die Aufmerksamkeit der Archäologen verdienen, welche vielleicht aus benachbarten Völkerkreisen die Bedeutung derselben zu erklären wissen.

Coban.

Carl Sapper.

May 22, 1898.

## Bücherschau.

Comte de Gobineau: Versuch über die Ungleichheit der Menschenrassen. Deutsche Ausgabe von Ludwig Schemann. 1. Bd. Stuttgart, Fr. Frommann (E. Hauff), 1898.

Die neue Ausgabe des berühmten Gobineauschen Rassenwerkes in deutschem Gewande beweist, wie sehr die allgemeinen Fragen der Anthropologie und Ethnologie in ihren Beziehungen zur Kulturgeschichte und Gesellschaftswissenschaft heutzutage die Geister beschäftigen. Wenn nach der Meinung des Übersetzers das in den fünfziger Jahren zuerst erschienene Buch in Deutschland im allgemeinen wenig be-

achtet wurde, so hat dazu nicht zum mindesten die scharfe, wenn auch durchaus urbane Kritik unseres A. F. Pott (Die Ungleichheit menschlicher Rassen hauptsächlich vom sprachwissenschaftlichen Standpunkte. Lemgo und Detmold 1856. 8<sup>o</sup>.) beigetragen, die Gobineaus Prätensionen „die wirkliche noch unerkannte Basis der Geschichte“ aufgedeckt zu haben, auf das rechte Maß zurückführte.

Wer, wie Referent, das Werk bis jetzt nur nach der Pottschen Besprechung kannte, wird sich nur schwer zu einer Durcharbeitung desselben entschließen, dann aber auch neben allen seinen Mängeln manche geistreichen und interessanten

thought. The scheme is comprehensive and well carried out in a brief compass.

Dr. Donath notes several of the prevalent errors in such investigations, as lack of discrimination between a given stage of culture and the psychic faculties of those who exhibit it, the mistake of assuming that mental power is correlated to cranial capacity, and especially the incorrectness of supposing a parallelism between the psychical evolution of a child and the race, as Bucke, Baldwin and others have too literally assumed. ('Die Anfänge des Menschlichen Geistes,' pp. 47; F. Encke, Stuttgart, 1898.)

#### MUSHROOM-SHAPED IMAGES.

THEOBERT MALER and others have published illustrations of stone pillars with mushroom-shaped summits, occurring in Yucatan, Guatemala and elsewhere in Mayan territory.

In the *Globus* for May 28th Dr. Carl Sapper gives a picture of one in excellent preservation, about 30 centimeters in height, from San Salvador. On the shaft the face of a man (or monkey) is roughly outlined. Over it is the umbrella-like expansion.

These have generally been considered phallic emblems. Dr. Sapper doubts this, and in fact there is no evidence for it beyond a vague resemblance. He advances, however, no other explanation.

I would offer a suggestion. They resemble in shape mushrooms or toadstools, and why should not that be their intention? Why should it be? Because the word for mushroom in Maya (Tzental dialect) is hu, sufficiently near to the word for moon, uh or yuh, to recall it in sound, and the night growth of the fungus would strengthen the mythical alliance. They would thus be emblematic of the lunar and nocturnal divinity.

#### AMERICAN INDIAN GAMES.

This subject is treated in an interesting manner from ample material by Mr. Stewart

Culin in the *Bulletin* of the Museum of the University of Pennsylvania, No. 3, Vol. I. He selects for analysis the game of dice or tossed staves, which he finds among sixty-one North American tribes. With much ingenuity he compares their implements and the decorations upon them, reaching the conclusion that they were all derived from some center in northern Mexico or near there; the thread of connection which leads him being the throwing-stick, or *atlatl*, of the Mexicans.

This is ingenious, but not wholly convincing. One may ask why the *atlatl* might not have drawn its local symbols and trappings from the game, rather than *vice versa*. The symbolism is surely more recent than the game; *atlatls* are found elsewhere without it; and there are simpler explanations of the elementary symbolism of the game in the northern tribes. In the study of development it is usually wiser to begin with the simple and proceed to the complex, rather than the reverse.

D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

#### SCIENTIFIC NOTES AND NEWS.

##### PREVENTIVE INOCULATION AGAINST PLAGUE.

M. HAFFKINE made recently an address on the above subject at Poona, in the course of which, according to the report in the *London Times*, he compared the invasion of India by the plague bacillus to the invasion of Australia by rabbits, to the invasion of certain soils in south Europe by the phylloxera, and to the invasion of South Africa by the organism of the rinderpest, and used these analogies to show that there are, in both the animal and vegetable worlds, diseases of which the cause, the morbid organism, can live and propagate outside the patient's body, can grow in the soil, in water, be carried by clothing, bedding, instruments, by any living or dead object. If it happens that the natural conditions of a country are favorable to the life and propagation of such an infectious organism, and as long

[See: Wason in Bull. Mus. Geol. 1963. P. 42]

R. GORDON WASSON  
42, Long Ridge Road · DANBURY, Conn., 06810  
TEL.: (203) 748:0123

27 May 1970

NEW YORK CITY:  
45, East End Avenue, 10028  
TEL: (212) BU-8:2323

Professor B. Lowy  
Department of Botany and Plant Pathology  
Louisiana State University  
Baton Rouge, Louisiana 70803

Dear Professor Lowy:

I am surprised and disappointed. To say that a view held by Steve and me on a subject to which we had given long and careful consideration is 'untenable' is arrogant and rude.

In your last missive you spoke of a 'distinguished German Professor' who supported your view. I cast about in my mind who he could be. I thought possibly it was the late Franz Termer, Director of the Museum in Hamburg, a solid scholar and a personage, a specialist in Middle American cultures, who had published a paper that we had missed. But it turns out to be someone who by your own admission knew nothing about the subject, knows nothing except what you have told him in introducing our correspondence. You quote him to me not realizing that his opinion is worthless, being merely an amiable agreement with his interlocutor. What naiveté! You look into a mirror, see yourself there reflected, and say there are two persons.

So far as I am concerned the correspondence is closed.

Sincerely yours,

*R. G. Wasson*

May 30, 1970

Mr. R. Gordon Wasson  
42, Long Ridge Road  
Danbury, Conn., 06810

Dear Mr. Wasson:

It appears that you consider my dissent so outrageous that words can scarcely offend sufficiently to express your indignation and contempt. Mine was not an attempt to discredit an entire corpus of information that had laboriously been accumulated over the decades. You may consider me arrogant and rude but I am not mad. Had you read my review of your book "Some" in Mycologia, it must have been clear that I thought the work of you and your collaborators admirable. My crime was to disagree with a small fragment of a much larger question. What I specifically demurred from was, as you put it, "the idea that they (mushroom stones) truly represented fungi never crossed his (Sapper's) mind." It is my contention that in all probability this idea had crossed his mind. It is not necessary that Zeus sit in judgement of trivia.

I will not countenance your malignant attack on a respected colleague who because he is unknown to you, you denigrate! Your gratuitous condemnation of a world renowned scholar's opinion as "worthless" would be incredulous, were it not so transparent an attempt to discredit out of hand a source that dares to disagree with you. My disagreement was and is genuine and to suggest that I had "an amiable agreement" with my interlocutor is reprehensible and repulsive in the extreme. For the record, should there be any doubt on this question, let it be understood that it was not merely "our correspondence" (which was secondary) that was shown to my consultant, but of course the appropriate papers of Sapper and Brinton, which he read.

I once held in esteem your zealous pursuit of knowledge, but you have cast a shadow on the methods you choose to employ, which hardly seem to reflect the sober deliberations of a scholar. You demean yourself, not others, by calling into question the legitimate differences that not uncommonly divide men of informed opinion. It is the discipline of constructive dissent that has always advanced knowledge and it is this privilege and duty of the scholar that you would now presumptuously deny to others.

Sincerely yours,

B. Lowy

R. GORDON WASSON  
42, Long Ridge Road · DANBURY, Conn., 06810  
TEL.: (203) 748:0123  
19 November 1971

Professor B. Lowy  
Department of Botany and Plant  
Pathology  
Louisiana State University  
Baton Rouge, Louisiana 70803

Dear Professor Lowy:

I am excited by your discoveries! I was not familiar with the Galindo codex. Where has it been written up or reproduced?

I hear you have published a paper in the Revisa Interamericana Vol. 1, No. 1, which appeared last spring. If you could send me one (or two, if there are any to spare) copy of this, I shall be much obliged.

Sincerely yours,

R Gordon Wasson

LOUISIANA STATE UNIVERSITY  
AND AGRICULTURAL AND MECHANICAL COLLEGE

BATON ROUGE • LOUISIANA • 70803

College of Arts and Sciences

DEPARTMENT OF BOTANY

November 17, 1971

Dear Mr. Wasson:

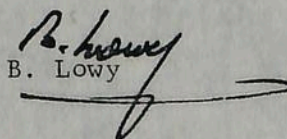
I shall not conceal from you that I was astonished and shaken by your letter of May 27, 1970 and considering your great provocation, I felt and still feel that my response was a moderate one. I thought it highly improbable then that the breach you so deliberately opened between us would ever be closed. But it is unseemly that men who pursue in research essentially the same goal, the advancement of knowledge, should forever remain incommunicado. It is in this spirit that I accept your apology, so let us continue where we left off.

My reference to mushrooms in the Maya codices is based on my interpretation of sections of the Galindo, Madrid and Dresden codices. I am preparing to publish the necessary documentation for the support of this statement. Meanwhile I may refer you to page 95, figure b of the Madrid codex (=Codex Tro-Cortesianus) which depicts a personage in priestly garb, holding in his numinous hand the object which I interpret as a sacred mushroom. It is also found in the Dresden (pages 25-28) held there by a priest, but in different circumstances. I consider ~~figure~~ figure b, page 95 of the Madrid to be deeply significant and believe it provides the key which will support the assertion that a mushroom cult existed among the Maya. In this painting, directly above the mushroom cap is the solitary glyph "cimi", which signifies death! The juxtaposition of this symbol with the mushroom is surely not accidental. I am aware of the objections that might be raised, inasmuch as the mushroom in question has traditionally been interpreted (but more commonly ignored in recent times) as a rattle or a fan (Förstemann 1906 and Villacorta 1933, who has simply accepted Förstemann's conclusion, although the latter was not too sure of this). To my knowledge this has never been challenged. In fact it has never been restudied in the light of recent evidence about the mushroom cult for the elucidation of which you have been largely responsible. The painting in the codex to which I call your attention (and there are others in the Madrid as well as in the Dresden) is in my estimation pregnant with meaning, only part of which we can adumbrate. But are we not witnessing in this painting the enactment of an ancient hieratic rite among the Maya, in which sacred hallucinogenic mushrooms played a vital role?

My statement about your pottery mushroom "said to be from Tenenexpan, Veracruz, Mexico" is based on Borhegyi's statement to this effect in his 1963 paper in "American Antiquity" entitled "Pre-Columbian pottery mushrooms from mesoamerica," p. 332, legend to fig. 5, which I have accepted at face value.

P.S. Because of the unexpectedly large number of requests for reprints of my paper (which I have not yet received from the printer) I must limit the number sent to <sup>each of</sup> my correspondents to one. There will, of course, be no question of any payment.

Sincerely yours,

  
B. Lowy

R. GORDON WASSON

42, Long Ridge Road · DANBURY, Conn., 06810

TEL.: (203) 748:0123

Saturday, 15 Aug. 1981

Dear Bernard:

There is a story I wish to tell concerning our anated friend, but I do not wish to write it.

I have written my letter to you in such hope that you can forward it to Gaston if you wish. He lacks the elements of a mycologist. How does he think he can write about Cambodia and Thailand if he has not studied Patonalland and Heem?

↑  
and India

RG  
WJ

R. GORDON WASSON

42, Long Ridge Road · DANBURY, Conn., 06810

TEL.: (203) 748:0123

15 August 1981

Professor Bernard Lowy  
Department of Botany and Plant Pathology  
Louisiana State University  
Baton Rouge, Louisiana 70803

Dear Bernard:

Now that you are retired, should I be sending letters to you to a different address?

Did I ever send to you the paper that Roger Heim and I published in the Cahiers du Pacifique, #14, September 1970? It is entitled 'Les Putka des Santals champignons doués d'une âme'. Do let me know, because it was an important one and it never appeared in English. I was then preoccupied with SCMA and never got around to translating it. In it twice I mentioned an inebriating mushroom widely known in India, apparently the same species now reported in Nepal, familiar as to its properties to the natives, Stropharia cubensis, which Guzman and Singer insist on calling Psilocybe cubensis (Earle) Singer. Somewhere I have surmised in print that it originated in Asia. It is named by the native Mazatecs Hongo de San Isidro and may have come over in the galleons from the Philippines or Asia. It has no real name in Mazatec, or, if they try a Mazatec name, they call it nti<sup>1</sup> xi<sup>3</sup> tho<sup>3</sup> ('sacred mushroom') that grows on cow dung!. They rate it lower than the psilocybes and most of them avoid using it. Heim accepted the native belief that it was inferior to the other ones, but it has become the most commonly used sacred mushroom by Mexicans (not indígenas) as well as Americans and is consumed in enormous quantities because it lends itself to cultivation. ...It is also rated lower by the Mixtecs whom I have asked about it. I think this lower rating is due solely to its being an alien.

This Cahier du Pacifique article you should have and I will send it to you if you do not already possess it. In it S. cubensis figures near the top of p. 65 and at some length on p. 66. Heim records that Patouillard reported it in Tonkin and he himself found it personally in Cambodia and Thailand. Guzman should familiarize himself with the work of the two first class French mycologists who studied the fungi of Africa and Asia.

Have you ever seen a justification for changing the name of this mushroom from Stropharia to Psilocybe? I am constantly on the lookout for it but have not come across it. Heim always contended that the change was without justification. At last Singer published a few years ago, in a volume brought out by Saltzman (?) of Denver, Heim's reasons for keeping it in the genus Stropharia but if I recall Singer gave no justification for his reclassifying it. Heim set forth his reasons in the Stropharia cubensis section of Les Champignons hallucinogènes du Mexique. Singer renamed it obviously for vanity, and Guzman followed suit.

As ever,

Rgw

24-VIII-1981

Dear Gordon,

I have not seen the 1970 paper you refer to, and the "Cahiers du Pacifique" is not in our library, so I would be glad to have a copy.

I quite agree with you about Guzmán.

Concerning the justification for including the species cubensis either in Stropharia or Psilocybe, taxonomists have had genuinely differing opinions. To the best of my knowledge, Singer gave his reasons (p. 505 enclosed) for transferring Stropharia cubensis Earle to Psilocybe as the new combination Psilocybe cubensis (Earle) Sing. (p. 507) in 1949 in the 1st edition of "The Agaricales ..." and has retained it in that status ever since.

At the moment I am preparing a paper on some of my Brazilian fungi but expect to be interrupted shortly by moving to a new building, herbarium and all, with more space than I now have.

as always,

*Bernard*

P.S. - My address remains the same since I continue working in the Botany Department.

(*Hypholoma*, Quél.); *N. radicosum* (Lange) Konr. & Maubl. (*Hypholoma*, Lange). *N. fasciculare* (Huds. ex Fr.) Karst. (*Hypholoma*, Quél.).

Sect. 3. **PSILOCYBOIDES** Sing. (1948) (= *Hypholoma*, sect. *Tenuioria* Konr. & Maubl. 1948) Spores often larger than 10  $\mu$ ; annulus none, or rarely inconstantly and weakly developed; stipe usually rather thin, flexuous and elongate; carpophores mostly solitary or gregarious, not (or exceptionally) fasciculate; on small logs and sticks, more often in deep moss, especially *Sphagnum* and *Polytrichum*, in marshy prairies and in humid forests, also in the subalpine shrub vegetation, in tundras and peat swamps.

*Type species*: *N. dispersum* (Fr.) Karst.

*N. dispersum* (Fr.) Karst. (*Hypholoma*, Quél.); *N. Polytrichi* (Fr. sensu Ricken) Sing. (*Hypholoma*, Ricken); *N. elongatipes* (Peck) Sing. [*Psilocybe*, Sacc.; *Hypholoma*, A. H. Smith; *Agaricus* (*Psilocybe*) *udus* var. *elongatus* Fr. (?)]; *N. subumbonatescens* (Murr.) Sing. (*Stropharia*, Murr.); *N. ericaeum* (Pers. ex Fr. sensu Kühner) Sing. (*Psilocybe*, Quél.; *Hypholoma*, Kühner); *N. subericaeum* (Fr.) Sing. (*Psilocybe*, Sacc.; *Hypholoma*, Kühner).

#### KEY TO THE SPECIES

The third section can be determined (as far as European species are concerned) by Kühner's key (*Bull. Soc. Myc. Fr.* 52: 17-30. 1936). The species of section 2 are comparatively easy to determine by most current keys, and the three species indicated here as belonging to section 1 can be looked up in the literature and distinguished by macroscopical characters.

#### 113. **PSILOCYBE** (Fr.) Quél.

*Champ. Jura Vosg.* p. 147. 1872-3, em.

*Type species*: *Agaricus semilanceatus* Fr.

*Syn.*: *Pholiotella* Speng., *Bol. Acad. Cordoba* 11: 412. 1889.

*Characters*: Pileus cylindric-conic or semiglobate to convex, campanulate, often umbonate or papillate, subviscid, or viscid, or hygrophanous, or dry; epicutis consisting of thin, hyaline, filamentous, repent, somewhat to rather strongly gelatinized, thin-walled, clamped hyphae; hypodermium consisting of hyphae somewhat broader than those of the epicutis but not subisodiametric in any case, pig-

mented, rather strictly radially arranged; subenticular layer consisting of intermixed-irregular elements as most of the context of the pileus; veil slight, fibrillose on the marginal zone of the pileus, often fugacious, or appendiculate, or else absent; lamellae broad, adnexed to adnate, sometimes with a slight decurrent tooth; cheilocystidia present; pleurocystidia present or absent but never any chrysocystidia present on either edges or sides of the lamellae; hymenophoral trama subregular; spore print deep lilac to fuscous sepia; spores with double wall and germ pore, smooth, small to large; stipe dry, glabrous or with a fibrillose coating, often with a distinct well developed annulus, usually strongly elongate and narrowly hollow but in some species frequently thick and fibrillose fleshy, sometimes with a pseudorrhiza; context sometimes bluing and then strongly reacting with monomethylparamidophenol, in other species flesh unchanging and reaction indistinct; odor raphanaceous or farinaceous (not with the astringent, « bitter » odor of *Naematoloma*); all hyphae with clamp connections; on sticks, stems, mud, peat, earth, forest humus, deep moss beds, on scattered dung, sawdust, etc.

*Development of the carpophores*: At least in some species hemiangiocarpous.

*Area*: Almost cosmopolitan.

*Limits*: This genus has been emended by Singer (1936, and again in the present work) in order to be adapted to the diagnosis of the group into which *P. semilanceata* (Fr.) Quél., the type species of the genus, belongs. Many species of the old conception originated by Fries, are now part of the large genus *Psathyrella*, others have been transferred to *Naematoloma*, and one forms the genus *Panaeolina* (this corresponds to the Fayodian emendation of *Psilocybe*). The species left are now combined with certain atypical species of the genus *Stropharia* sensu lato, characterized by the lack of chrysocystidia. Though Fries originated the artificial group *Psilocybe* which was gradually transformed into a natural unit by drastic transfers of species and whole groups of species, he undoubtedly had a vision of the true affinities when he states, under *Agaricus semilanceatus*, that this species continues the *Merdarii* of *Stropharia* (*S. semiglobata* and its allies, and *S. merdaria*), a group that is now divided between *Stropharia* and *Psilocybe*. Fries often surprises the modern taxonomist by short observations on the true affinity of certain groups while at the same time he refuses to express these affinities in his classification.

Some species of *Naematoloma* which are deprived of a true subcellular hypodermium, nevertheless are provided with chrysocystidia, and are therefore easily separable from the genus *Psilocybe*.

Some authors, even in recent works, have considered *Deconica* as the same genus as *Psilocybe*, or, if two groups are separated, they have not been considered as worthy of generic status. It appears to the author that these genera are separable on the basis of a character which is very easy to observe, viz. the shape and size of the spores. This one character is always correlated with at least one alternative character, i. e. in *Psilocybe*, the spores are either larger than 11  $\mu$  or non-lentiform, and at the same time, the pileus is non-hygrophanous, or the veil is strongly annuliform, or else pleurocystidia are present. The true *Deconicas* are very uniform macro- and microscopically; they can easily be recognized in the field as belonging to a particular group, and they are related but undoubtedly separable from *Psilocybe* as well as *Melanotus*.

*State of knowledge*: This genus is in need of a monographic treatment. Only eight species are admitted below which are completely known; some others, especially American species, are in the process of being worked up by A. H. Smith who has begun to publish on them. There are probably many more species in North America, and also in Asia.

*Practical importance*: At least one species is used as a drug in Mexico (causing a temporary narcotic state of hilarity) but is poisonous when used in excess.

#### SPECIES

Sect. 1. **MERDARIAE** (Fr. ut subsect. *Agarici* subg. *Strophariae* 1874) Sing. Pileus neither acute nor conic-campanulate; veil present, sometimes annuliform; cystidia on the sides of the lamellae present or absent; spores up to 13  $\mu$  long; context not bluing on exposure; monomethylparamidophenol reaction weak or none. On dung or on manured fields.

*Type species*: *P. merdaria* (Fr.) Ricken.

*P. merdaria* (Fr.) Ricken (*Stropharia*, Quél.); *P. coprophila* (Bull. ex Fr.) Quél. (*Deconica*, Karst.); *P. blattariopsis* (Speg.) Sing. (*Pholiotella*, Speg.).

Sect. 2. **CAERULESCENTES** Sing. (1948). Pileus neither acute nor conic-campanulate; veil present, fugacious, or annuliform; cys-

tidia on the sides of the lamellae usually absent; spores medium to large; context bluing on exposure (reaching « deep Medici blue » Ridgway), and strongly reacting with monomethylparamidophenol.

*Type species*: *P. cubensis* (Earle) Sing.

*P. cubensis* (Earle) Sing. (*Stropharia*, Earle; *Nematoloma caeruleescens* Pat.; *Hypholoma*, Sacc.; *Stropharia*, Sing.; *Stropharia caeruleescens* Imai; *Stropharia venosa* Imai; *Stropharia cyanescens* Murr.); obviously also *P. cyanescens* Wakefield apud Wakefield & Dennis and *P. caeruleescens* Murr.

Sect. 3. **ATROBRUNNEAE** Sing. (1948). Pileus campanulate or conic at first, hygrophanous; stipe with fibrillose velutinous coating, or sometimes glabrous, exannulate; veil little developed and always absent in mature specimens; pleurocystidia few or numerous; on mud in swamps, among *Sphagnum*, on small sticks, on peat, never on dung.

*P. atrobrunnea* (Lasch) Gillet.

Sect. 4. **TENACES** (Fr.) Sacc., em. Pileus cylindrical-fusoid-campanulate acute, or convex becoming applanate, slightly viscid and subhygrophanous or partly hygrophanous, not very strongly hygrophanous, often with fulvous-ocher or olive tinge and somewhat reminiscent in the general habit of the *Naematolomas* of the section *Psilocyboides*; veil very little developed, in some individuals practically absent, in others fibrillose-subcortinoid and leaving indistinct traces in young specimens but never annuliform; pleurocystidia none; habitat on mud in swamps, among *Sphagnum*, on peat, on sticks and decayed trunks, also on the earth in low clearings, margins of the woods and in open coniferous woods, not on dung.

*Type species*: *P. semilanceata* (Fr.) Quél.

*P. semilanceata* (Fr.) Quél.

It is possible that *P. uda* (Pers. ex Fr. sensu Quél., Ricken) Gillet enters this genus but the author has not recently studied the cystidia.

#### KEY TO THE SPECIES

The small number of species admitted and the large number of species described, do not make it appear advisable to publish a key to the species of *Psilocybe* at the moment.

R. GORDON WASSON  
42, Long Ridge Road · DANBURY, Conn., 06810  
TEL.: (203) 748:0123

27 May 1970

NEW YORK CITY:  
45, East End Avenue, 10028  
TEL: (212) BU-8:2323

Professor B. Lowy  
Department of Botany and Plant Pathology  
Louisiana State University  
Baton Rouge, Louisiana 70803

Dear Professor Lowy:

I am surprised and disappointed. To say that a view held by Steve and me on a subject to which we had given long and careful consideration is 'untenable' is arrogant and rude.

In your last missive you spoke of a 'distinguished German Professor' who supported your view. I cast about in my mind who he could be. I thought possibly it was the late Franz Termer, Director of the Museum in Hamburg, a solid scholar and a personage, a specialist in Middle American cultures, who had published a paper that we had missed. But it turns out to be someone who by your own admission knew nothing about the subject, knows nothing except what you have told him in introducing our correspondence. You quote him to me not realizing that his opinion is worthless, being merely an amiable agreement with his interlocutor. What naiveté! You look into a mirror, see yourself there reflected, and say there are two persons.

So far as I am concerned the correspondence is closed.

Sincerely yours,

*R. G. Wasson*

May 30, 1970

Mr. R. Gordon Wasson  
42, Long Ridge Road  
Danbury, Conn., 06810

Dear Mr. Wasson:

It appears that you consider my dissent so outrageous that words can scarcely offend sufficiently to express your indignation and contempt. Mine was not an attempt to discredit an entire corpus of information that had laboriously been accumulated over the decades. You may consider me arrogant and rude but I am not mad. Had you read my review of your book "Some" in Mycologia, it must have been clear that I thought the work of you and your collaborators admirable. My crime was to disagree with a small fragment of a much larger question. What I specifically demurred from was, as you put it, "the idea that they (mushroom stones) truly represented fungi never crossed his (Sapper's) mind." It is my contention that in all probability this idea had crossed his mind. It is not necessary that Zeus sit in judgement of trivia.

I will not countenance your malignant attack on a respected colleague who because he is unknown to you, you denigrate ! Your gratuitous condemnation of a world renowned scholar's opinion as "worthless" would be incredulous, were it not so transparent an attempt to discredit out of hand a source that dares to disagree with you. My disagreement was and is genuine and to suggest that I had "an amiable agreement" with my interlocutor is reprehensible and repulsive in the extreme. For the record, should there be any doubt on this question, let it be understood that it was not merely "our correspondence" (which was secondary) that was shown to my consultant, but of course the appropriate papers of Sapper and Brinton, which he read.

I once held in esteem your zealous pursuit of knowledge, but you have cast a shadow on the methods you choose to employ, which hardly seem to reflect the sober deliberations of a scholar. You demean yourself, not others, by calling into question the legitimate differences that not uncommonly divide men of informed opinion. It is the discipline of constructive dissent that has always advanced knowledge and it is this privilege and duty of the scholar that you would now presumptuously deny to others.

Sincerely yours,

B. Lowy

R. GORDON WASSON  
42, Long Ridge Road · DANBURY, Conn., 06810  
TEL.: (203) 748:0123

12 November 1971

Professor B. Lowy  
Department of Botany and Plant Pathology  
Louisiana State University  
Baton Rouge, Louisiana 70803

Dear Professor Lowy:

Your paper on the mushroom stones in the current Mycologia is a valuable contribution to our knowledge. So far as I recall it also marks the first time that a serious ethno-mycological paper has appeared in that journal. This is a progressive step for Mycologia but perhaps it diminishes the number of interested people who will see it.

Could you spare three or four reprints? I shall gladly pay for them.

Near the top of p. 984 you say that mushrooms are occasionally depicted 'in the few remaining known Maya codices'. I had not known this and would appreciate the references. At the bottom of p. 988, referring to the pottery mushroom that I own, you say that it is said to be from Tenenexpan, Vera Cruz. I had never associated this locality with it. I had known it only as of the Remojadas culture type. I had thought it came into the market of unknown provenience. Gordon Ekholm confirmed its authenticity and everyone has linked it with the Remojadas culture.

I wish to apologize for that letter of mine dated May 27, 1970. It was quite inexcusable and I have regretted it from the day I mailed it. I hope you will accept my apology.

Sincerely yours,

*R. Gordon Wasson*

LOUISIANA STATE UNIVERSITY  
AND AGRICULTURAL AND MECHANICAL COLLEGE

BATON ROUGE . LOUISIANA . 70803

College of Arts and Sciences

DEPARTMENT OF BOTANY

November 17, 1971

Dear Mr. Wasson:

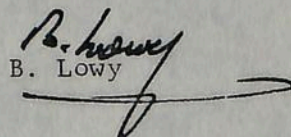
I shall not conceal from you that I was astonished and shaken by your letter of May 27, 1970 and considering your great provocation, I felt and still feel that my response was a moderate one. I thought it highly improbable then that the breach you so deliberately opened between us would ever be closed. But it is unseemly that men who pursue in research essentially the same goal, the advancement of knowledge, should forever remain incommunicado. It is in this spirit that I accept your apology, so let us continue where we left off.

My reference to mushrooms in the Maya codices is based on my interpretation of sections of the Galindo, Madrid and Dresden codices. I am preparing to publish the necessary documentation for the support of this statement. Meanwhile I may refer you to page 95, figure b of the Madrid codex (=Codex Tro-Cortesianus) which depicts a personage in priestly garb, holding in his numinous hand the object which I interpret as a sacred mushroom. It is also found in the Dresden (pages 25-28) held there by a priest, but in different circumstances. I consider ~~figure~~ figure b, page 95 of the Madrid to be deeply significant and believe it provides the key which will support the assertion that a mushroom cult existed among the Maya. In this painting, directly above the mushroom cap is the solitary glyph "cimi", which signifies death! The juxtaposition of this symbol with the mushroom is surely not accidental. I am aware of the objections that might be raised, inasmuch as the mushroom in question has traditionally been interpreted (but more commonly ignored in recent times) as a rattle or a fan (Förstemann 1906 and Villacorta 1933, who has simply accepted Förstemann's conclusion, although the latter was not too sure of this). To my knowledge this has never been challenged. In fact it has never been restudied in the light of recent evidence about the mushroom cult for the elucidation of which you have been largely responsible. The painting in the codex to which I call your attention (and there are others in the Madrid as well as in the Dresden) is in my estimation pregnant with meaning, only part of which we can adumbrate. But are we not witnessing in this painting the enactment of an ancient hieratic rite among the Maya, in which sacred hallucinogenic mushrooms played a vital role?

My statement about your pottery mushroom "said to be from Tenenexpan, Veracruz, Mexico" is based on Borhegyi's statement to this effect in his 1963 paper in "American Antiquity" entitled "Pre-Columbian pottery mushrooms from mesoamerica," p. 332, legend to fig. 5, which I have accepted at face value.

P.S. Because of the unexpectedly large number of requests for reprints of my paper (which I have not yet received from the printer) I must limit the number sent to <sup>each of</sup> my correspondents to one. There will, of course, be no question of any payment.

Sincerely yours,

  
B. Lowy

R. GORDON WASSON  
42, Long Ridge Road · DANBURY, Conn., 06810  
TEL.: (203) 748:0123  
19 November 1971

Professor B. Lowy  
Department of Botany and Plant  
Pathology  
Louisiana State University  
Baton Rouge, Louisiana 70803

Dear Professor Lowy:

I am excited by your discoveries! I was not familiar with the Galindo codex. Where has it been written up or reproduced?

I hear you have published a paper in the Revis<sup>+</sup>a Interamericana Vol. 1, No. 1, which appeared last spring. If you could send me one (or two, if there are any to spare) copy of this, I shall be much obliged.

Sincerely yours,

R Gordon Wasson