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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.

1 August 1981

ANNUAL REPORT

MYCOLOGIA, Volume 74

This report covers the period 1 August 1981 through 30 July 1982.

(1) Manuscripts

Number carry-over from 1980-81	17
Number received	218
Total	235

Disposition of manuscripts

Accepted and edited, or published	107
Rejected	53*
Withdrawn	2
On hand, pending final action (in review process or under revision by author)	73
TOTAL	235

*Five resubmitted after revision; three accepted.

(2) Volume 74 is complete. Numbers 1-4 have appeared, No. 5 proofread, and No. 6 (November/December) manuscripts have been sent to the Press. The January/February issue, Volume 75 is about one-fourth completed (and copy edited).

(3) Appointments to the Editorial Board. One appointment for a five-year term is needed. Dr. Bernard Lowy agreed to serve a second five-year term if nominated by the Editorial Board. The Board nominates Dr. Lowy, and requests approval by the Council. elected unanimously.

(4) Miscellaneous - At the 1981 meeting, the Editorial Board established a policy and procedure for handling contested book reviews. One case is pending; both parties have agreed to submit statements for simultaneous publication in the NEWSLETTER.

The Editorial Board did not approve a request to publish (periodically) records of Deuteromycetes in North America, suggesting that such records be distributed by means of a "newsletter" established and operated by interested parties.

Reviewers continue to be needed, particularly in the areas of mycorrhizal associations, ultrastructure, and fungal ecology.

Papers submitted by nonmembers of the Society have required considerably more handling and processing than those contributed by members. The Editorial Board may waive the membership requirement for exceptional papers, and the Editor is expected to get the approval of the majority of the Board for waiver. In theory, this has meant getting permission of the author to have additional copies made of the manuscript and art work, and submitting the paper to all members of the Board. As this can be cumbersome, I have had a preliminary

review of the paper by two specialists, and on the basis of their review decided on whether or not to poll the Editorial Board. This preliminary processing has worked well, and does not unnecessarily burden the Board. In all but one instance in '81-'82, the reviewers' comments clearly made waiver request unnecessary.

I take this opportunity of expressing my thanks to the Editorial Board for their service, help, and advice. Their prompt cooperation has accelerated the processing of manuscripts and has provided much-needed guidance to me. The Editorial Board has an indispensible role in publishing the journal, and the current Board members meet their responsibilities admirably.

For the Editorial Board,

T.W. Johnson, Jr.

TWJ:jd

MYCOLOGIA
OFFICIAL JOURNAL OF THE
MYCOLOGICAL SOCIETY OF AMERICA
PUBLISHED BY
THE NEW YORK BOTANICAL GARDEN

T.W. JOHNSON, JR.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

27 July 1982

Memo: Annual Report

Re: Editorial Board

I enclose a copy of our annual report as it will be presented to the MSA Council on Sunday, August 8, 1982.

While there is at present no business matters to be taken up with the Board, it might be well if we met informally during the meetings at Penn State. Perhaps Board members have comments, suggestions, ideas they would like to discuss, and an opportunity for that should be provided. We might, for instance, review the policy of processing papers from nonmembers, for I may have overstepped my bounds in the way I handle such manuscripts.

There will be a brief, informal meeting of the Editorial Board at 4:15 p.m., Wednesday, August 11, in the MSA Headquarters, Room 319 HUB. Any Board member attending the AIBS meetings is asked to attend this brief meeting as well.

If you have any comments, criticisms, or suggestions about the journal, (format, quality, handling of manuscripts) I will welcome them as ideas for improving MYCOLOGIA.

Thank you very much for your cooperation, help, and advice this past year. Your efforts have made my task infinitely easier.

Terry

T.W. Johnson, Jr.

TWJ:jd

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803-1705
504/388-8485

11-VI-1982

Dr. T.W.Johnson, Jr.
Editor-in-Chief, MYCOLOGIA
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Terry:

Earlier this year I had tentatively planned to return to Guatemala for mycological and ethnomycological work, but because of the unsettled political situation there, it would hardly be prudent to engage in field work just now. Instead, I have decided to go for the first time to Hungary on something of a sentimental journey, with mycological overtones. I have had an abiding interest in Hungary since childhood, both my parents having been born there. They emigrated to the U.S. at the turn of the century. Also, for the past two decades or so I have maintained friendly relations with two mycologists in Budapest (G.Bohus and M. Babos) and expect to visit them there.

The chief purpose of this note is to let you know of my impending absence from the university from July 1 to August 15, after which date I'll be ready again for such editorial work on MYCOLOGIA as you may wish to send me.

With best regards,

Bernard

B. Lowy

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

21 April 1982

Bernard – For your information.
Terry

Memo: Editorial Board Meeting, August, 1982

To: Editorial Board

The Editorial Board meets during the annual meeting of the Society. Ordinarily, we can get together only after the Council has met, hence must conduct some business by mail.

So far as I am aware, the only immediate matter requiring action is the election of a member of the Editorial Board. Dr. Bernard Lowy's term expires at the end of this calendar year. As you know, from an earlier memo, he has agreed to serve an additional five-year term if elected by the Board. However, members of the Board are free to recommend other candidates. No suggestions or recommendations have yet come to me.

I will be grateful if you will send me names of persons you would recommend for the Board vacancy; alternatively you may wish to vote for a second term appointment for Dr. Lowy. I enclose a postcard for your response, and ask that you reply promptly, please.

I know of no other business facing the Board at this time, but if you have matters to be brought to our attention, please let me know. There have been no complaints about the journal, it is getting out on time, and has no problems so far as I am aware. No one has proposed any pertinent matters to come before the Editorial Board. We have an excellent relationship with Allen Press, and they have been most cooperative and prompt. We continue to need reviewers, so any suggestions will be welcome.

I will send you a copy of my annual report to the MSA Council, prior to the meeting. As is customary, I will schedule (at Penn State) a meeting of the Editorial Board, and will contact there all members attending the AIBS meetings.

Once again I express my sincere thanks to each of you for your cooperation and help this past year. Your efforts to provide constructively critical reviews are most welcome, and make my task of dealing with authors infinitely easier. Your support bolsters my confidence in my own efforts. Thank you.

Terry

T.W. Johnson, Jr.

TWJ:jd

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

1-11-1982

Dear Roger,

I am not sure whether I sent you a check earlier, so in the event that you have not received one from me, I send you the enclosed.

Best regards,

Bernie

Instituto de Biología

DEPARTAMENTO DE BOTANICA

UNIVERSIDAD NACIONAL
AUTONOMA DE MEXICO
Apartado Postal 70-233
México 20, D.F.
Tel. 550 52 15 ext.

2 de abril de 1982.

Dr. B. Lowy
Department of Botany
Louisiana State University
Baton Rouge, La., 70803/1705
U.S.A.

Estimado Dr. Lowy:

Con esta fecha nos permitimos enviar a usted el manuscrito en Inglés del trabajo, que usted amablemente corrigió, al cual se le hicieron las correcciones por usted sugeridas.

Aprovechamos la oportunidad para darle también a usted las más cumplidas gracias por haber leído y corregido el manuscrito, a sabiendas que este representó mucho trabajo.

Mucho le agradeceríamos que se dirigiera en la correspondencia a nombre de las dos autoras.

Atentamente,

Dra. Evangelina Pérez-Silva. Dra. Regla Ma. Aroche Alfonso.

EPS'RGMAA'grm.

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CLARK T. ROGERSON
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The New York Botanical Garden
Bronx, New York 10458

4 January 1982

Memo: Pitt letter on book review

To: Editorial Board

You have received letter copies and documents from Dr. J.I. Pitt regarding the unfavorable review of his book by Robert A. Samson. In his letter, Dr. Pitt requests that some action be taken, and proposes three alternatives.

I call your attention to the Board's policies concerning controversial reviewers. Briefly, these are (1) MYCOLOGIA's pages will not be used to publish "letters-to-the-editor", and (2) in a controversy, the editor is responsible for making certain that both parties see each other's comments before any public "airing, and the editor of the NEWSLETTER will be asked to publish simultaneously one letter each from the author and reviewer provided both parties agree in writing to release their letters in this fashion."

The first policy does not permit us to agree to Dr. Pitt's second alternative, but the second allows us to agree to his third alternative (with his permission and that of Dr. Samson).

The Editorial Board is now asked to rule on Dr. Pitt's first alternative, namely, to have a second review published in MYCOLOGIA. I have told Dr. Pitt that I would poll the Board for their decision. He has also been informed of our policy (copy of my letter to him is enclosed).

I will be grateful if you will let me know whether or not to ask the Book Review Editor to solicit and publish a second review in the journal. I enclose a postcard for your response.

Sincerely yours,

Terry

T.W. Johnson, Jr.

TWJ:jd

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Bronx, New York 10458

4 January 1982

Dr. John I Pitt
CSIRO Division of Food Research
Food Research Laboatory
P.O. Box 52
North Ryde, NSW 2113, AUSTRALIA

Dear Dr. Pitt:

Your letter regarding Robert A. Samson's review of your book "The genus Penicillium" arrived on 2 January, and I hasten to reply. The matter is of course a serious one, and a solution should not be delayed. I have today written to each member of the Editorial Board (a copy of my memo is enclosed) explaining the situation as it now stands, and asking for their guidance (they are the immediate governing body for the journal, and also establish its operational policies.)

I can appreciate your concern and disappointment; unfavorable reviews are never pleasant. Because we have had similar instances in the past, the Editorial Board adopted a procedure for handling complaints about reviews. In brief, that procedure is as follows.

When it appears that a controversy over a review will occur between author and reviewer, the Editor-in-Chief becomes responsible for contacting both parties. We believe that differences of opinion in evaluating books should not be kept hidden from the principals. In the case at hand, I am charged with giving Dr. Samson opportunity to see your comments, and for you to see his rebuttal or response, if any. If both parties agree (in writing) to airing the matter "in public", I will ask the Editor of the NEWSLETTER to publish (simultaneously) letters or comments from both the author and the reviewer. Only one such response/rebuttal will be published. The Editorial Board has decided not to open the pages of MYCOLOGIA to "letters-to-the-Editor" type of communication. This is policy that I must follow, in conjunction with the Book Review Editor.

In your letter you propose three alternatives designed to inform the "public" of the nature of the review and your concerns. I now respond to those alternatives in light of Editorial Board policy. (1) The second alternate is not acceptable under current policy. (2) I have asked the Editorial Board to rule on whether or not to ask Dr. Hughes to seek and publish another review of your book. (3) The third alternative is acceptable under current policy, provided your comments are made known to Dr. Samson before any publication, and he in turn gives you opportunity to see any statements he might make in rebuttal. With permission of both parties I would then approach the Editor of the NEWSLETTER.

At this time I ask you for permission to copy your letter, to send it to Dr. Samson, and invite him to respond and provide you with copy of any statement he would care to make.

Perhaps this procedure will seem cumbersome to you, but it is necessary because I must insist that all parties involved be made fully aware of the matter in all details. I insist on this level of communication because to do otherwise is to open the door to endless argumentation in print in MYCOLOGIA or the NEWSLETTER.

The Editorial Board has declined to set policies and guidelines for the Book Review Editor. The Editor is selected after careful screening, and then is allowed to run his own shop. I am available to him for consultation and advice (as is the Editorial Board), but he must have freedom to handle his responsibilities in a professional manner that best suits his local situation. I must add in this regard that the Book Review Editor cannot be held responsible in any way for the accuracy of reviews submitted to him. The author of the review is alone answerable for accuracy, and the Editor could not possibly be expected to evaluate each review. (I do not hold myself responsible for the accuracy of articles in MYCOLOGIA - save in instances where I am a reviewer - and I do not believe the Editorial Board expects to hold me accountable.)

If you will permit me to contact Dr. Samson, I will do so at once, and pursue this matter vigorously to its end. I am sending a copy of this letter to the Editorial Board and to Dr. Hughes, and a copy will be forwarded to Dr. Samson when I have your permission. I will see to it that both you and Dr. Samson are given copy of all pertinent correspondence.

I apologize for this lengthy reply, but circumstances demand that all parties be aware of policy and procedure. In the meanwhile, I will be getting replies from the Board regarding your first proposal; I shall see to it that you are fully informed of their decision.

Sincerely yours,

T.W. Johnson, Jr.

TWJ:jd

cc: Dr. G.C. Hughes, Book Review Editor
Dr. Don Pfister, Newsletter Editor

CSIRO
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(Headquarters: North Ryde NSW)

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23rd December, 1981

Dr B. Lowy,
Dept of Botany,
Louisiana State University,
BATON ROUGE, Louisiana 70803
U. S. A.

Dear Dr. Lowy,

Review of "The Genus Penicillium" in Mycologia

I have recently received my copy of Mycologia for May-June 1981, with the review by Robert Samson of my book "The genus Penicillium" (Academic Press, London, 1980). I, and colleagues with whom I have discussed this matter, believe that Dr. Samson has published an inaccurate and unacceptably biased review of the book. As you are a member of the Editorial Board of Mycologia, I am enclosing a copy of a letter I have written to the Book Review Editor, setting out some of my reasons for this belief.

As I have explained to the Book Review Editor, Mycologia is rightly regarded as an authoritative journal, and hence I believe that the editors have a great responsibility to ensure that it provides accurate information, not only in papers, but in book reviews also. If after consideration you agree with my point of view, I would appreciate any action you can take in your official capacity to assist me in obtaining some measure of redress. I would be grateful for your help in any way that you see to be possible.

Yours sincerely,

John I. Pitt

John I. Pitt

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22nd. December, 1981

Dr. G. C. Hughes,
Book Review Editor, Mycologia,
University of British Columbia,
Vancouver, B.C. V6T 1W5
Canada.

Dear Dr. Hughes,

Review of "The genus Penicillium"

Copies of Mycologia come to Australia by surface mail, so I have received only recently the May-June 1981 issue which contains Robert Samson's review of my book "The genus Penicillium".

I must say that I am deeply disappointed. I am upset not because the review is critical - any work of this magnitude by a single author must fall far short of perfect - but because the criticism so obviously lacks objectivity. Several of Dr. Samson's statements, in fact, are simply erroneous.

I appreciate the fact that editors have the right to publish as they see fit, and sometimes are not in a position to judge the accuracy of reviews. Nevertheless I believe that editors have a great responsibility to ensure accuracy both because reviews in Mycologia are regarded as authoritative, and because authors have no right of reply to, or means of redress against, biased or inaccurate reviewing.

I wish therefore to draw to your attention errors of fact in Samson's review, and also some of his less objective statements.

A. Errors of fact [red numerals]

1. P. 582, para. 1, 3rd. sent. "He bases his divisions on using growth, temperature and water relations". Not so: in "The genus Penicillium", subgeneric and sectional classification is based entirely on microscopic or (in one subgenus) macroscopic morphology.

2. P. 583, para. 1, l. 6. "The restrictions of the genus to only green species.." Not so: the classification includes several species which produce blue conidia, and 5 species in which conidia entirely lack blue or green pigments: P. arenicola, P. humuli, P. olivicolor, P. brunneum, and P. digitatum. The genus limits, as defined on p. 155, are based on microscopic morphology; most species just happen to be green.

3. P. 583, para. 2. "Pitt divides Penicillium into 4 subgenera: ... determined by the penicillus type, ..." Correct, but Dr Samson goes on "In the resulting classification the growth on G25N, however, proved often to be more important than the morphology of the penicillus..." Not so: subgeneric classification is based strictly on penicillus type - on microscopic morphology alone.

4. P. 584, last para., l. 9. "The arbitrary classification..." As noted above, this book bases subgeneric classification entirely on penicillus morphology; and sectional classification entirely on microscopic or macroscopic morphology. Series are based on the abovementioned characters

plus colony diameters, and species are based on these characters plus colours. Is this arbitrary? With the exception of colony diameters, all of these characters have been used by all previous 20th Century monographers of Penicillium.

5. P. 584, last para., l. 9. "...the incomplete documentation..." This book contains the most complete documentation ever assembled of taxonomic literature on the species of Penicillium described up to the end of 1975. While living in London, months were spent on a literature search of all species names described since 1821, including many not fully documented by Raper and Thom, "Manual of the Penicillia", 1949 (hereafter termed simply "R & T"). For example, on p. 600, a bibliographic index page chosen at random, see P. socium, P. subtile var. ramosium, P. tenuis, P. tenuissimum. The few species references I was unable to locate or examine personally have been clearly indicated (see P. verruculosum, p. 576; P. versicolor, p. 601).

P. 584, para. 3, l. 3. "The bibliography of mainly taxonomic references ...." This is a taxonomic text, with no other expressed aim. "....comprises 9 pages...." Samson ignores the 48 pages, in small type, of references to nearly 1000 species names. "....and lacks several important contributions ...." There are only 3 unreferenced "important contributions" of which I am aware. First, the species compendium of Kulik, U.S.D.A. Handbook No. 351, 1968, which was omitted because it contains no new taxonomic information. Second, the paper mentioned by Samson on p. 583, last para., by Jorgensen & Gunnerbeck (Taxon, 26: 581, Nov. 1977), which was received by me too late to be included readily; in the opinion of Dr David Hawksworth it shed little light on a complex situation. The third is reference to the genus Trichocoma, an omission worthy of criticism, but without effect on the accuracy of the book or its usefulness.

6. P. 584, last para., l. 10. "....together with the time-....consuming identification procedure...." "The genus Penicillium was painstakingly planned to enable positive identifications in 7 days, a much shorter time than is possible using the identification schemes of R & T or Stolk, Samson and coworkers.

7. P. 582, para. 2, l. 8. "....because one can use the keys only if each isolate is grown on 5 (five) Petri dishes ...." Not so; the regime outlined on p. 19 utilises seven Petri dishes for two cultures, i.e. 3.5 plates per culture. While this consumes 16% more materials than the three media (and plates) recommended by R & T, it is 40% less than Samson indicates.

8. P. 582, opening sentence. "....identification is still problematic due to the confusing taxonomy and literature". Also p. 584, second last sentence: "This book does not contribute to a more stable Penicillium taxonomy and will probably only increase the already existing confusion". Penicillium must be almost unique among fungal genera: its circumscription has changed little since the early years of this century, and its classification has followed a single pattern since that time. "Confusion", in my view, arises only from (1) failure by R & T to follow the provisions of the Botanical Code, necessitating many name changes now (undesirable indeed, but unfortunately unavoidable) and (2) the quite radical departure from R & T species concepts by Samson, Stolk and Hadlok, Stud. Mycol, Baarn 11: 1, 1976, who combined no less than 20 R & T species under the name P. verrucosum. Samson et al. are entitled to have done this; but it is inaccurate to suggest that confusion is due to my work rather than to the two points mentioned above.

9. P. 582, para. 2, l. 5. "....and Penicilliopsis, which in nature normally only produces Penicillium synnemata". Both the illustration by Fennell in Ainsworth et al., "The Fungi" Vol. 4A, Fig. 17d, p. 54, 1973, and the description by Malloch and Cain, Can. J. Bot. 50: 2623, 1972, of yellow conidia up to 16 µm long, borne on phialides up to 28 µm long, clearly demonstrate that the anamorph of Penicilliopsis is not a Penicillium.

B. Lack of objectivity [blue numerals]

In any review it is expected that personal opinion will be expressed. However it is important that the reader be able to gauge what is fact and what is opinion. Some examples follow of opinion not distinguished from fact in Samson's review.

i. P. 583, l. 11. "The resemblance of P. (Eladia) sacculum to P. restrictum .... is so obvious...." This is only Samson's opinion.

ii. P. 583, para. 2, l. 6 "....many related species (e.g. P. marneffei and P. citrinum; P. brevicompactum and P. olsonii) are incorrectly arranged". Again, this is only Dr. Samson's personal opinion. I believe that even casual examination of the illustrations of P. marneffei (p. 432) and P. citrinum (p. 295), especially of the penicilli, will show there is no close resemblance. Similarly the descriptions of the microscopic morphology of P. brevicompactum (p. 373) and P. olsonii (p. 393) will show a resemblance at the subgeneric level, not more.

iii. P. 584, last para. "For mycologists who prefer to use a microscope for identification of fungi...." implies both that Penicillium species can be identified by microscopy alone, and that my work places little or no reliance on microscopic morphology. Neither implication is accurate.

iv. P. 584, last para., l. 4. "....new circumscriptions, sometimes differing greatly from conventional concepts...." In compiling this taxonomy, the greatest care has been taken to examine the original descriptions and illustrations, and wherever possible, original and authentic isolates, so that the species circumscriptions are compatible with those of the original describer. Where descriptions differ from "conventional concepts", whatever that term may mean, then the conventional concept is wrong - or I have made an honest mistake. I am aware of none, nor has Samson pointed out any such errors.

v. P. 584, first line. "The species concept is unclear as is illustrated by the statement on p. 528: '....it is as difficult for me to state my concept of a Penicillium species....as it was for....Raper and Thom.'" My statement has been taken out of context. In context, this sentence was a summary of a short section (pp. 527-8) in which I pointed out the difficulty of delimiting species in a genus such as Penicillium. The previous paragraph (p. 528, para. 2) is an honest attempt to demonstrate the derivation of my concepts - something previously attempted only by Charles Thom, Ann. N.Y. Acad. Sci. 60: 24, 1954.

vi. P. 583, last sentence. "The critical reader will miss several recently described taxa....". To the critical reader of the book, the reason will be obvious, though perhaps not to the reader of the review. No species described after 1975 are included, because of the time lags inevitable in cataloguing in "Index of Fungi", and in typing, typesetting,

proofreading and printing of a book. Concerning "P. radiolobatum (1972)": I have never heard of this species and it is not recorded in "Index of Fungi".

vii. P. 584, para. 2, l. 3. "....new species...,I. gossypii, is very similar to T. assiutensis (1978), a species not included in the book". T. assiutensis was published (in Europe) only a few weeks before "The genus Penicillium" was sent to the publishers.

I could go on, but I believe that I have made my point. I would be grateful if you, or the Editorial Board of Mycologia, would do one of three things, listed in my order of preference:

1. Publish a second review of "The genus Penicillium" in Mycologia, by an unbiased North American. Richard Hanlin, Donald Wicklow, Philip Mislivec, Larry Beuchat, Charles Hodges, Gloria Warner or David Malloch would all be obvious candidates. I could readily provide a new copy of the book for this purpose.

2. Publish all of, or extracts from, this letter in Mycologia as soon as possible.

3. Publish this letter, or extracts from it, in the next issue of MSA Newsletter.

I appreciate that asking for any of these alternatives is demanding a great deal, but I feel sincerely that the review in Mycologia was unjust. To emphasise this point, I enclose a selection of reviews of "The genus Penicillium" from Europe, the U.S.A. and Australia.

I have sent copies of this letter and the other enclosed documents to each member of the Mycologia Editorial Board.

Yours sincerely,

John I. Pitt

John I. Pitt, Ph.D.
Principal Research Scientist

previous editions, have maintained an enthusiastic approach to mycology. The questions and frustrations experienced by those working with fungi are fully amplified in a stimulating way. It will be difficult to find a better text in which to get someone "hooked on mycology." More research minded students will find the extensive bibliographies at the close of each chapter extremely useful. A large majority of these appeared since the publication of the second edition. The new text will serve admirably for both the undergraduate and graduate courses in mycology. The authors have done a superb job of organizing and writing the text; the publisher, a fine job in printing; and the marketing managers have maintained a very reasonable price. All with an interest in fungi will want a copy.—JAMES W. KIMBROUGH, Dept. of Botany, University of Florida, Gainesville, FL. 32611.

The Genus Penicillium and its teleomorphic states Eupenicillium and Talaromyces, by John I. Pitt. Academic Press, London, New York, Toronto, Sydney, San Francisco, 1979. vi + 634 p. Price, $92.00.

Although the Penicillia are among the most common fungi, identification is still problematic due to the confusing taxonomy and literature. In the book which reached us in the spring of 1980, John Pitt presents a new taxonomic treatise of this important genus. He bases his divisions on using growth, temperature and water relations.

Following an introduction and historical review, Pitt discusses the two teleomorphic states of *Penicillium*, *Eupenicillium* and *Talaromyces*. It is surprising that he does not mention two other ascomycetous genera: *Trichocoma*, which has a beautiful *Penicillium* anamorph, and *Penicilliopsis*, which in nature normally only produces *Penicillium* synnemata. In Chapter 4 directions are given for the isolation, cultivation and preservation of *Penicillium* isolates. This is a very important part of the book because one can use the keys only if each isolate is grown on 5 (*five*) Petri dishes with Czapek Yeast Autolysate (CYA), malt extract and 25% glycerol nitrate (G25N) agars. These plates have to be incubated at 5°, 25° and 37°C and colony diameters are measured after 7 days. After the chapter with the instructions for examination and characters used in the classification the systematic arrangement of the *Eupenicillium* species is given. The synoptic key, which also includes certain sclerotinogenic *Penicillium* taxa, emphasizes the growth on CYA after 7 days, a period in which most *Eupenicillium* species have not matured. In Chapter 7 the genus *Penicillium* is defined and subdivided. *P. ingelheimense* and *P. lavendulum* and related species are transferred to two new genera, *Merimbla* and *Geosmithia*. The descriptions of these genera were published in the Canadian Journal of Botany 57 (1979) and it is unfortunate that the species descriptions have not been included in the book for comparison. The restrictions of the genus to only green species with ampulliform to acerose phialides is very debatable, particularly when taxa such as *P. camemberti*, *P. digitatum* and *P. arenicola* are still included. It is also unclear why *P.* (*Gliocladium*) *vermoesenii* has been omitted. This species has a typical *Penicillium* morphology even though it is pink. The resemblance of *P.* (*Eladia*) *sacculum* to *P. restrictum*, *P. dimorphosporum*, *P. megasporum* is so obvious that maintainance of the genus *Eladia* is unfounded.

Pitt divides *Penicillium* into 4 subgenera: *Aspergilloides*, *Penicillium*, *Bivertillium* and *Furcatum* and each subgenus into sections and series. This division is determined by the penicillus type, while the growth on G25N agar "is included to aid recognition of this type". In the resulting classification the growth on G25N, however, proved often be more important than the morphology of the penicillus and many related species (e.g. *P. marneffei* and *P. citrinum*; *P. brevicompactum* and *P. olsonii*) are incorrectly arranged.

In the following chapters the descriptions of the taxa are given. Each description mainly contains growth characters, while the morphological diagnosis is short and sometimes poor. Each species is illustrated by a photograph of the colony on agar and Nomarski interference micrographs of the penicillus and conidia. It is a pity that this good attempt to illustrate each species is somewhat diminished by the low quality of many plates. In many cases a camera lucida drawing would have been preferable.

A positive aspect of Pitt's monograph is the long overdue revision of the nomenclature. Many corrections were necessary, though I question the introduction of old and obscure names such as *P. glabrum*, *P. hirsutum* and *P. mineoluteum*. The species are typified, mostly with neo- or lecto-types. The genus is neotypified according to the proposal made by Hawksworth et al. (1976), but it is unfortunate that the plausible proposition for typification made by Jorgensen & Gunnerbeck (1977) is not discussed. Twenty six new names are proposed, of which the majority are nomenclatoral changes following Pitt's interpretation of Art 59. The 134 accepted species are placed in an analytical key rather than the synoptic keys used for *Eupenicillium* and *Talaromyces*. The critical reader will miss several recently described taxa e.g. *P. brevissimum* (1976), *P. radiatolobatum* (1972), *P. hispanicum* (1978), *P. palmensis* (1978).

The species concept is unclear as is illustrated by the statement on p. 528: "In short it is as difficult for me to state my concept of a *Penicillium* species in overall terms as it was for Thom and Raper and Thom". In some instances emphasis is placed on the colony diameter, in others on the morphology. Using his criteria Pitt lumps species of different morphology or ecology (e.g. *P. jensenii* and *P. malgiovense; P. herquei* and *P. coralligerum*), while some taxa with an identical morphology are separated by only a slight deviation in colony diameter (e.g. *P. verrucosum* and *P. viridicatum*).

Chapter 12 comprises the classification of *Talaromyces*, which mainly follows the lines of Stolk and Samson (1972). Again the species are keyed out synoptically. One of the two new species described here, *T. gossypii*, is very similar to *T. assiutensis* (1978), a species not included in the book.

In the chapter "Polyglotta," speciation, ecology, mycotoxin evolution and species concepts are briefly discussed, while in chapter 14 accepted, doubtful and rejected epithets are listed. The bibliography of mainly taxonomic references comprises 9 pages and lacks several important contributions.

For mycologists who prefer to use a microscope for identification of fungi rather than numerous Petri dishes, this book is a disappointment. Particularly, the applied mycologists will have difficulties with the many nomenclatoral changes and new circumscriptions, sometimes greatly differing from conventional concepts, and I feel sorry for this group of researchers, who wish a simple and practical but reliable system. This book does not contribute to a more stable *Penicillium* taxonomy and will probably only increase the already existing confusion. The arbitrary classification, the incomplete documentation together with the time- and material-consuming identification procedure, contradict the statement on the coverleaf that this book "will become the standard and essential reference work to *Penicillium* for some decades".—ROBERT A. SAMSON, Baarn.

Fungal Viruses. Edited by H. P. Molitoris, M. Hollings, and H. A. Wood. Springer-Verlag, Berlin, Heidelberg, New York. 1979. xiv + 194 p. Price, $31.90.

This book consists of papers and abstracts given at a symposium with the same title held in Munich in September 1978 during the 12th International Congress of Microbiology. It presents a rather mixed selection of glimpses of developments in this field. Many of the authors have interesting things to say but several of them have already said their pieces elsewhere (e.g. Viruses and Plasmids in Fungi, edited by P. A. Lemke, Marcel Dekker, New York, 1979). The chapters by R. F. Bozarth on physicochemical properties and by K. W. Buck on virion-associated polymerases are more fully covered in the book edited by Lemke but nevertheless are useful, if short, summaries.

As a geneticist I found the chapters on infectivity and transmission (H. Lecoq *et al.*), morphological changes induced in fungi (J. Albouy) and interactions of fungal viruses and secondary metabolites (R. W. Detroy & K. A. Worden) less interesting than the two papers dealing with killer systems in yeast (M. H. Vodkin & G. A. Alianell) and *Ustilago* (Y. Koltin & R. Levine) which record important contributions to understanding virus gene function. In his short chapter on taxonomy M. Hollings proposes a set of "minimum essential information" for mycovirus taxonomy. This should be very useful in guiding those who are interested in where the viruses they work on fit in the overall scheme that will no doubt emerge in the coming years. Three papers on methods deal with dsRNA and virus detection in; *Histoplasma capsulatum* (J. P. Adler), *Agaricus bisporus* by immunochemical techniques (V. G. Del Vecchio *et al.*) and a range of fungi by serological methods (R. M. Lister).

Symposium contributions are usually ephemeral because weighty, comprehensive treatments make for restless, inattentive audiences. Nevertheless this book, although light in weight, I recommend as a convenient entry point for those who wish to know more about fungal viruses and where the growing points are.—P. R. DAY, Plant Breeding Institute, Cambridge.

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A.H.S. Onions, Trans. Br. mycol. Soc. 76: 521-522, 1981

The Genus *Penicillium*. By JOHN I. PITT. (Academic Press, London, New York, Toronto, Sydney, San Francisco, 1979, 1980.) Pp. 634. Price £40.

'Rare indeed must be the human individual who has not encountered fungi of the genus *Penicillium* or been affected by one of the many metabolites produced by them.' Thus John Pitt starts his introduction to his new monograph on the genus *Penicillium*. As he states near the end 'as the purpose of this work is primarily taxonomic, it is not the purpose of this work to review current knowledge of the ecology of *Penicillium*' and indeed the work is one based entirely on the taxonomy of *Penicillium* and its teleomorphs.

It was in 1949 that Raper and Thom produced the last major revision of the genus that has been accepted and internationally available. This was a fine and useful work, but the last 30 years has been a period of change in the field of mycology and in the interval many new species have been described, techniques developed, concepts changed, new antibiotics found and the importance of mycotoxins appreciated. All of which stresses the need for a modern reliable standard book for the identification of *Penicillium* isolates. This is what J. Pitt aimed to do and has succeeded in producing.

It is a large book of 634 pages packed with information and strictly adheres to its aim as a taxonomic study. After introductory chapters on the history of the genus, discussion of teleomorphic states (J. Pitt has adopted modern terminology), isolation, cultivation and preservation and the characters used in classification he goes on to the serious descriptive chapters on the teleomorphic genera and subgenera of the genus *Penicillium*. This is followed by a very short account labelled polyglotta of the other aspects of the genus such as evolution, species concept, ecological notes, parameters influencing growth, specific habitats, mycotoxins and a summary of growth data. Finally there are some 90 pages of useful lists of species epithets, accepted species, disposition of species accepted by Raper and Thom, epithets applied to species of *Eupenicillium*, *Talaromyces* and *Penicillium*, indeterminative and excluded names, new species, a comprehensive bibliography, glossary – no doubt important to those not familiar with the new terms – and a useful index.

J. Pitt follows modern taxonomic treatment by including those species which produce a perfect state under the teleomorphic name. Thus there are two chapters dealing with teleomorphs, *Eupenicillium* for species with sclerotium-like cleistothecia and *Talaromyces* with soft cleistothecia. The anamorphs of species which sometimes produce cleistothecia are cross-keyed where this is considered necessary. Synoptic keys have been used for both cleistothecial genera. This type of key is generally popular but in this case some workers have experienced difficulties, which may be due to the emphasis laid on growth characters. The use of teleomorphic names for cleistothecium-producing *Penicillium* species has previously been questioned as being confusing to the many non-taxonomists working with Penicillia. However, the monograph covers both the teleomorphic (cleistothecial) and anamorphic (imperfect) species in the one book, and in practice the majority of isolates of economic importance fall in the anamorphic form genus or true *Penicillium*.

In the anamorphic section primary separation is based on penicillus type, although the author has redefined the forms adopted to give four subgenera – *Aspergilloides* (the old Monoverticillata), *Furcatum*, *Penicillium* and *Biverticillium*. Further separation is based on growth data and colony morphology, but the author avoids colony texture, a character used extensively by Raper and Thom and which proved so difficult of comprehension at least to workers new to the genus. However, with the greater importance placed on growth characters such as effect of nutrient, temperature and water activity at least 5 Petri dishes are required for identification of each isolate.

The subgenera are divided into sections and series. After a short introduction these are followed by descriptions of individual species. These are in a standard form consisting of sections on synonymy, morphological descriptions, typifications, distinguishing characters (a particularly useful paragraph), taxonomy, affinities, occurrence and distribution and a list of isolates examined. Black and white photographs of 2 Petri dish cultures, a typical penicillus and conidia are given. For the penicillus and conidia interference microscopy has usually been used. Very few drawings have been included.

I miss the more comprehensive introduction to the series and species and rekeying which was one of the pleasant characters of the old book, but took a lot of space.

J. Pitt has reduced many species to synonymy and the general situation has been simplified. However, we have lost some common and familiar names because he has revived some old Dierckx names, e.g. *P. cyclopium* becomes *P. aurantiogriseum*, as he believes these are recognizable. Samson and his colleagues have undertaken revisionary work on some sections of the 'Penicillia' but Pitt has only followed this work to any extent in the genus *Talaromyces*. However, the separation of species in a variable genus like *Penicillium* will remain a matter of opinion as to where the species lines should be drawn and will always open to controversy. For the important area of the '*Penicillium cyclopium/expansum/verrucosum/aurantiogriseum*' complex we have authoritative work by Raper and Thom (who have the advantage of familiarity), Fassatiova, Samson and his colleagues and now Pitt. All have good bases for their opinions. However, it seems likely that the most recent work which is all in our book and also includes a reliable basis for separation and keys will prove the most popular. It will be a brave worker who undertakes a similar task again in the near future.

John Pitt must be congratulated on producing such a comprehensive and useful work on this difficult genus and certainly all those who hope to do any serious work on the genus will feel the need to have a copy preferably on the bench or at least, in view of the price, available in the library.

AGNES H. S. ONIONS

G.J.F. Pugh, J. Appl. Bacteriol. 49: 537-538, 1981

THE GENUS *PENICILLIUM* AND ITS TELEOMORPHIC STATES *EUPENICILLIUM* AND *TALAROMYCES* (1979). J.I. Pitt. 634 pp. London: Academic Press. £40.00; $92.00.

The identification of species of *Penicillium* has never been easy, and all too often the unhelpful aggregate *Penicillium* spp. appears in papers and books concerned with the occurrence and activities of these fungi. They are amongst the most widespread both globally and in the range of habitats, and it is especially important to obtain correct identification when the specimen in question produces mycotoxins (as Dr Pitt points out, penicillin is a mycotoxin that affects only bacteria).

In the past, identifications have been made using *The Penicillia* by C. Thom (1930 London: Bailliere, Tindall & Cox) or *A Manual of the Penicillia* (Raper, K.B. & Thom, C. 1968 facsimile of the 1949 edition, London & New York: Hafner Publ. Co.). Pitt brings in a number of changes, of which the adoption of dual naming of the ascosporic species is perhaps the most controversial. Those with cleistothecia are assigned to *Eupenicillium* and those with gymnothecia to *Talaromyces*. He has assessed the status of the many descriptions of species of *Penicillium* and has recognized as valid only 150 species of *Penicillium*, 37 of *Eupenicillium* and 16 of *Talaromyces*. He has approached the identification of the species he accepts on the basis of descriptions of individuals which have been grown under standardized conditions: incubation for 7 d at 5°, 25° or 37°C on Czapek yeast autolysate agar, malt extract agar and 25% glycerol nitrate agar (G25N).

A major criterion then used is the colony diameter, as the growth data can be used to differentiate species, series and most subgenera, as growth on G25N is highly correlated with the type of penicillus produced. The other important criteria used are concerned with the characteristics of the microscopic morphology, and of the colony.

The bulk of the book consists of detailed descriptions of the species, which occupy about three-quarters of the text. They are illustrated with photographs of the colony, which are not always very helpful, and of the conidiophores. These are mainly illustrated with Nomarski interference light microscopy, and some could with advantage be replaced by line drawings.

The synoptic keys for *Eupenicillium* and *Talaromyces* appear complex, but as Dr Pitt says 'in practice use is simple and rapid'. Each species is represented by a number, and by listing those with a positive criterion, and subsequently eliminating those lacking second and subsequent criteria, a single name should emerge. Keys for *Penicillium* are of the normal dichotomous type and are given at the beginning of each subgenus. This causes a certain amount of searching through the book, and the keys may well have been better placed immediately following the separation of the four subgenera.

The remainder of the book contains chapters on the history of the genus and the names given to the perfect states, and detailed notes on the handling and examination of cultures. Growth characteristics are given in tabular form. All the accepted epithets are listed, and the disposition is given of the more recently erected species as well as those accepted by Raper & Thom. There is a short glossary and a comprehensive index.

Any book such as this will stand or fall by the use made of it and the ease or otherwise of working with it. Not everyone will have ready access to incubators preset at the three required temperatures, nor perhaps to the three media, and they will be at a great disadvantage. With these exceptions I can see many workers who have to handle isolates of unknown species of *Penicillium* making use of this book, and I foresee, increasingly, the appearance of *Eupenicillium* and *Talaromyces* in ecological and physiological papers as a result.

This book may not be immediately accepted by all workers, but it offers many advantages over the earlier volumes. As it draws together information on all published species, it should be in every laboratory where these fungi are handled. The two earlier volumes were in use for 19 and 31 years before a major revision appeared: I can visualize Dr Pitt's book still in use well into the next century.

G. J. F. PUGH

C'est dans le traitement des *Penicillium* s. str. que s'exprime davantage l'originalité de l'ouvrage. L'auteur propose un découpage en quatre sous-genres, fondés essentiellement sur la morphologie du pinceau conidiogène. A l'intérieur des sous-genres, la reconnaissance des espèces groupées sur la base de similarités morphologiques en sections et séries, repose essentiellement sur l'observation macroscopique des colonies, dans des conditions de culture non seulement précises mais variées. Le diamètre des colonies (mesuré à 7 jours) exprime la capacité de la souche à se développer, d'une part dans des conditions «normatives»: à 25°C sur milieu de Czapek enrichi à l'extrait de levure (CYA) et sur malt-agar (MEA); d'autre part à la même température sur un milieu hypertonique à 25% de glycérol (G25N); enfin sur CYA à deux températures limites: 5°C et 37°C. La morphologie microscopique et les caractères biométriques interviennent en dernier lieu dans la description des espèces.

Ainsi la démarche de l'auteur est sensiblement la même que celle de RAPER et THOM; les conditions de culture sont, pour une part, comparables. Mais la hiérarchie des caractères est interprétée différemment, de sorte que la classification apparaît, au premier abord, profondément modifiée.

Les sous-genres *Aspergilloides*, à phialides insérées directement sur le stipe, et les *Biverticillium*, où un verticille de métules s'interpose entre le stipe et les phialides étirées, correspondent sensiblement aux sections «Monoverticillata» et «Biverticillata symmetrica» de RAPER et THOM. Le sous-genre *Furcatum* regroupe les espèces régulièrement ou irrégulièrement biverticillées, mais à phialides ventrues et (secondairement) à croissance plus rapide que celle des *Biverticillium* sur un milieu hypertonique à 25% de glycérol; chez RAPER et THOM, on trouve ces espèces dispersées parmi les sections et sous-sections: Divaricata, Velutina, quelques Monoverticillata ramigena, et même Biverticillata symmetrica (ser. *herquei*). Sans nul doute, la définition de ce sous-genre et plus particulièrement de la section *divaricatum*, à pinceaux irréguliers, est opportune; la difficulté d'attribution des souches aux catégories «divariqués» ou «ramifiés» se trouve heureusement levée.

Le sous-genre *Penicillium*, le plus vaste et le plus diversifié, accueille toutes les espèces à pinceau dissymétrique compact, de structure complexe (trois niveaux de ramification et parfois plus), c'est à dire la plupart des Assymetrica du «Manual». Mais les critères retenus pour diviser le sous-genre en sections et sous-sections, et parvenir à des «séries» d'un petit nombre d'espèces similaires, sont autres que ceux adoptés aux mêmes fins par RAPER et THOM. Comme SAMSON et al. (1976), PITT accorde un intérêt mineur à la texture des colonies, qui fonde la division classique en velutina, funiculosa et fasciculata. Les sections sont ici définies par les particularités microscopiques de l'appareil conidien.

A l'exception de quelques espèces assignées aux sections *cylindrosporum* (*P. italicum* et aff.), *Inordinate* et *Coronatum* (chacune une seule espèce), tous les *Penicillium* sous-genre *Penicillium* possèdent un même type de pinceau à conidies sphériques ou ellipsoïdes portées par des phialides ampulliformes; en outre, la plupart se développent bien sur milieu hypertonique; aucun ne pousse à 37° C. La section *Penicillium* qui englobe ces formes typiquement représentatives du genre est la plus vaste, avec 16 espèces groupées en quatre séries seulement (au lieu de 11 séries chez THOM et RAPER). Les espèces sont définies en termes plus larges mais plus homogènes que dans le «Manual» et, en principe, leur identification devrait être plus aisée. Malheureusement, les synonymies reconnues par l'auteur ne sont pas exactement celles qu'admettent SAMSON et al. et, dans un certain nombre de cas, il faudra faire un choix inconfortable entre la révision proposée par les spécialistes néerlandais et celle de PITT. Celui-ci convient d'ailleurs que le sous-genre reste «taxonomiquement difficile»; à ce propos, on lira avec profit le chapitre final intitulé «Polyglotta», où il envisage en premier lieu «ce qu'il faut faire lorsque les clés ne marchent pas».

Une remarque encore sur la nomenclature. L'auteur, soucieux de traditions authentiques, s'est intéressé aux espèces proposées au début du siècle par le belge DIERCKX, et remarquablement analysées par son disciple BIOURGE (1923). Il est ainsi amené à reconnaître dix des espèces «dierckxiennes» qui, dans quelques cas, ont la priorité sur des binômes universellement adoptés

dès les premiers travaux de THOM. Ainsi réapparaît fort heureusement le *P. griseo-fulvum* (*P. patulum* Bain.) déjà réhabilité par SAMSON et al.; mais le *P. cyclopium* Westl., une des espèces les plus répandues et les plus fréquemment citées, s'efface en faveur d'un *P. aurantio-griseum* Dierckx au nom beaucoup moins évocateur. On pourra regretter aussi la disparition de *P. frequentans*, *P. nigricans* et quelques autres qui nous étaient devenus familiers.

En marge du traitement taxinomique des *Penicillium* qui fait l'objet principal de cet ouvrage, il faut souligner l'intérêt des informations qui concernent l'écologie et la physiologie de ces moisissures, leur présence dans l'alimentation humaine et leur toxicité potentielle. On appréciera aussi la table qui résume clairement la capacité de croissance des 150 espèces dans les conditions définies dans le texte, le glossaire des termes spécifiques, ainsi que les index et la bibliographie qui complètent le volume. Enfin une typographie aérée, un plan rigoureusement établi pour chaque description d'espèce, sont des attraits non négligeables.

La présente monographie comble un vide de trente ans et, à ce titre, elle était vivement attendue. S'imposera-t-elle désormais comme le système de référence fondamental pour la détermination des *Penicillium*? Au premier abord, la méthode paraît très contraignante, avec ses trois milieux de culture et ses trois températures d'incubation; et les changements de nomenclature ont de quoi déconcerter ou irriter. Mais, à notre avis, il vaut largement la peine d'essayer. Il est indispensable, certes, d'expérimenter consciencieusement le «système», sans en négliger aucune étape; sans préjugés aussi, et sans lui demander plus qu'il n'a la prétention de fournir, c'est à dire un outil commode pour l'attribution d'une dénomination spécifique à une souche particulière de *Penicillium*. L'auteur lui-même reconnaît ce que cette démarche comporte toujours de subjectivité: «... *Penicillium* species, like beauty, lie in the eye of the beholder».

J. Nicot

P. Hübsch, Zeitschrift für Allgemeine Mikrobiologie 21: 629, 1981

J. I. Pitt, The Genus *Penicillium* and its Teleomorphic States *Eupenicillium* and *Talaromyces*. 634 S., 132 Abb. London-New York-Toronto-Sydney-San Francisco 1979. Academic Press. $ 92.00.

Dreißig Jahre nach „A Manual of the Penicillia" von Raper und Thom bringt Pitt eine völlig neue Bearbeitung dieser Gattung heraus. Dies wird sicher von vielen Mykologen in aller Welt, die in irgendeiner Weise mit dieser in vieler Hinsicht sehr wichtigen Gattung zu tun haben, sehr begrüßt.

Der Autor stellt in seinem Buch 150 Arten der Gattung *Penicillium* vor. Allerdings hat er, im Gegensatz zu Raper und Thom, diejenigen Arten, die eine Hauptfruchtform (oder wie der Autor es nennt —Teleomorph) bilden, in den Ascomycetengattungen *Talaromyces* und *Eupenicillium* (früher *Carpenteles*) untergebracht und folgt damit den internationalen Nomenklaturregeln (Artikel 59). Raper und Thom hatten die hohe Artenzahl aus „The Penicillia" (1930) bei ihrer Bearbeitung im Jahre 1949 auf 137 reduziert. Inzwischen wurden viele neue Arten beschrieben. Diese wurden kritisch verglichen und oft in die Synonymie verwiesen. Ein Zuwachs von 13 Arten in dreißig Jahren scheint real.

Die bisherigen *Penicillium*-Arten, bei denen Kleistothecien gefunden wurden, werden — wie erwähnt — den Gattungen *Eupenicillium* und *Talaromyces* zugeordnet. Die in der Gattung *Penicillium* verbleibenden Arten werden auf die Untergattungen *Aspergilloides*, *Furcatum*, *Penicillium* und *Biverticillium* verteilt. Für jedes Subgenus existiert ein dichotomer, analytischer Schlüssel, während für die Gattungen *Eupenicillium* und *Talaromyces* je ein synoptischer Schlüssel erarbeitet wurde.

Das Klassifikationsschema der vorliegenden Bearbeitung stützt sich in erster Linie auf mikroskopische Merkmale, in zweiter Linie auf Wachstumsdaten und drittens auf die Koloniemorphologie. Weitere Charakteristika kommen vom teleomorphen Stadium. Die Beschreibung des Wachstums bezieht sich auf drei verschiedene Nährböden bei Temperaturen von 5 °C, 25 °C und 37 °C. Die Beschreibungen beinhalten Koloniecharakter, Typ der Konidiophorverzweigung und weitere Merkmale des Konidiophors, Konidien, Ascosporen und Kleistothecien (soweit sie gebildet werden). Das Buch enthält nur Schwarz-weiß-Abbildungen, aber diese sind sehr gut und informativ.

Wichtig erscheinen mir bei den Beschreibungen der einzelnen Arten die Hinweise auf ähnliche, leicht verwechselbare Pilze, was sicher nicht nur für den Anfänger in der Bestimmung dieser schwierigen Gruppe von großem Wert ist. Auch die Angaben über die Stämme, die untersucht wurden, und deren kritische Bewertung haben hohen praktischen Wert. Ein alphabetisches Register (48 Seiten) aller für *Penicillium*, *Talaromyces* und *Eupenicillium* verwendeter Epitheta mit Hinweisen auf die Synonymie ist sehr hilfreich.

Der taxonomischen und deskriptiven Bearbeitung werden einige einleitende Kapitel vorangestellt. Eines davon bringt einen kurzen Abriß der Geschichte dieser Pilze. Ein weiteres behandelt die Beziehungen zu den teleomorphen Stadien. In knapper Form wird auch das notwendige theoretische Rüstzeug für die Isolation, Kultivierung und Aufbewahrung der Stämme behandelt. Dabei wird besonders auf den Wert der Lyophilisierung, vor allem von Typus- und anderen wichtigen Stämmen, hingewiesen. Die Anwendung dieser Methode auf das in den großen Sammlungen vorhandene Stammaterial bildete eine wichtige Grundlage für die vorliegende Bearbeitung. Schließlich wird in Kapitel 5 erläutert, wie die einzelnen Merkmale für die Bestimmung zu benutzen sind. Eine Bibliographie der wichtigsten Arbeiten und ein Glossary schließen das Buch ab.

Das inhaltlich einwandfreie und auch drucktechnisch sehr gute Werk wird für diejenigen, die mit diesen Pilzen zu arbeiten haben, eine wertvolle Hilfe sein.

P. Hübsch (Weimar)

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REVUE DES LIVRES

par

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THE GENUS *PENICILLIUM* AND ITS TELEOMORPHIC STATES *EUPENICILLIUM* AND *TALAROMYCES*, par John I. PITT, viii + 634 p., 132 fig., in 8°, relié toilé, "1979" (publication date communicated by A.P. being 13.2.1980). Academic Press, 24-28 Oval Road, London NW1 7DY, UK. Prix: US 92.-.

Depuis 1949, le *Manual of the Penicillia* de Raper et Thom était la seule monographie relativement complète disponible pour l'identification des *Penicillium*. Elle reconnaissait 141 espèces et variétés.

Le concept générique de *Penicillium* supporté par Thom puis Raper et Thom voulant, en opposition avec l'art. 59 du Code de nomenclature, que seul le nom le plus ancien, qu'il soit anamorphique ou téléomorphique, couvre l'entièreté du champignon (l'holomorphe)(concept botanique des genres, voir Hennebert 1971), suscita d'autant plus d'opposition que le nombre de découvertes de *Penicillium* à fructification sexuée allait grandissant. Une revision s'imposait donc.

Pitt l'entreprit et la publie. Sa revision est caractérisée par trois traits: (1) le désir de conformité avec le Code de Nomenclature, du moins dans l'esprit, si ce n'est dans la lettre, (2) l'acceptation de 93 espèces des 141 espèces de Raper et Thom et l'addition de 57 autres espèces pour la plupart récentes, (3) l'emphase donnée sur la croissance en milieux de culture nouveaux et à différentes températures, sans s'attacher beaucoup plus que Raper et Thom à la morphologie.

Les 150 espèces reconnues par Pitt comprennent 37 *Eupenicillium* et 16 *Talaromyces* ayant une forme *Penicillium* pour anamorphe et 97 espèces anamorphiques de *Penicillium*.

Des 37 espèces incluses dans le genre *Eupenicillium*, 12 répondent à des taxa décrits dans Raper et Thom (1949), les 25 autres étant plus récentes. Du genre *Talaromyces*, l'auteur reprend 16 espèces à forme conidienne *Penicillium* (les espèces à anamorphe *Merimbla* et *Geosmithia* n'étant pas reprises), dont 9 espèces de Raper et Thom et 7 postérieures à 1949. C'est dire les progrès de ces dernières décades.

Le genre anamorphique conidien *Penicillium* Link ex S.F. Gray est divisé en 4 sous-genres, puis en sections et en séries. Le sous-genre *Aspergilloides* (25 espèces) ne comprend que les monoverticillés stricts à l'exclusion des *Ramigena* de Raper et Thom, soit 18 espèces de ces auteurs et 7 postérieures. Le sous-genre *Furcatum* Pitt regroupe les biverticillés, c'est-à-dire les *Ramigena* et les *Divaricata* de Raper et Thom ainsi que *Penicillium citrinum* qui dans les *Velutina* n'occupait qu'une place de transition. Des 27 espèces incluses, 5 seulement sont postérieures à 1949. Le sous-genre suivant, *Penicillium* sbg. *Penicillium*, regroupe les espèces triverticillées (ou bivertillées asymétiques) et comprend 22 espèces, dont 18 appartenaient aux *Velutina, Lanata, Funiculosa et Fasciculata* de Raper et Thom (des *Lanata et Funiculosa*, beaucoup d'espèces étant rendues synonymes aux autres). Ce sous-genre inclut *Penicillium expansum* lectotype proposé du genre. Le sous-genre *Biverticillium* Dierckx (23 espèces) regroupe 13 espèces des *Biverticillata symetrica* de Raper et Thom, 2 espèces réhabilitées de Dierckx et 8 espèces récentes.

On notera que les formes *Penicillium* des espèces d'*Eupenicillium* et *Talaromyces* ne sont pas reprises dans la classification générale des *Penicillium*, ni même mentionnées (à l'exception de trois, p.166-169) dans les clés dichotomiques des espèces. C'est regrettable d'autant plus que l'auteur voit dans la séparation des nomenclatures anamorphique et téléomorphique la possibilité d'aborder l'espèce par une seule de ses formes et ainsi arriver à son identification précise. Il est d'autre part curieux de constater que le traitement de *Eupenicillium* et celui de *Talaromyces* (dont l'un précède celui de *Penicillium* et l'autre le suit, on ne sait pourquoi) sont accompagnées de clés synoptiques chiffrées selon le modèle de Leenhouts, alors que le genre *Penicillium* est introduit par des clés dichotomiques. Pourquoi cette différence? Des clés synoptiques pour les *Penicillium* auraient beaucoup aidé à l'identification rapide.

Sur le plan de la nomenclature, Pitt s'efforce de suivre le Code International de Nomenclature Botanique, ainsi fait-il, contrairement à Raper et Thom, une claire distinction entre la nomenclature des téléomorphes et des anamorphes. Cependant il s'en écarte, sans doute à bon droit, si ce n'était prématurément. En effet il suit la nomenclature qui devrait résulter de la conservation du genre *Penicillium* Link ex Gray (lectotype *P. expansum* Link ex Gray) contre *Penicillium* Fr. non Link (holotype *Mucor crustaceus* L.) selon la proposition 420 de Hawksworth, Pitt and Sutton in Taxon, 25:665-670, 1976. De plus il met déjà en application les propositions d'amendement de l'art. 59 par le Secrétariat de Nomenclature de l'Association Mycologique Internationale (Taxon 28: 424, 1979). Ainsi il recombine dans les genres téléomorphiques appropriés les 17 espèces décrites avec téléomorphe et classées par Raper et Thom dans le genre *Penicillium*. Cette prise de position hardie peut sans doute démontrer le bien-fondé et le bon fonctionnement de ces propositions.

Il n'en reste pas moins que la revision de Pitt exigera du taxonomiste habitué à la monographie de Raper et Thom un effort de réadaptation En plus des changements nomenclaturaux des espèces retenues, l'identificateur devra se convaincre de la synonymie des espèces rejetées. Un index donnant la redisposition des espèces de Raper et Thom pourra un peu l'aider, mais cet index est sans pagination. De plus, l'auteur a modifié les milieux de culture standards utilisés par Raper et Thom, y ajoute un troisième milieu et recommande leur usage à trois températures différentes. Cette exigence ne facilitera pas le passage d'une monographie à l'autre bien qu'elle soit acceptable.

L'auteur semble en effet mettre l'attention sur les caractères culturaux, en particulier la vitesse de croissance à différentes températures. On eut sans doute souhaité un approfondissement de l'étude morphologique et de la variabilité des espèces. Les descriptions morphologiques sont succintes. Les photographies, en microscopie interférentielle, excellentes pour la plupart, suppléeront, espérons-le, à la pauvreté des dessins.

Le livre est fort bien édité, comme il se doit. Cependant quelques imprécisions ou erreurs sont passées inapercues. Le Professeur Biourge est considéré le "student" de son élève Fr. Dierckx. Ceci est d'autant plus contraire à l'histoire que la plupart des souches étudiées par Dierckx lui avait été données pour étude par Biourge lui-même. Dierckx a élaboré son *Essai de revison du genre Penicillium* à la fois sous la direction et l'inspiration de Biourge qui s'y était attelé déjà depuis 1898. Je ne mentionnerai encore qu'une autre erreur, dans la diagnose du sous-genre *Furcatum* p. 233 où les "metularum" doivent être des "phialidum" pour que ce sous-genre regroupe des biverticillés.

Dans l'identification de champignons aussi répandus et aussi importants que les *Penicillium*, on est heureux d'accueillir l'ouvrage de Pitt, fruit de 10 années de travail difficile, louable effort vers une taxonomie et une nomenclature plus sûre de ces champignons.

J. Walker, Search 12: 183-184, 1981

The Genus *Penicillium* and its Teleomorphic States *Eupenicillium* and *Talaromyces*

J. I. PITT

1979 634 pages $109.50
(London: Academic Press)

For the past thirty years, workers with fungi have relied on the monumental work of Raper and Thom for information on the taxonomy, identification and biology of species in the ubiquitous genus *Penicillium*. The present work by Dr John Pitt represents another major landmark in the classification of species of *Penicillium*, and more especially on their teleomorphic states. It incorporates much original work by many workers since Raper and Thom's Manual was published in 1949. published in 1949.

The book is divided into fourteen chapters, plus an extensive bibliography, glossary and index. The first four chapters give details of the history of the genus, the teleomorphic states proposed for the species of *Penicillium* and their growth in culture. The nomenclatural difficulties associated with the naming of the *Penicillium* anamorphic (imperfect) states and their corresponding teleomorphic (perfect) states are summarised. Chapter 5 is a detailed discussion of the characters used in classifying and identifying species. As in the Raper and Thom Manual, the detailed morphology of the penicilli plays a major part in classifying subgenera and groups of species, but Dr Pitt also places much more emphasis on growth and cultural data than previous workers. For some subgenera, penicillus type appears to be correlated with seven-day growth on special media, such as 25% glycerol nitrate agar, and a table showing this relationship is included. Chapters 6 to 12 inclusive are devoted to the taxonomy of the anamorphic (*Penicillium*) and teleomorphic (*Eupenicillium* and *Talaromyces*) states. Clear detailed descriptions are given and most species are illustrated with good black and white photographs of Petri dish cultures and Nomarski interference contrast photos and/or line drawings of conidiophores and comdia. Keys to the subgenera, sections and species are provided. In chapter 13, entitled 'Polyglotta', we are told what to do when the keys fail, there is a table summarising the colony diameter for all the species after seven days growth on various media at a range of temperatures, and notes on mycotoxins, specific habitats for some species and other ecological data are provided.

Chapter 14 is a valuable summary, with a series of lists showing (*i*) the names of all the species accepted in the present work; (*ii*) the names of all the species accepted in the Raper and Thom (1949) Manual and how the present author disposes them; (*iii*) all the specific epithets applied to species of *Eupenicillium*, *Talaromyces* and *Penicillium* and the author's comments on them; (*iv*) indeterminate and excluded names and (*v*) the names of the 22 new species of *Penicillium* and four of *Talaromyces* described in the book. Some idea of the changes that have been made may be gained from the following figures — of the 141 names of species and varieties accepted by Raper and Thom, only 63 are accepted unchanged by Dr Pitt, and there is a total of 203 accepted names in contrast to Raper and Thom's 141.

The many keys throughout the book provide ready access to the characters used in identifying species. It is a pity that all the conidial states of *Eupenicillium* spp were not included in the keys to *Penicillium* spp. The author mentions on pp 520-521 that this had not been done, but the present reviewer spent some time puzzling over the omissions before he came across this statement. Some more detailed discussion of the criteria used in splitting off the eight series of species in the genus *Eupenicillium* would have been welcome. It is also felt that, with some of the synonymies listed, some nomenclatural problems may arise, especially regarding priority. However, these are minor criticisms of a book where the wealth of information and very full coverage given provide an essential reference for all whose work brings them into contact with this widespread and important group of fungi.

JOHN WALKER
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Biological & Chemical Research Institute,
NSW Department of Agriculture

MYCOLOGIA
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Editor-in-Chief
Department of Botany
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Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

30 December 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge LA 70803

Dear Bernard:

There is an unexpected lull in the manuscript-handling department, and I am able to look ahead to matters that the Editorial Board will have to consider. One of these is the termination (at the end of 1982) of your appointment to the Board. Early in the coming spring, I will have to canvass the Board for suggestions.

I am writing at this time to ask if you would be willing to serve for an additional five year term if voted on by the Board? I realize that the task is sometimes a burden, but the Board does need on it someone with your expertise. I have no difficulty finding reviewers for the larger groups of Basidiomycetes, but there are very few who can help judge papers on the more unusual forms and taxa. You have served admirably in this capacity.

If you would be willing to be considered for an additional term appointment, please let me know. If not (and I would understand why you might decline), perhaps you can suggest names of persons whom you would recommend to replace you.

Kindest regards.

Sincerely yours,

Terry

T.W. Johnson, Jr.

TWJ:jd

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

5-I-1982

Dr. T.W.Johnson, Jr.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Terry:

My situation here at the university is somewhat unusual. No sooner had I retired, when the Dean of our College made available to me more space than I have had for the past decade, and I have moved the mycological herbarium to a new facility which is a great improvement over what I formerly had. Everyone here seems to take it for granted that I shall continue working in the Department as before, except for classes, and in fact that is what I intend to do. Since the facilities are available, I can see no reason for hibernating or for avoiding certain small academic duties that I have been performing for some years. As long as you believe I can be useful I would gladly accept an appointment for another term on the Editorial Board, should the Board so recommend. However, I should remind you that in the past I have absented myself from the campus for 6-8 weeks (or more, occasionally) each year in order to pursue mycological or ethnomycological projects in the Americas. I plan to continue doing this in future, but if I am reappointed to the Board, I shall notify you in advance, whenever possible, of upcoming peregrinations. I would like to return this summer to Guatemala for more ethnomycological work, but because of the unstable political situation there it may be unsafe for a foreigner (or anyone else, for that matter) to go wandering alone through the countryside. Anyway, I'll keep you informed.

as ever,

Bernard

B. Lowy

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
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2-I-1982

Dr. T.W.Johnson, Jr.
Department of Botany
Duke University
Durham, NC 27706

Dear T[illegible]y:

Perhaps it is because I have been reading Frazer's "Golden Bough," and other little treasures of mythology lately, but I believe that adopting Dr Wang's modest proposal would open Pandora's Box. Not that mankind does not richly deserve punishment for a multitude of crimes against both humanity and the gods, but to willingly embrace a scheme that would result in a labour of Sisyphus is incomprehensible.

All of Chris Davison's points are valid. It also appears that Dr. Wang might savor becoming Co-Editor of her part of the journal. If this precedent is established, what is to prevent a proliferation of serpents such as: Distributional Records of Phycomycetes, Ascomycetes, Basidiomycetes, etceteramycetes ? And these too would require separate editors. A journal within a journal. It would uselessly inflate MYCOLOGIA, inflict migraine upon the editor, and ultimately drive away legitimate contributors.

My suggestion is that Dr. Wang consider starting a new journal, possibly entitled DISTRIBUTIONAL RECORDS OF FUNGI, dedicated solely to this end.

Yours sincerely,

Bernard

B. Lowy

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

28 December 1981

Memo: Proposal for publication of distributional records

To: Editorial Board

As I was directed to do at the Editorial Board meeting in August, I explored the feasibility of including in MYCOLOGIA a section on "Distributional Records of North American Deuteromycetes." This proposal, by Dr. June Wang (letter enclosed), was sent to you in my report (August) of the Board meeting.

It has taken some time to get responses to my queries about such a published section. Recently, I heard from the editor of MADRONO, and his letter sums up quite adequately the general response I have had from others. I enclose a copy of his letters to me. Dr. Wang has contacted me again, and is anxious to have her proposal put into action. She is unwilling to ask the editors of MYCOTAXON to consider her proposal since she feels it is not an official publication and does not have the circulation of MYCOLOGIA.

You can see from Dr. Wang's first letter to me that the proposed format is relatively simple, and space for one entry would be very little (if 8 point type were used). There are a number of editorial problems associated with the plan: would all records submitted be published? Would authors receive proof? Reprints? What volume of submission could be expected?

At this time, Dr. Wang is working on a major monograph of the Imperfects.

Please read the enclosures, and let me know whether Dr. Wang's proposal should be implemented or declined. If the latter, reasons for your decision would be most helpful to me. I enclose a stamped envelope for your convenience.

Thank you.

Terry

T.W. Johnson, Jr.

TWJ:jd

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Department of Environmental and Forest Biology

August 5, 1981

Dr. Terry W. Johnson, Jr.
Editor-in-Chief
Department of Botany
Duke University
Durham, NC 17706

Dear Dr. Johnson:

This is to request that a new section on Distributional Records of North American Deuteromycetes be instituted in MYCOLOGIA to promote a better account of distribution of fungi in North America.

At the Symposium on Fungi Imperfecti in Tampa, 1977, I stated that distributional data on Deuteromycetes in North America was fragmentary. One of the reasons is failure to report. While a specimen or culture may be new for a state, a region, or North America, it is often not published for the following reasons: (1) there is no convenient place in a mycological journal to report a new collection or isolation, and (2) it is part of an extensive work which often requires many years for completion. For example, Zygosporium masonii Hughes has been reported only twice, in West Virginia and Hawaii, but it is my belief that the fungus is more common than the published records suggest. After the Symposium, two persons told me they have isolated the fungus from soil in California and Texas.

To improve our system of recording distribution of Deuteromycetes in North America, I suggest that once or twice a year, a Distributional Records of Deuteromycetes be included in MYCOLOGIA following Brief Articles. The following information should be provided:

Fungus Name: _____

Class (according to Dictionary of Fungi, 6th Ed. p. ix):

_____

Substrate: _____

Locality: _____

Date : _____

Collected by: _____

Specimen or Culture Number: _____

Identified by: _____

Verified by (if applicable): _____

Specimens and/or cultures, semipermanent slides deposited at:

_____

The last three items are essential to provide opportunity for interested mycologists to examine the specimen or culture. They will also safeguard proper identification of the fungus. The record cannot be published unless the last item is furnished.

I realize this may give additional work to the editor, however, if this proposal is approved, I am willing to assist in editing it. If this system proves to be worthwhile, it can be extended to include other taxa. I would greatly appreciate your bringing this up for discussion at the MSA Council meeting.

Best regards.

Sincerely yours,

C. J. K. Wang
Professor

rkc

cc: Dr. Marie L. Farr, President
Dr. Margaret B. Bigelow, President-Elect
Dr. Harry D. Thiers, Vice-President
Dr. Roger Goos, Secretary-Treasurer

Madroño

Quarterly Journal of the California Botanical Society

Christopher Davidson, Editor
Idaho Botanical Garden
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Boise, ID 83701

(208) 343-8649

17 Dec. 1981

Dr T. W. Johnson, Jr.
Botany Dept.
Duke University
Durham, NC 27706

Dear Dr. Johnson,

My overall impression of our Noteworthy Collection section is that it is more trouble than it is worth. We receive many submissions for this section, up to 30 %, and they require nearly as much work as regular research papers, although I have been somewhat lax in having some of the later entries reviewed by other than myself. Also, I find that the authors almost always are the only ones that give me much trouble. They are convinced that some of the most insignificant things are worth publishing. At $40.00 a page we have reconsidered our policy toward them and decided to prune them back. I enclose a xerox of "before and after." It is true that new species need to be published as a formal requirement of our field; no editorial decision is required in this case, but how does one judge what is a noteworthy new collection. There are hundreds of such new distribution records made each year. Sometimes one need only walk ten m across a county line. Surely this is true also among the Deuteromycetes and other fungus groups. This kind of information really belongs in a flora, whereas new species are supposed to be published separately from a flora.

These are a few of my thoughts about our NC section. Concerning your questions, you are right we do not publish every submission. We would be inundated in a few weeks. All submissions are reviewed, either by me or by someone I think knows about the plant involved or who has worked in the area it has been reported from. This is time-consuming and, I think really wastes the time of a reviewer unless the collection is startling. Very few are startling. I have a few criteria to help me decide what is noteworthy: 1. New state record. 2. New county record. 3. Record of a distinctly different habitat type. 4. Rare or endangered plant. I have had to reconsider some of these categories, too, because the California Native Plant Society publishes records of rare plants, thus eliminating any obligation on our part. Given my new format, the other records may be acceptable. My solution for the time being is to compromise the wishes of several of our Society Council members to do away with NCs altogether. Submissions are likely to fall off dramatically, particularly among the "vanity paper" people who send these things in just to see their names in print, and those that do come in will perhaps be more noteworthy. We shall see.

Your request for information was no trouble. No need to apologize. If you need more help, do not hesitate to write.

Sincerely,

Chris Davidson

Many thanks, Bernard, for all your help in editing this year; I am grateful.

May you have a very Merry Christmas, and the happiest of New Years.

Terry

T. W. JOHNSON, JR.
DEPARTMENT OF BOTANY
DUKE UNIVERSITY
DURHAM, NC 27706

XII-1981

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
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17-XI-1981

Dr. T.W.Johnson, Jr.
Department of Botany
Duke University
Durham, NC 27706

Dear Terry:

The manuscript by Punja et al. arrived at a critical moment, and I am sorry that I must immediately return it to you. I am in the uncomfortable process of moving from my present location to a new building, and the herbarium plus every stick of furniture in my office and lab goes with me. My library of several thousand books is now in boxes, ready for the move which begins tomorrow morning! It has taken me 5 days to pack all the major items, and some odds and ends still remain. After moving the unpacking will begin. I think this is enough to give you the picture. What I am building up to is that for the first time since I have been on the editorial board I must with regret return a paper unreviewed, because of the circumstances I have outlined. Another factor that makes this advisable is that on Nov. 24 I plan to leave for about 2 weeks to visit various members of my family in Atlanta, Boston, and New York, and expect to return on or about Dec. 8. After Dec. 15 I should again be ready to receive manuscripts from you. I know that you will understand my predicament.

With best regards,

Bernard

B. Lowy

P.S. My address remains unchanged.

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The New York Botanical Garden
Bronx, New York 10458

28 October 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernard:

Would you be willing to review the enclosed manuscript for MYCOLOGIA?

"Tremelloscypha gelatinosa, a species of Sebacinaceae" by K. Wells and F. Oberwinkler.

If so, please prepare two copies of your comments, one an unsigned copy that can be sent to the author (reviewers who prefer not to remain anonymous may sign both copies of their appraisal). Minor corrections and suggestions may be placed directly on the manuscript using a soft lead pencil.

The following points should be considered in your review:

1. Is the material new and worthy of publication in MYCOLOGIA?

2. Is the presentation clear and logical?

3. Are the conclusions justified by the evidence?

4. Could the paper be shortened substantially without interfering with its merit?

5. Have the tables been prepared as clearly and concisely as possible? Could any be deleted? Added?

6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated. Kindly return the paper with your comments promptly.

Should you anticipate not being able to review this paper within two or three weeks, of if you prefer not to review it at all, please return the manuscript to me without delay so that it can be sent to another reviewer.

Thank you for your assistance.

Sincerely yours,

Terry

T.W. Johnson, Jr.

received: 2-XI-'81

returned: 3-XI-'81

Comment on "Tremelloscypha gelatinosa ....."

by K. Wells and F. Oberwinkler

This carefully documented and well illustrated paper presents a further cogent argument for emphasizing micromorphology as opposed to the exclusive use of gross basidiocarp morphology in the classificatation of tremellaceous fungi. The present questionable status of Efibulobasidium which is "tentatively assigned to the Sebacinaceae" indicates the need for further morphological (and physiological) studies of this and related genera. The authors have made a very useful contribution in this direction with their observations on Tremelloscypha.

B. Lowy

ABSTRACT

Eichleriella gelatinosa Murrill, known only from the type locality in Jamaica and from Florida, is transferred to *Tremelloscypha* on the basis of its coriaceous, flabellate to infundibulifrom basidiocarps, the relatively thick-walled, brownish hyphae lacking clamps in the trama, and the ovate to pyriform basidia lacking basal clamps. A new family, Sebacinaceae, is proposed to include *Tremelloscypha*, *Sebacina s. str.*, and *Tremellodendron*. It is suggested that *Efibulobasidium* be tentatively assigned to the Sebacinaceae. This arrangement brings together several taxa that possess similar basidial and hyphal characters but whose basidiocarps vary from pustulate or incrusting to erect and branching or flabellate through infundibuliform.

Murrill (apud Burt, 1915) described Eichleriella gelatinosa Murrill on the basis of two collections, one of which was sterile, from Jamaica. Either Murrill or Burt noted on the packet of the collection designated as the type, evidently prior to examining the specimen microscopically, that the collection was either a representative of the genus Stereum or of the genus Cladoderris. Subsequently, Wells (1961) and Wells and Raitviir (1980) noted that E. gelatinosa was to be excluded from Exidiopsis and Eichleriella because of the lack of clamp connections and the infundibuliform shape of the basidiocarps of E. gelatinosa.

Recently Reid (1979) proposed a new genus, Tremelloscypha, including a single species, T. australiensis Reid, based on several collections from Australia. After examining the type collection of T. australiensis, we have concluded that it is congeneric with Eichleriella gelatinosa and, accordingly propose the following new combination and emended description.

Tremelloscypha gelatinosa (Murrill) F. Oberwinkler et K. Wells, comb. nov.

Figs. 1, 2.

Eichleriella gelatinosa Murrill apud Burt, Ann. Mo. Bot. Gard. 2: 748. 1915.

Hirneolina gelatinosa (Murrill) Sacc. et Trott. apud Sacc., Syll. Fung. 23: 575. 1925.

Basidiocarps coriaceous, spongy, 3.5-7.5 X 2-5.5 cm, 0.1-0.6 cm in thickness, flabellate, infundibuliform, or pseudoinfundifuliform, with poorly defined stipe; hymenial surface inferior, decurrent, smooth, generally continuous, when dried cartilaginous in appearance, ochraceous-tawney to tawney with vinaceous tint [Cinnamon-Buff, Bone Brown to Seal Brown (Ridgway, 1912)] in dried specimens, margins somewhat lighter; stipe irregular, poorly defined to almost absent with the hymenium extending almost to the substrate in some specimens, usually attenuate or more rarely somewhat bulbous, hyphae of stipe without clamps, becoming thick-walled, 2-5.5 µm in diam, in fascicles or single, essentially parallel to the

surface; abhymenial surface light buff [Capucine Buff to Pale Yellow-Orange (Ridgway, 1912)], strigose, somewhat zonate, especially near the upper margin, spongy, hyphae in interwoven fascicles of varying sizes or single, parallel to the surface, faintly brownish, becoming thick-walled, 2-6.5 µm, walls up to 1 µm in thickness, usually agglutinate, collapsing, septate, without clamps; margins blunt, somewhat lighter in color than abhymenial surface, somewhat recurved, hyphae also in fascicles, radially oriented and closely packed, similar to the hyphae of the trama and abhymenial surface; growth is evidently marginal; trama composed of ascending, interwoven hyphae, mainly in loose fascicles, terminating at the hymenial surface in interwoven hyphal layer that gives rise to the fertile hyphae and dikaryophyses, in some regions there is a dense layer of hyphae parallel to the surface adjacent to the trama that forms externally a palisade-like layer beneath the subhymenium possibly representing a first formed hymenial layer, hyphae in trama 3-5 µm in diam, often in fascicles, often agglutinate, thin- to thick-walled, walls up to 1 µm, septate, without clamps; hymenium of dikaryophyses and basidia; dikaryophyses somewhat indistinct and collapsed, abundant, sparsely branched or unbranched, 2-3.5-5 µm in diam; fertile hyphae without clamps, proliferating laterally near apex of sub-basidial hyphal segment, 2.5-5 µm; basidia oval, ovate, obovate to clavate, rarely subglobose, without basal clamps, developing 2-4 hypobasidial segments, 14.5-18-20(-23) X 10-13.5-16 µm, epibasidia tubular, usually flexuous, up to 35 µm in length, 2-5 µm in diam; basidiospores elliptical or ovate and laterally depressed to short cylindrical-curved, 9-13.5(-14.5) X 5.5-8(-9.5) µm; germination not observed.

On decaying wood. Known only from Jamaica and Florida.

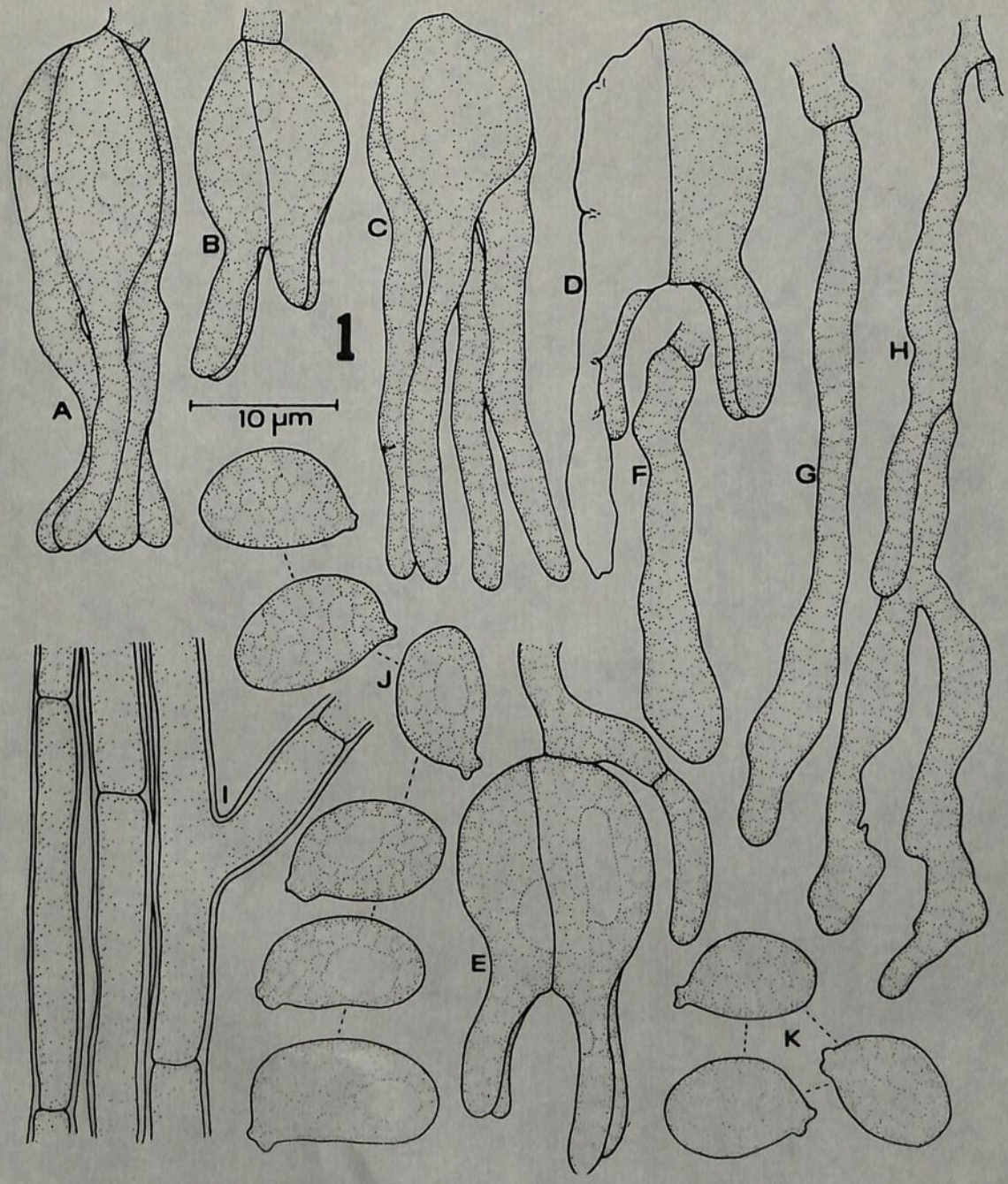
Type locality.--Jamaica.

Illustrations.--Murrill, W. A. apud E. A. Burt, 1915. Ann. Mo. Bot. Gard. 2: 771, Fig. 12.

LEGENDS TO FIGURES

Fig. 1. Tremelloscypha gelatinosa. A-E. Basidia. F-H. Dikaryophyses. I. Hyphae from trama. J,K. Basidiospores. A, B, F, G, and K from Fungi of Jamaica 1087 (holotype). C, D, E, H, I, and J from NO 01322. All drawings made with the aid of a Zeiss Drawing Apparatus.

Fig. 2. Sebacinaceae. A. Tremelloscypha australiensis. Habit sketch of a basodiocarp from the type collection, X 5. B. T. australiensis. Portion of the hymenium including basidiospores. C. Tremelloscypha gelatinosa. Portion of the hymenium. D. Sebacina incrustans. Cross section of a portion of a basidiocarp showing the trama with thick-walled hyphae and hymenium with basidiospores. E. Tremelloscypha gelatinosa. Left, a habit sketch of TU 01322; right, longitudinal section of an infundibuliform basidiocarp, X 9. F. Tremellodendron candidum. Habit sketch of a basidiocarp, X 0.8. G. Tremelloscypha gelatinosa. Hyphae from trama, X 875. H. Tremellodendron candidum. Hyphae from trama. I. T. candidum. Portion of hymenium and subhymenium including two basidiospores.


A
B
C
D
F
G
H
1
10 µm
I
J
E
K

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

21-IX-1981

Dear Terry,

When I sent you the page proofs of the Syzygiopsora paper, I think I forgot to request that the original drawings (Obermüller's) and photos be returned after the printer is through with them. If it is not too late, may I ask that this be done?

I regret this lapse, which is the sort of thing that makes editors prematurely grey!

Yours,

Bernard

Mycologia

MYCOLOGIA

Instructions for Proofs and Page Charges

Proofs: Please read the enclosed proof carefully, making only absolutely necessary changes, and return promptly. Author's will be charged for alterations where warranted. Mark on the margin of the proof where figures, tables, etc., if any, should be inserted. Also, indicate clearly if there are places where additions (page numbers, incomplete references, etc.) will have to be made by the editor in page proof. Do not return your artwork unless it is necessary to remake an engraving.

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I have substituted the corrected page 1 sent later.

Cordially,

Richard P. Korf, **Managing Editor**

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AN INTERNATIONAL JOURNAL DESIGNED TO EXPEDITE PUBLICATION OF RESEARCH ON TAXONOMY & NOMENCLATURE OF FUNGI & LICHENS

P.O. BOX 264, ITHACA, N.Y. 14850, U.S.A.

28 April 1981

Dr. B. Lowy
Botany Department
Louisiana State University
Baton Rouge, LA 70803

Dear Bernie,

This will acknowledge safe receipt of and **acceptance for publication** of your 4 page paper: A new species of Dacryopinax from Brazil

It will appear in MYCOTAXON 13(2) scheduled for publication about 1 Aug 81

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I have substituted the corrected page 1 sent later.

Cordially,

Richard P. Korf, **Managing Editor**

The Mycological Society of America

FOUNDED DECEMBER, 1931

Office of the Secretary-Treasurer
Department of Botany
University of Rhode Island
Kingston, RI 02881
Telephone: (401) 792-2161

August 28, 1981

Dr. Bernard Lowy
Mycology Herbarium,
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Dr. Lowy:

On behalf of the Mycological Society of America, I am very pleased to inform you that you have been elected to Emeritus status in the Society. Your election entitles you to all the privileges of full membership without the payment of dues. However, if you wish to continue to receive MYCOLOGIA, you must subscribe to it at the current student rate ($12.00).

May I congratulate you upon your election to Emeritus status, and particularly, upon the active professional life which necessarily preceded such election. I hope that you will enjoy many years of fruitful retirement and that you will continue to take an active interest in our Society.

Sincerely yours,

Roger

Roger D. Goos
Secretary-Treasurer

RDG:lbt

MYCOLOGIA—EDITORIAL OFFICE

August 19,1981

Dear Dr. Lowy,

Your review of the paper "Atractogloea a new genus..." was received today. The editor will acknowledge the review when he returns from the AIBS Meetings.

Sincerely yours,

Marjorie Watkins

Marjorie S. Watkins

T.W. JOHNSON, JR.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

MYCOLOGIA
OFFICIAL JOURNAL OF THE
MYCOLOGICAL SOCIETY OF AMERICA
PUBLISHED BY
THE NEW YORK BOTANICAL GARDEN

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

21 August 1981

Memo: Report of August (1981) meeting of Editorial Board

To: Editorial Board, MYCOLOGIA

The Editorial Board met on Wednesday, August 19, 1981, at Indiana University. Drs. Shearer, Aldrich, Collins, and Rogerson attended.

Drs. Darrell Weber and David McLaughlin were elected to the Board to serve five year terms. (Dr. Weber has accepted; an invitation has been sent to Dr. McLaughlin). The Council approved the alternates nominated by the Board.

It was suggested that the Editor send to each reviewer a copy of the letter written to each author when a paper is accepted or rejected. This plan was not adopted, but the editor may send such copies if the reviewer requests it. Since the Editorial Board is a major "reviewer", members should inform the Editor if they wish to receive such copies after papers have been reviewed.

The Board agreed to allow the symposium papers (Indiana meeting) to be published. However, the symposium organizer must initiate and coordinate assembly of the manuscripts, and the papers must be reviewed, as is customary, before a decision is made on acceptance. When all papers were accepted, they could be published in a single issue, adequately identified as a symposium paper. (I am in the process of contacting all symposium coordinators.)

The Editor was directed to explore the implications of publishing periodically a section in MYCOLOGIA: "Distributional Records of North American Deuteromycetes." This proposal, from a member of the Society, was discussed. Two comments prevailed. Some Board members thought that such a section would be more appropriate to MYCOTAXON. The Board agreed that if the proposal proved feasible, the section could not be limited only to imperfects, but would include all groups. This could prove costly in space, and in efforts to assemble the information. The Editor will explore with the Editor of MADRONO the extent and implications of such a listing; this journal periodically lists new records.

The "Instructions to Authors" will be amended to include an invitation to authors to suggest reviewers for their papers.

Terry

T.W. Johnson, Jr.

TWJ:jd

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

10 August 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernard:

Would you be willing to review the enclosed manuscript for MYCOLOGIA?

"Atractogloea: a new genus in the Actractiellaceae (Heterobasidiomycetes)"
by F. Oberwinkler and R. Bandoni

If so, please prepare two copies of your comments, one an unsigned copy that can be sent to the author (reviewers who prefer not to remain anonymous may sign both copies of their appraisal). Minor corrections and suggestions may be placed directly on the manuscript using a soft lead pencil.

The following points should be considered in your review:

1. Is the material new and worthy of publication in MYCOLOGIA?

2. Is the presentation clear and logical?

3. Are the conclusions justified by the evidence?

4. Could the paper be shortened substantially without interfering with its merit?

5. Have the tables been prepared as clearly and concisely as possible? Could any be deleted? Added?

6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated. Kindly return the paper with your comments promptly.

Should you anticipate not being able to review this paper within two or three weeks, of if you prefer not to review it at all, please return the manuscript to me without delay so that it can be sent to another reviewer.

Thank you for your assistance.

Sincerely yours,

Terry

T.W. Johnson, Jr.

14-VIII-1981

"Atractogloea: a new genus ....." by F. Oberwinkler and R. Bandoni

This carefully researched, exceptionally well illustrated paper contributes much significant data to the study of Heterobasidiomycetes. It also alerts mycologists to the importance of perspicacious collectors!

B. Lowy

14-VIII-1981

Atractogloea: a new genus in the Atractiellaceae (Heterobasidiomycetes)

Part 14 in a series, "Studies in Heterobasidiomycetes" of the Institut für Biologie I, University of Tübingen and the Department of Botany, University of British Columbia.

F. Oberwinkler

Lehrstuhl Spezielle Botanik und Botanischer Garten, Universität Tübingen

Auf der Morgenstelle 1, D 7400 Tübingen 1, West Germany

and

R. Bandoni

Department of Botany, University of British Columbia, Vancouver

Canada V6T 2B1

Summary

Atractogloea stillata Oberw. & Bandoni gen. et sp. nov. is described and illustrated. Morphological features of the taxon, including ultrastructural details, are similar to those of other species in the Atractiellaceae. However, Atractogloea has pulvinate fructifications and clamped hyphae; other genera of the family have stilboid fructifications and lack clamps.

Typus generis: Atractogloea stillata Oberw. & Bandoni, descripta in opere ipso, pagina 0000.

Etymologia: ἄτρακτος - spindle, arrow; referring to the closely related genus, Atractiella; γλοῖος - oily, slimy substance.

Atractogloea is a heterobasidiomycete (Atractiellaceae, Oberwinkler and Bandoni, in press), with auricularioid basidia and sessile, drop-like, gelatinous basidiocarps. The hyphae are hyaline, slightly gelatinized, clamped, and have simple pores (Figs. 5-8); they produce terminal basidia but no cystidia or hyphidia. The cylindric, transversely-septate basidia develop sessile spores terminally and laterally; the basidiospores are not forcibly discharged. The basidiospores are hyaline, thin-walled, the walls smooth and non-amyloid; they germinate by budding.

The genus Atractogloea contains a single species, A. stillata, as follows:

Atractogloea stillata Oberwinkler & Bandoni, sp. nov.

Fructificatio minuta, stillata, gelatinosa, hyalina vel pallide alutacea, usque ad 1.0 mm, raro ad 1.5 mm longa. Hyphae hyalinae, fibulatae, (1.5-)2-3(-5) um in diam., frequenter anastomosantes, quamquam subgelatinosae distinctae sunt. Cystidia desunt. Basidia longi-cylindracea, 3.5-4.5(-6) X 40-100 um, mature transverse septata. Basidiosporae sedentes, 3-4 X 5-8 um, hyalinae, tenui-tunicatae, tunicis levibus, non amyloideis, non eiectae sunt cellulasque singulas

basidiosporarum simillimas producunt.

Hab: Fungus in spatha emortua Phoenicis canariensis cultae in California (Stanford) collectis occultus, inde crevit in substrato aequo aqua adsperso.

Typus: R. Bandoni no. 6662, in BPI.

Basidiocarps soft-gelatinous, sessile, pulvinate (Figs. 1, 2), mostly around 1 mm in diam., some slightly elongate and reaching 1.5 mm in the longest dimension. Hyphae (1.5-)2-3(-5) µm in diam., hyaline, slightly gelatinized but distinct, clamped (Figs. 3, 5, 6), two or three clamps frequently closely associated and anastomosed, branches typically arising from the clamp. Basidia terminal, long-cylindric, 3.5-4.5(-6) X 40-105 µm, becoming transversely 3-septate (Figs. 3, 4). Basidiospores (Figs. 3, 4) produced blastogenously, sessile, passively released when mature, 3-4 X (4-)5-8 µm, the walls thin, smooth, hyaline, non-amyloid, germinating by budding, the buds often developing while spores are still attached to the basidium.

Associated with numerous fungi on weathered spathe of Phoenix canariensis Chabaud, Stanford, California, December 27, 1980.

Discussion

The Atractiellales is a small order of gasteroid heterobasidiomycetes with phragmobasidia or holobasidia (Oberwinkler and Bandoni, in press). With the exception of Atractogloea all previously described genera in the order have minute, synemata-like basidiocarps. Atractogloea also differs

from Atractiella and Agaricostilbum, the only other genera in the Atractiellaceae, in having clamps and in lacking sterile paraphysis-like structures (hyphidia). In both Atractiella and Agaricostilbum, hyphidia protrude between the basidia or are concentrated around the margin of the hymenium. The only other taxon in the Atractiellales with clamps is Phleogena, a genus differing from other members of the order in having basidia irregularly arranged within a peridium, pigmented hyphae and basidiospores, and a distinctive anamorph.

Small, pustulate, gelatinous basidiocarps occur in widely distributed genera of the Heterobasidiomycetes, e.g. Platygloea and Mycogloea in the Auriculiales, Stypella and Tremella in the Tremellales, Dacrymyces in the Dacrymycetales, and Dicellomyces in the Exobasidiales. Such basidiocarps are relatively rare in Homobasidiomycetes, although approached in early developmental stages of some resupinate basidiocarps. The simple structure suggests that the pustulate basidiocarp form is primitive, a possibility also supported by its predominantly heterobasidiomycetous distribution.

Another unusual feature of Atractogloea is the presence of simple pores (Figs. 5-8) in the septa of clamps. Simple septal pores, also present in Phleogena, are more common among taxa where clamps are lacking, e. g. the Uredinales and Septobasidiales. In Atractogloea, a swollen zone immediately surrounding the pore tapers abruptly toward the center. Similar pores have been illustrated by Sebald (1977) and Khan and Kimbrough (1980) in Eocronartium muscicola (Fries) Fitzp., an auricularioid parasite of moss gametophytes. Dystra (1974) also illustrated such a septal type

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

3 August 1981

ANNUAL REPORT

MYCOLOGIA, Volume 73

This report covers the period 1 July 1980 through 31 July 1981.

(1) Manuscripts

Number carry-over from 1979-80	56
Number received	215
Total	271

Disposition of manuscripts (total) -

Accepted and edited, or published	143
Rejected	80
Withdrawn	3
Pending final action (in hands of reviewers, or authors for revision)	45
	271

(2) Volume 73 is complete. Numbers 1-4 have appeared; No. 5 (September-October) proofread; No. 6 (November-December) manuscripts sent to printer in early July.

(3) Negotiations with Allen Press are complete. January-February issue, volume 74, 1982, is being assembled, with about two-thirds of the issue completed and marked for the printer.

(4) Appointments to the Editorial Board. Two appointments for five year terms are needed. The Editorial Board was mailed a list of eleven names. The following persons were nominated by the Board: Physiology - D. Weber, A. Jaworski (alternate); Ultrastructure - D. McLaughlin, C. Mims (1st alternate), M. Powell (2nd alternate), I. Ross (3rd alternate).

(5) Reviewers. Several changes were made in the roster of reviewers. Four were dropped from the list (retirement, unreliability were the chief reasons). Twelve were added, but there are still needs for competent, reliable reviewers, particularly in the areas of physiology/biochemistry, ultrastructure, and mycorrhizal associations.

(6) Change in cover format. Editorial Board was polled by mail for their opinion on the proposed (NYBG) change. I also contacted two previous editors for their views. Editorial Board: 8 favored, 3 opposed, 1 abstention. Editors: both opposed to change unless substantial cost saving or some other compelling reason.

For the Editorial Board:

T.W. Johnson, Jr.

T.W. Johnson, Jr.

TWJ:jd

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

7 July 1981

Dr. Jack Kiefer
Department of Statistics
University of California, Berkeley
Berkeley CA 94720

Dear Dr. Kiefer:

The last response is in from the Editorial Board regarding the publication of your paper "On taxonomy-acknowledgement and explanation." I am enclosing copies of letters from some of the Board members; these are not all the replies (all members did respond), but they clearly most expressively convey the feelings of the Board, pro and con.

The Board voted evenly on the question (six to six), leaving the matter squarely in my hands for a decision. As you know, I have from the beginning favored the NEWSLETTER as the proper route for your response article, and I have not changed my view even after having read again all the printed matter, our correspondence and reconsidered your arguments and those of the Board. I cannot in good conscience let the journal go any further as a medium for argument/rebuttal "letters", no matter what has taken place before my time. It still seems to me that the NEWSLETTER, with its wide circulations, is the appropriate vehicle, not only for your response, but also for the earlier one by Dr. Machol.

I am sorry that this reply is unfavorable to your request. However, I believe the question was treated fairly, and that this course of action is best for the journal. I am returning your manuscript, but I repeat my offer to endorse your paper should you choose to submit it to the NEWSLETTER editors.

Sincerely yours,

TWJ

T.W. Johnson, Jr.

TWJ:jd

MYCOLOGIA—EDITORIAL OFFICE

23 June 1981

Dear Bernard,

I have copy edited your manuscript on Syzygospora, and am writing to compliment you--and thank you--for such clean copy! It is always so much easier to copy edit a typescript that is neatly done and "clean" as yours was; I wish other authors were as thoughtful as you.

Many thanks. Kindest regards.

Sincerely yours,

Terry

T. W. Johnson, Jr.

MYCOLOGIA—EDITORIAL OFFICE

22 June 1981

Dear Bernard,

This will acknowledge receipt (today) of your revised paper "Syzygospora alba..." I am pleased to inform you that this paper is accepted for publication, and is scheduled for the November/December, 1981, issue. Proof will be mailed to you in late August. I will shortly copy edit the manuscript, and if queries come to mind, I shall contact you.

Kindest regards.

Sincerely yours,

Terry

T. W. Johnson, Jr.

22-VI-1981

Dear Terry,

I vote to approve the format change in the cover of MYCOLOGIA.

Bernard

B Lowy

MYCOLOGIA
OFFICIAL JOURNAL OF THE
MYCOLOGICAL SOCIETY OF AMERICA
PUBLISHED BY
THE NEW YORK BOTANICAL GARDEN

T.W. JOHNSON, JR.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

18 June 1981

Memo: Cover format, MYCOLOGIA

To: Editorial Board

The Publications Office of the New York Botanical Garden will propose to the Council of the Mycological Society (at the Indiana meetings, 16 August) a format change in the cover of MYCOLOGIA.

The Garden is attempting to have all of its serial Publications display the same cover format, and have chosen BRITTONIA as the model. This means that the cover for MYCOLOGIA would have the same type size, volume designation, and content style as that of BRITTONIA (copy enclosed). The journal would retain the designation of "Brief Articles" (this does not appear on the cover of BRITTONIA).

As this proposed change will be voted on at the Council meeting, it is essential that I have the view of the Editorial Board toward this change. Please let me know (return the enclosed postcard) how you wish your vote cast - for or against the change. I should like to have your vote promptly so that I may draft my report to the Council.

I should add that the Publications Office expected us to adopt the very same style of BRITTONIA for the individual papers (author's address, paper title, and citation as part of the abstract). As our "instructions to authors" had already been published, this change could not be made, and we will retain the present format for the title page of each article.

Thank you.

Terry

T.W. Johnson, Jr.

TWJ:jd

Brittonia 33(1)

January–March 1981

A journal of systematic botany published by The New York Botanical Garden

Issued 15 April 1981

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Bronx, New York 10458

MEMO: Changes in "Instructions to Authors"

TO: Editorial Board, MYCOLOGIA

In anticipation of the change to a new format for MYCOLOGIA, I have drafted a revised statement of "Instructions." Instructions to authors preparing papers for the 1982 volume will be included in the December (1980) issue of the NEWSLETTER. Copy will have to be sent to the editors in late October.

A copy of the proposed revised "Instructions" is enclosed, and I ask for any changes, additions, or deletions you may care to make. A portion of the current "Instructions" will be retained (see January/February 1980, issue of MYCOLOGIA); I have left out those portions and simply indicated their position by ellipsis. The first paragraph on the enclosed copy would only appear in the NEWSLETTER to introduce the reason for the changes. Some of the proposed changes reflect my experience with the journal's manuscripts thus far.

Thank you.

TWJ
24 September 1980

INSTRUCTIONS TO AUTHORS

Beginning with the volume for 1982, MYCOLOGIA will adopt an enlarged format single column; page size of BRITTONIA; plate size of AMERICAN JOURNAL OF BOTANY), and will be printed by Allen Press. Authors intending to submit manuscript for the 1982 volume should prepare their papers to conform to the new format. The revised "Instructions", reflecting the various changes, follow. Contact the Editor-in-Chief (919-684-3715) if there is any question as to whether a manuscript to be submitted would likely appear in the 1982 volume.

INSTRUCTIONS (Revised)

Publication in MYCOLOGIA...Expected to be a member. Papers appropriate for the journal are (1) regular articles reporting original research, (2) notes or brief articles reporting research or new techniques, and (3) invited papers.

Papers are limited...

Regular articles will be published...

Manuscripts. Authors should follow the suggestions in the latest edition of the CBE Style Manual, AIBS. Authors are encouraged to have one or more colleagues read and criticize the manuscript prior to submission.

Submit manuscripts to Editor-in-Chief (or any member of the Editorial Board) in triplicate (three copies by an electrostatic process, or an original on white bond paper plus two carbons or electrostatic copies), double-spaced throughout; manuscripts not spaced (including legends, tabular data, literature citations) will be returned to the author for retyping. Number all pages, and identify each page with author(s) name(s); leave at least a one inch margin all sides. Regular articles include the following items, in this order: title, author'(s) name(s), author'(s) address(es), summary, text (with desired headings), acknowledgements, literature cited, tables or figure legends.

Manuscripts will be reviewed...the final decision.

Titles...not abbreviate

Headings...from left margin.

Text. Refer to recent issues of MYCOLOGIA, and the CBE manual for matters of style, abbreviation, punctuation. Boldface type will not be used in citing literature.

Headings...from left margin. The summary is designated by this word, centered on the page; introductory matter is not identified by a heading.

Footnotes. Avoid using footnotes if at all possible. If used, number...the first page. Lengthy descriptions of tabular material should not be in footnotes, but incorporated into the text.

Scientific names. Underline only...in the text.

New taxa, keys, and formal descriptions. Place names of new taxa at left margin followed by author(s) and status (e.g., sp. nov.). Follow with brief but descriptive. Latin diagnosis (required for all new taxa except bacteria and fossils) in paragraph form. English description, in paragraph form, follows the Latin. Record measurements as...

Center title of key on page. Key must be dichotomous, the couplets numbered, and block indented. Leads of first couplet begin at left margin, as do those of third, fifth, etc. Leads of second (fourth, sixth, etc.) indented five spaces. Turned-over lines should be justified at left with the preceding line.

Illustrations. Designate all illustrations (photographs, graphs, line drawings) as figures (abbreviate: Fig., except at beginning of sentence), and number consecutively in Arabic numerals. Type figure caption on separate sheet of paper, in paragraph form. More than one caption may be included on one sheet. See recent issue of MYCOLOGIA for format.

Figures must be designed to fit a maximum of 5.6 inch (14 cm) width x 8.7 inch (22 cm) height, after reduction. Ordinarily the height limit include space for the figure legend.

Mount photographs on heavy illustration board, leaving some margins for editorial notations. Trim photographs for composite figures and mount them together without space between. Do not submit loose photographs or line drawings intended for composite figures; the editor will not arrange or mount cuts or photos. Trim carefully to crop and provide straight margins. Do not include photographs and line drawings in the same composite figure. Write author's name, figure number, and key title words on the back of each figure.

Number or letter figures by using a lettering instrument or printed graphic art aids. Sizes are indicated preferably by one or more bars drawn or affixed on the figure. If magnifications are used, any reduction to fit the 5.6 x 8.7 inch printed page space must be calculated. Write magnifications as x 1200, e.g.

Graphs or line drawings should be grouped as much as is consistent with ease of reading after reduction to the maximum plate size.

In addition to the original figures, authors must provide two good copies of each original figure (plate) for review purposes. Electrostatic copies of photographs are usually unsatisfactory.

Tables. Keep tables to a minimum. Before constructing a table, determine whether the data might be better treated in narrative form in the text. Almost all short tables can be put in such form. Tables are numbered in Arabic numerals; the legend is in paragraph form, double spaced. Keep footnotes to an absolute minimum, using symbols or (preferably) letters set as superscripts. Titles must be brief. Do not use vertical lines.

Literature Cited. Cite references in the text by number or author-date; arrange references alphabetically regardless of which system is used. Do not indicate boldface either for author's name or volume number, and do not mark for italics either book or journal titles. Use long dash for repeated author(s) name(s).

Consult recent issue of MYCOLOGIA for citation style. Examples of the

most common forms of citation follow.

Hemmes, D.E., and H.R. Hohl. 1969. Ultrastructural changes in directing germinating sporangia of Phytophthora parasitica. Amer. J. Bot. 56: 300-313. (Journal)

_____, and _____. 1973. Mitosis and nuclear degeneration: simultaneous events during secondary sporangia formation in Phytophthora palmivora. Canad. J. Bot. 51: 1673-1675. (Journal)

Fuller, M.S. 1966. Structure of the uniflagellate zoospores of aquatic Phycomycetes. Pp. 67-84. In: The fungus spore. Ed., M.F. Madelin. Butterworth, London. (Edited book)

Singer, R. 1975. The Agaricales in modern taxonomy. 3rd. Ed. J. Cramer, Vaduz. 912 p. (Book)

Ridgway, R. 1912. Color standards and color nomenclature. Publ. by the author. Washington, D.C. 43 p. + 53 pl. (Book)

Cornaby, B.W. 1973. Population parameters and systems models of litter fauna in a white pine ecosystem. Ph.D. Thesis, Univ. Georgia, Athens. (Thesis or dissertation)

Burpee, L.L., P.L. Sanders, H. Cole, and R.T. Sherwood. 1980. Pathogenicity of Ceratobasidium cornigerum and related fungi representing five anastomosis groups. Phytopathology 70. (In press)

Journal citations and abbreviations must follow those used in Botanico-Periodicum-Huntianum insofar as possible. Do not cite personal communications, unpublished data or manuscripts; place such references in the text. Manuscripts must have been formally accepted for publication before they may be cited as "in press." Cite journal and volume number (if known).

Brief Articles. Notes or articles of...other than Literature Cited.

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

14-II-1980

Dr. Clark T. Rogerson
The New York Botanical Garden
Bronx, New York 10458

Dear Clark:

I have just examined the materials for evaluation, and after careful consideration recommend that we choose option 2, that is, to retain Lancaster Press and the same format, increase the line screenings to 175 or higher (if no additional cost is involved), widen the line of text and reduce the margins. As long as Lancaster can match Allen in all essential printing techniques, and at equivalent cost, Lancaster's contract should be continued. Seventy years of satisfactory service to MYCOLOGIA should not be relinquished for light and transient reasons. The packet is being fowarded to Henry Aldrich, the penultimate name on the list.

Sincerely,

B. Lowy

P.S. The plates from Allen Press were in the packet, but not those from Lancaster.

ROGERSON

The New York Botanical Garden

Bronx, New York 10458 (212) 220 8700

22 January 1980

Dr. B. Lowy
Dept. of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernie

Enclosed is a memorandum regarding possible format and printer changes for Mycologia to be considered by all members of the Editorial Board of Mycologia and then by the Council of the Mycological Society of America.

Please read and hold until the packet of materials reaches you. I do not have enough copies of the examples to send to each member. Hence I am asking you to look through the materials as soon as you get them and then forward them on to the next Board member as indicated. Your response should come directly to me. The last person on the list should return the packet to me.

I hope that you will be able to give the matter prompt attention so that your feelings and decisions can be given to the Council no later than May 1, 1980.

Sincerely,

Clark

Clark T. Rogerson
Managing Editor
Mycologia

CTR/mk
Enc.

Packet I should be coming to you from D. Weber. Please forward to Henry Aldrich

MEMO

TO: Members of the Editorial Board, Mycologia

V. Ahmadjian	R. Korf
H. Aldrich	C. Shearer
M. Bigelow	C. J. Kwon-Chung
E. Butler	B. Lowy
O. R. Collins	D. Weber
T. Johnson	K. Wells

FROM: Clark T. Rogerson, Managing Editor

DATE: 22 January 1980

SUBJECT: Possible format and printer changes

At the 1977 and 1978 meetings, members of the Council of MSA discussed the possibility of enlarging the page size of Mycologia, of improving the reproduction of half-tones (photographs), and of changing printers. I was asked to get comparative costs and examples of improvement of photographic reproduction from both Lancaster Press and from Allen Press. The Council went on record (1977) that the format was not to be changed to the size of American Journal of Botany or of Canadian Journal of Botany with double column but that an increased size from the current page size of 9 1/4 x 6 inches to the size (10 x 7 inches) of several journals being printed by Allen Press, such as Brittonia, be considered. The space occupied by print and/or illustrations in the current format is 7 x 4 1/2 inches; the space in the larger format would be 8 x 5 inches (this latter width is about the limit in a single column for easy and comfortable reading). The main advantage expressed by council members for the larger size was the possibility of larger illustrations and more photographs per plate, particularly for papers in morphology.

Another suggestion to be considered is to leave the page size as is but widen the line of text and the width of illustration and thus reduce the margins. It could be feasible to have photographs fill the entire width of the page. The problem here would be that part of the margin is trimmed away when the volumes are bound.

Attempts to compare costs between the two printers were indecisive. Different equipment, different methods of printing, different sizes of print, different ways of cost-accounting the work, etc. are involved. Both printers could print either size but Lancaster Press gets a more efficient (and thus less costly) production with the smaller size; Allen Press with the larger size. Both agree that the major problem on future costs is the skyrocketing increase in prices of the high quality paper that we use. At the moment printing costs per volume are approximately the same by either press. Some saving in paper costs should result with the larger sized page.

In the modern method of printing, half-tones (photographs) are reproduced with various categories of "line screening." In the current and recent issues of Mycologia a 135 screening has been used. Lancaster Press has the necessary equipment to increase this to 150 or 175 without additional cost. Allen Press can provide these but in addition has the equipment for a 300 screening at no extra cost. The 300 screening will be used by Allen Press in the 1980 volume of American Journal of Botany. Incidentally the page size of AJB will be reduced to 8 1/2 x 5 3/4 inches (with considerable claimed savings in printing costs) beginning with the 1980 volume.

Dr. Charles Mims provided two plates of photographs in two sizes. Lancaster Press reproduced these using 150 and 175 screenings; Allen Press has reproduced these at 175 and 300 screenings. Keep in mind that both printers can provide best reproduction if original plates are not reduced.

Enclosed are the following items for your study:

1. From Lancaster Press--reproduction of the two plates at 150 and 175 screenings.

2. From Allen Press--reproduction of the two plates at 175 and 300 screenings.

3. From Allen Press--typeset material from an article published in Mycologia 69(6): 1196, 1977. Compare with your issue of Mycologia and note the smaller size and different kind of print plus the wider line. If possible, compare a current issue of Brittonia with current issues of Mycologia.

After you have studied these materials, please consider the following questions and let me know your opinion:

1. Should we stay with Lancaster Press and maintain the same format and size of Mycologia but increase the screening of photos to 150 or 175? Lancaster Press has been our printer for all of the 72 volumes or Mycologia published to date and the page size has been the same from the beginning, 1909.

2. Should we stay with Lancaster Press and maintain the same format, increase the line screenings, etc., but widen the line of text and width of illustrations, thus reducing the margins?

3. Should we stay with Lancaster Press but enlarge the page size and screenings as indicated above?

A meaningful saving in space would result from adopting option 2, with the added advantage of retaining the original format.

4. Should we change to Allen Press, keep the small size page but increase screenings to 150, 175, or 300? Increase the width of text and illustrations?

5. Should we change to Allen Press, enlarge the size of the page and increase the screenings? Allen Press is now printing most of the North American botanical journals.

Your views and comments will be passed on to the Council in advance of the 1980 meetings in August so that a discussion and a decision can be made in August 1980 in time for the 1981 volume of Mycologia.

Please send your opinions to me as soon as possible.

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Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

June 9, 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge LA 70803

Dear Bernard:

Your paper "Syzygospora alba..." has been reviewed. I enclose copy of the reviewers' remarks, and two marked copies (one in red) of the manuscript.

The reviewers recommend that the paper be accepted, but have made some suggestions for improvement. My comments are largely of an editorial nature, designed to put the account in the current format for the journal (for example, spacing, journal abbreviations, legend style).

Would you please consider the various suggestions as you revise the account? When you send the revised manuscript will you also return the copy which I marked, please? Thank you.

With changes, this paper can be accepted. If you send revised copy to me by the end of this month (or shortly thereafter), I shall try to include the paper in the November-December (1981) issue.

Kindest regards.

Sincerely yours,

Terry

T.W. Johnson, Jr.

REVIEWER (1)

June 5, 1981

Dr. T. W. Johnson, Jr.
Department of Botany
Duke University
Durham, N. C. 27706

Dear Terry:

I have examined the enclosed manuscript by F. Oberwinkler and B. Lowy and would recommend its publication in MYCOLOGIA.

The paper is well written with excellent illustrations. The true nature of Syzygospora alba has been a matter of debate for some time, and the authors made a significant contribution to the understanding of this species.

More detailed comments follow:

p. 1, l. 17	dolipore septa....
p. 2, l. 2	basidiomycetes....
p. 2, l. 16	Tremellaceae, i.e., the ....
p. 2, l. 21-24	Panama: Prov. Chirique, Valley of Upper Rio Chiriqui Viejo, G.W. Martin 2167 (holotype*, BPI); .... [Shorten description of second specimen in a similar manner.]
	*There is no such thing as a co-type. Is GWM 2167 the holotype? I don't know.
p. 3, l. 21	asymmetrically attached basidiospores. [?]
p. 3, l. 25	young developmental stages.
p. 4, l. 3, 4	, and with thin, smooth, non-amyloid walls.
p. 4, l. 5	basidiospores that germinated showed ....
p. 4, l. 9, 10	and is apparantly the only propagative stage during certain developmental stages of the basidiocarp.
p. 4, l. 14	that finally becomes detached.
p. 4, l. 21, 23	Surprisingly, good results were obtained from material that had been stored in the herbarium for several years.
p. 5, l. 18	Martin (1937)
p. 5, l. 23	She indicated ....
p. 6, l. 3	Boidin (1970) ....

p. 6, l. 9 holotype .... [?]

p. 6, l. 11 paratype (Martin 2517) Boidin (1970) .... [?]

p. 7, l. 2 "co-type" shouldn't be used.

REVIEWER (2)

Review of

"Syzygospora alba Martin, a mycoparasitic Heterobasidiomycete"

by Oberwinkler & Lowy

This is an interesting and well written article quite worthy of publication in Mycologia. I have made a few minor remarks in the manuscript. It is quite likely that P. H. B. Talbot would transfer this genus to the Homobasidiomycetes. It is my opinion, however, that it should be retained in the Heterobasidiomycetes as recommended by these authors. Finally, I wonder if the authors could include within their illustrations an EM photograph of a dolipore septum?

REVIEWER (2)

Syzygospora alba Martin, a mycoparasitic Heterobasidiomycete[1]

F. Oberwinkler

Lehrstuhl Spezielle Botanik und Botanischer Garten der

Universität Tübingen, Auf der Morgenstelle 1,

D 7400 Tübingen 1, West Germany

and

B. Lowy

Department of Botany, Louisiana State University

Baton Rouge, Louisiana 70803

SUMMARY

Syzygospora alba Martin is recognized as a mycoparasite and its ana- and teleomorphs are illustrated and redescribed from a recent collection from Mexico. The basidiocarps are similar to those of some species of the genus *Tremella*. Haustoria of the *Tremella* type attach to and penetrate into the basidiomycetous host cells. Holobasidia produce basidiospores capable of yeast-like budding. The dolipore structures appear to be similar to those in species of the genera *Filobasidium* and *Filobasidiella*. On the basis of these characters a heterobasidiomycetous affinity is proposed. The name *Syzygospora alba* is accepted for the holomorph of the species. It appears that this taxon is generically different from *Christiansenia pallida* Hauerslev.

---

[1]Part 10 in a series "Studies in Heterobasidiomycetes" of the Institut für Biologie I, University of Tübingen.

INTRODUCTION

Mycoparasitism is widespread among different species of Basidiomycetes, especially of Heterobasidiomycetes. Many species of Tremella are parasitic on other fungi. The parasitic behavior is often clearly indicated by the presence of haustorial hyphae which penetrate into the host cells. In addition, sometimes the host-parasite interaction can be observed in the changed morphology of the host, as in Tremella encephala Pers. ex Pers. on Stereum sanguinolentum (A. & S. ex Fr.) Fr. (Bandoni 1961) or Tremella aurantia Schw. on Stereum hirsutum (Willd.ex Fr.) S.F. Gray. Several of these Tremella species share a common fruiting body structure by which they can be recognized in the field.

A fungus collected by G. Guzmán in Mexico, and with the external appearance of a Tremella, was sent to us for identification. The species turned out, however, to lack the leading character of the Tremellaceae, viz. the cruciate-septate basidium. Therefore a more detailed study was carried out to determine the appropriate taxonomic position of the fungus.

MATERIALS AND METHODS

The following specimens are described and illustrated in the present contribution. Syzygospora alba Martin, Fungi of Panama, Prov. Chiriquí: Valley of the upper Río Chiriquí Viejo, alt. 1600-1800 m, July 1, 1935; G.W. Martin No. 2167, co-type (Herb. State Univ. Iowa, now BPI). Syzygospora alba Martin, Mexico: Entre Los Guayabos y Las Cabañas, 15 km al SW de Mazamitla, Carretera a Tamazula, Jalisco; bosque de Pinus-Quercus, muy perturbado, en transacción con vegetación subtropical; alt. 1700-1800 m, Agosto 24, 1974; Col. G. Guzmán, No. 11843 (Herbario de la

Escuela Nacional de Ciencias Biólogicas, Instituto Politécnico Nacional, México, D.F.).

For transmission electron microscopy material was soaked in water, then fixed with glutaraldehyde and osmium tetroxide, washed with distilled water, dehydrated in an alcohol series and embedded in ERL according to Spurr (1969). Ultrathin sections were mounted on unsupported mesh copper grids, poststained in uranyl acetate and lead citrate solutions, and examined in a Zeiss EM 9 S-2 transmission electron microscope.

RESULTS

The basidiocarps of Syzygospora alba are tremelloid and gyrose (Figs. 1,2). This structure is apparently not or not essentially produced by gall-like, hypertrophic growth of the host. In dried specimens the fruiting bodies are brownish and have a very tough to hard-horny consistency, but may be soft gelatinous when they are fresh. The trama consists of a layer of hyphae 2-4 µm in diam, hyaline, thin walled, loosely branched, the branches commonly originating from clamps (Figs. 7,8). Short, very narrow hyphal outgrowths capable of functioning as haustoria (Figs. 7,8,12) are formed mainly from clamp swellings. The hymenium (Fig. 8) is composed of long, apically swollen basidia (6-8 x 50-100 µm) with four curved, stout sterigmata (Figs. 9,10) which bear asymmetrically formed basidiospores. Sometimes partly cruciate-septate basidial apices can be found (Figs. 9,10) which appear Metabourdotia-like as described by Olive (1957), and by Lowy (1964) for Pseudotulasnella. The basidia are intermixed with small, thin walled hyphae which obviously represent young developmental stages in basidial ontogeny. These hyphae sometimes branch to form lateral haustoria. It appears that the hymenium is thickened by hyphal proliferation below the basidia. All hyphae in the

Could any of those be paraphyses? →

hymenium and at the bases of the basidia are septate at the locus of clamp connections. The basidiospores are hyaline, depressed and drop-like (Fig. 3), 6-8 x 9-11 µm, asymmetrically apiculate (Fig. 6), the walls thin, smooth, and non-amyloid.

Those basidiospores which were germinating showed predominantly a yeast-like budding of the Tremella type (Figs. 4,5). Rarely germination by hyphal formation could also be observed (Fig. 5). We do not know whether or not secondary spores are produced. In all known collections, conidial formation is very striking and is apparently the only propagative state in special developmental stages of the fruiting bodies. Short-celled conidiophores produce two terminal cells which simultaneously begin to form opposed beak-like outgrowths (Figs. 11,13,14). The out-growths extend to globose bodies which fuse to form a one-celled conidium that finally breaks off the conidiophore. The conidiogenous cells are capable of repeated conidium formation around the circumference of the transverse septum separating the cells.

The conidiophore-bearing hyphae proliferate strongly to form succes-sive conidiophores. An immense quantity of conidia is produced, and these are distributed over the outer and inner surfaces of the fruiting body. In addition to examination with the light microscope we also studied the fungus with the transmission electron microscope. Surprisingly the several years old herbarium material yielded successful results when prepared for ultrastructural study. Two septal pore types are present, both with dolipore structures. Many dolipores, however, seem to be without parenthe-somes, as in the septal pores of Filobasidium floriforme L. Olive (Moore & Kreger van Rij 1972) and Filobasidiella neoformans Kwon-Chung (Kwon-Chung &

Popkins 1976). Because they are present in the hymenial region, we are convinced that these dolipores belong to the hyphae of Syzygospora alba. In basal portions of the basidiocarp, on the other hand, the second dolipore type with perforated parenthesomes can also be found. We are fully aware of the difficulties of studying ultrastructural details in old herbarium material. However, some of the structures revealed are quite significant and useful in understanding the taxonomic relationships of this fungus.

The parasite grows in hyphae which are thin walled and clamped (Fig. 7); therefore the host must be a basidiomycete, but because of the paucity of additional characteristics a more detailed interpretation of the host cannot now be given. To our knowledge, Syzygospora alba is presently known only from six collections from Panama and one collection from Mexico.

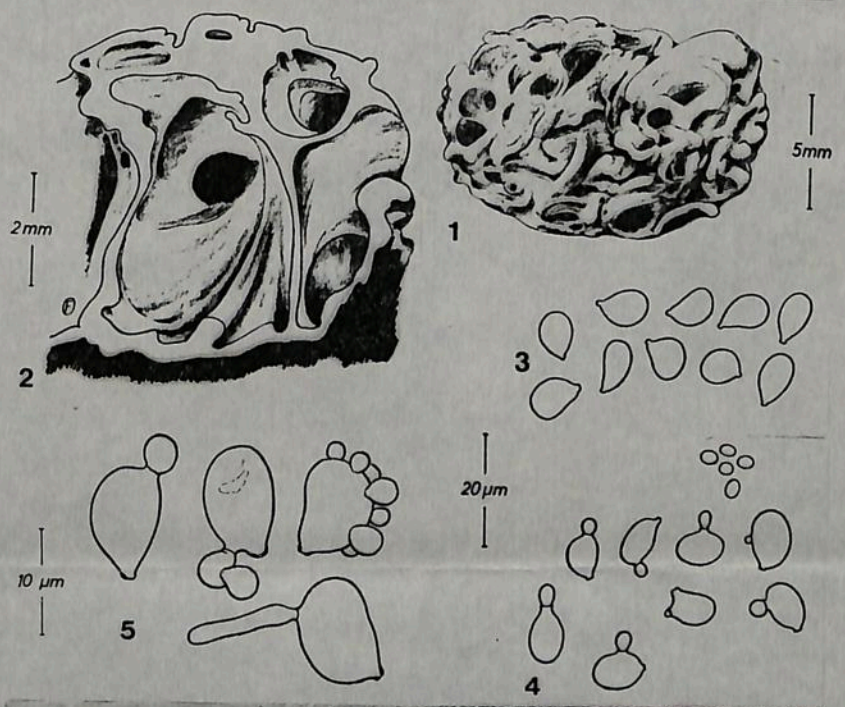
DISCUSSION

The genus Syzygospora was erected by Martin in 1937 to describe the species Syzygospora alba. This fungus is characterized by the formation of paired blastogenous cells which fuse and are released from the supporting hyphae (Figs. 11,12). Martin (l.c.) interpreted this structure as a special type of auriculariaceous basidium. However, in a restudy of the species by Kao (1956), it was shown that the unusual structures were conidia and that the conidiophores were connected with generative hyphae that produce holobasidia. This can now be confirmed by our investigations. Futhermore, Kao elucidated nulcear behavior during conidium formation. He indicated that the primary outgrowths of the conidiogenous cells are uninucleate, and after fusion, a dikaryotic conidium is formed. In a study of Christiansenia pallida Hauerslev, Boidin (1970) was able to demonstrate that conidium

formation in this species is essentially similar. Because of the unique development, he proposed the term "zygoconidium" for the propagule.

After a detailed discussion, Boidin (1.c) accepted the interpretation of Donk (1962) to consider the name Syzygospora as a "nomen anamorphosis." It seems obvious that Martin (1937) assigned the basidial stage to the anamorph in his description. However, in the type material the true teleomorph is also present. Fig.1a of Martin's illustration shows a young basidium and not a cystidium, as Boidin (1970) already correctly reinterpreted this figure. Our study of a co-type specimen (Martin 2167) confirms that collasped basidia are connected with the conidiophore-bearing hyphae (Fig. 12). In another co-type (Martin 2517) Boidin (1.c.) found several basidiospores intermixed with innumerable conidia. Our study shows that ana- and teleomorphs can be present in the same fruiting body. Because of the same condition found in the type material we are inclined to accept the interpretation of Art. 59 of the Nomenclatural Code in the sense of Weresub et al. (1974), "...that the application of a name is determined by its type material." We consequently propose to use the name Syzygospora alba Martin for the teleomorph and the holomorph of this fungus. Therefore we cannot accept the nomenclatorial transfer of the species to "Christiansenia alba Boidin ex Martin" (Boidin 1970). On the other hand it seems justified to accept Christiansenia (Hauerslev 1969) as a separate genus mainly because of the very different fruiting body morphology. We are unable at present to assess the generic value of the different characteristics of the basidia, and of basidiospore germination types.

PLATE I



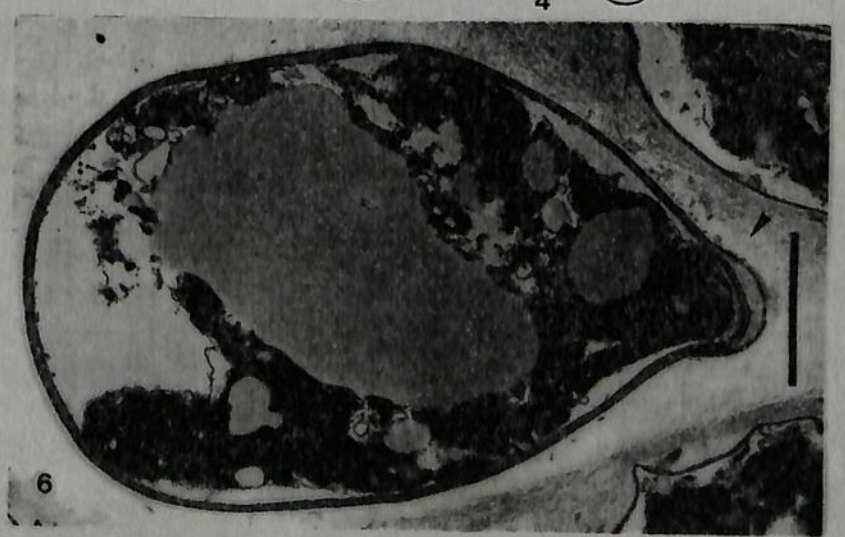
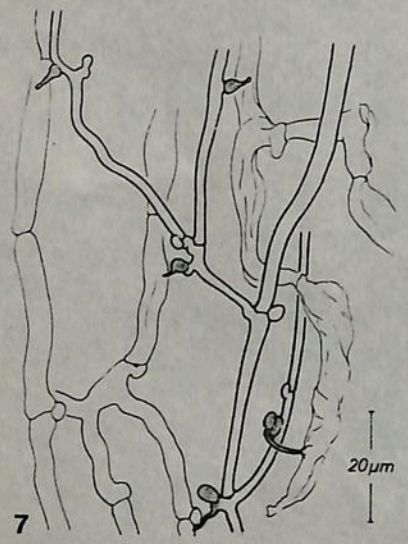


PLATE II



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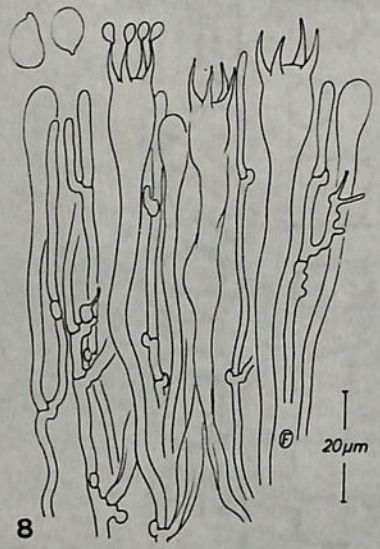

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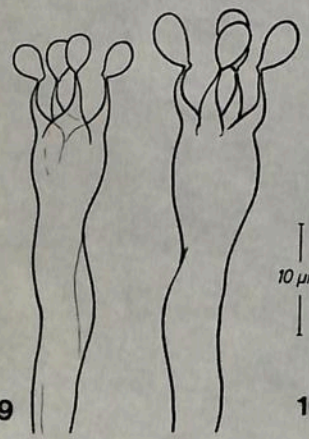

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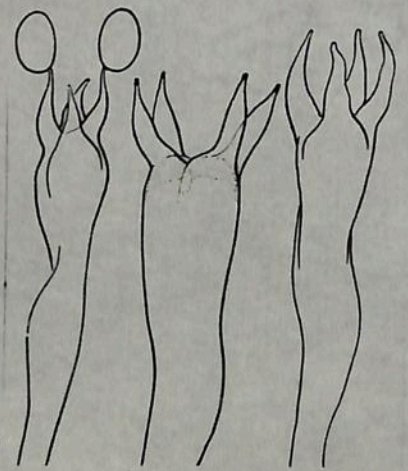

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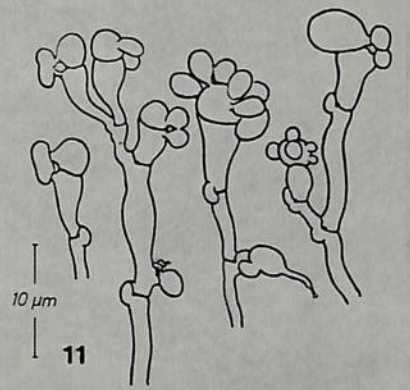
PLATE III



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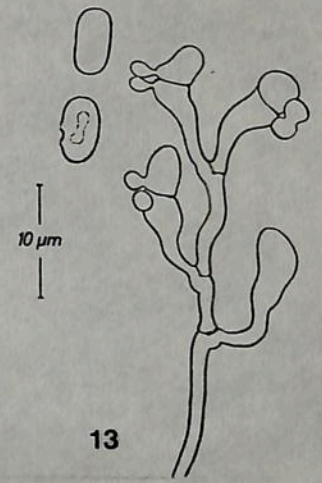

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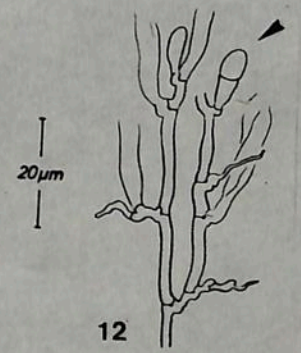

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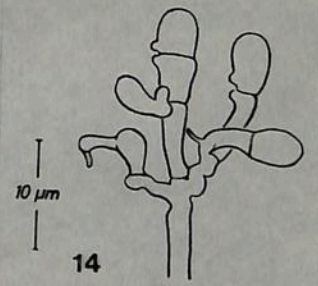

14

ACKNOWLEDGMENTS

The late Dr. G.W. Martin provided the co-type material used in this study. We are grateful to Dr. G. Guzmán for sending the specimen of Syzygospora alba from Mexico.

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LEGENDS TO FIGURES

Plate I

Fig. 1. Habit sketch of dried herbarium specimen. Fig. 2. Section through a dry basidiocarp showing the gyrose morphology. Fig. 3. Mature basidiospores. Fig.4. Budding basidiospores and yeast cells. Fig. 5. Basidiospores showing yeast-like budding (above) and germination by hypha (below). Fig. 6. TEM micrograph of a median section of a basidiospore. Note the splitting of the cell wall of one side of the apiculus (arrow). Bar equals 2 µm.

Plate II

Fig. 7. Hyphal context of the inner part of the basidiocarp; hyphae of the parasite connected to the host cells with tremelloid haustoria. Fig. 8. Part of the hymenium with different stages of basidial development, and basidiospores. Fig. 9. Apical parts of basidia with young basidiospores. Fig. 10. Apical details of mature basidia with central figure showing a partial septum.

Plate III

Fig. 11. Conidiophores with different stages of zygoconidium formation. Fig. 12. Conidiophore (arrow) showing connection with haustoria and collapsed basidia (from co-type of Syzygospora alba, Martin 2167). Fig. 13. Condiophores and conidia (Martin 2167). Fig. 14. Young conidiophores with haustoria (Martin 2167).

MYCOLOGIA—EDITORIAL OFFICE

18 May 1981

Dear Bernard,

This will acknowledge receipt (today) of your paper on *Szyzgospora alba* (with F. Oberwinkler. The paper has been sent for review, and I will contact you as soon as the reviewers have responded. Since only two copies were sent of the manuscript, I took the liberty of making an additional copy (for review), and assume this has your permission.

Thank you. Kindest regards.

Sincerely yours,

Terry

T. W. Johnson, Jr.

MYCOLOGIA
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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

June 1, 1981

MEMO: August, 1981 meeting, Editorial Board

TO: Editorial Board

Customarily, the Editorial Board meets during the annual meeting of the Society, but, because of the early schedule of the Council, cannot do so until after the latter body has met. I would like to meet with the Editorial Board sometime during the AIBS meeting, perhaps on Wednesday, August 19th, late in the afternoon.

(1) The matter of appointments to the Editorial Board (replacements for Henry Aldrich and Darrell Weber) must be settled by mail prior to the Council meeting.

There is a substantial list of persons recommended for five-year appointments to the Board. As you know, we need Board member replacements in the areas of physiology/biochemistry, and ultrastructure, hence the two lists:

Physiology/Biochemistry

D. J. Weber (has agreed to serve an additional five-year term if elected)
G.C. Carroll
Alan Jaworski

Ultrastructure

I. K. Ross
David McLaughlin
I. Brent Heath
Charles Mims
Larry Littlefield
G. W. Erdos
Martha Powell
W. M. Hess

Please reply promptly by mail (postcard enclosed), listing, in order of preference, two names from each list. I will sort the returns, and follow a simple majority. I ask for two names in each group so that alternates may be contacted if some choose to decline election.

(2) Earlier this year, I asked for opinions regarding the addition of "key words" or "descriptors" (for indexing purposes) to all printed articles. Five members of the Board favored this requirement; there were no other responses. If you have

not expressed your view in this matter, please do so, and I will act accordingly. This matter should go to the Council for their approval if the Board in the majority recommends it.

(3) I am not aware of any other matters requiring Board action before the Council meets. I will see to it that you get copies of my report to the Council, and a summary of the Editorial Board meeting to be held at Indiana. It would be helpful in planning if I knew in advance whether or not you will be at the Indiana meetings.

(4) Reviewers are always needed, particularly in the area of physiology/biochemistry and plant pathology. I will be very grateful for suggestions from you - please!

I take this opportunity to thank you for your service on the Board. I am grateful for all your advice and help, and especially for the prompt, and critical but fair reviews of manuscripts. Your efforts to provide careful, conscientious reviews are of great value to authors, and make my task infinitely easier. I am more confident in what I am doing because of the support you have given so willingly to me. Thank you very much.

Terry

T.W. Johnson, Jr.

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

15-V-1981

Dr. T.W.Johnson, Jr.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Terry:

I am submitting for your consideration as a contribution to MYCOLOGIA the enclosed paper in which Franz Oberwinkler and I have collaborated. In addition to the original plates I have included xerox copies of them which should be adequate for review purposes since all but one of the figures are drawings.

Under separate cover I am also returning Helana S. Hoover-Litty's paper to you with my comments and corrections.

as ever,

Bernard

B. Lowy

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
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3-VI-1981

Dr. T.W.Johnson, Jr.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Terry:

Dr. Kiefer's letter dated May 5, 1981, makes several cogent points that I had not previously considered, and which have made me change my earlier opinion regarding the advisability of publishing his response in MYCOLOGIA. Although I believe that rebuttals of this nature should preferably appear in the MSA NEWSLETTER (assuming the consent of the Editorial Board), and that their publication in MYCOLOGIA establishes an undesirable precedent, unfortunately this was already done when Malchol's 1980 paper was accepted by our previous editor. Incidentally, I was a Board member at the time, but the paper was not sent to me for an opinion.

Upon reconsideration of all the information now at hand, I believe that Dr. Kiefer is justified in making his request. If we measure the desirability of either following or breaking precedent on the one hand, as opposed to fair and equal treatment in the press on the other (for this response), I must choose the latter. I recommend that Dr. Kiefer's paper be published in MYCOLOGIA with the understanding that further rebuttals should be published elsewhere.

Quite apart from the final decision that may be made in this case by the Editorial Board, I think that your objective treatment of this problem is beyond reproach.

Sincerely yours,

Bernard

B. Lowy

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

27 May 1981

Memo: Kiefer Manuscript

To: Editorial Board

I am enclosing copy of all the correspondence I have had with Professor Kiefer regarding publication of his article "On taxonomy...," and also a copy of the manuscript. Since there was still some question in his mind about the decision on publishing his manuscript, I agreed to resubmit the paper to the Board (my letter of 12 May).

Would you please read the manuscript again (it is not in MYCOLOGIA format), and consider it in light of the explanations/comments presented by Dr. Kiefer? Please let me know whether or not the paper should be accepted for publication. I enclose a stamped envelope for your convenience. Please do not return the manuscript or correspondence copies.

It would be very helpful to me if you would give some justification (brief, of course) for your decision.

If we are to publish this brief note, I think it is essential that Machol know this beforehand. I shall so inform Professor Kiefer. I hope you would agree that any further responses/rebuttals would not be published in MYCOLOGIA.

Your advise is very much needed.

Terry
T.W. Johnson, Jr.
28 May 1981

TWJ:jd

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

April 6, 1981

Dr. J. Kiefer
Department of Statistics
University of California, Berkeley
Berkeley CA 94720

Dear Dr. Kiefer:

The majority of the members of the Editorial Board have replied to my request for a ruling on your paper "On taxonomy--acknowledgment and explanation." The recommendation is unanimous that your article should not be published in MYCOLOGIA.

While those who have thus far responded appreciate the justification for your remarks, they regard the manuscript as being in the form of a "letter to the editor," and therefore not appropriate for the journal. Some members of the Board felt that if you would care to submit a complete manuscript that presented additional data, this would certainly be looked on as appropriate. However, all those who responded suggest that the paper should be sent to the Society's NEWSLETTER, which has become the proper outlet for criticisms and responses. I must, of course, abide by the decision of the Editorial Board.

The Editors of the NEWSLETTER require that some officer of the Mycological Society "endorse" the submission of response/rebuttal manuscripts. I am prepared to do this in your case, if you wish to submit your paper to the NEWSLETTER. If you choose to send it to Dr. Wicklow, please let me know, and I will write to him immediately. The NEWSLETTER requires camera-ready copy, hence I am returning your manuscript to you. I enclose a copy of the pertinent instructions sent out by the editors of the NEWSLETTER.

Sincerely yours,

T.W. Johnson, Jr.

(2)

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DEPARTMENT OF STATISTICS
BERKELEY, CALIFORNIA 94720

May 5, 1981

Dr. T.W. Johnson, Jr.
Editor-in-Chief, Mycologia
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Dr. Johnson:

Thank you for your letter of April 6. The delay in my answering is not due to lack of interest and concern, but because your mailing, clearly marked "First Class Mail", only arrived yesterday! With the amount of mail you must handle, you have undoubtedly experienced similar difficulties with the postal service.

I am grateful for your efforts in connection with the note "On Taxonomy--acknowledgement and explanation" which I submitted to Mycologia. I understand your obligation to abide by the decision of the Editorial Board, and am grateful for your offer to endorse the submission of this paper as a "response/rebuttal" to the NEWSLETTER. Before I take this course, I would like to clarify some aspects of this situation.

Firstly, I do not really understand the difference in character between Machol's 1980 paper and the present submission from me. There was no more "additional data" in his paper than in mine, unless one views the description of an isolated species collection in the Farlow Herbarium as an excuse for publishing a paper. His paper is mostly a continuation of our previous discussion and argument, together with his own polemics, accusing me of "an extraordinary polemic" that contains "many ad hominem comments". He accuses me of making attributions to him and Singer of "remarkable assertions they did not make" and makes other statements about my writing that are contradicted by what he and Singer have published explicitly. Since I do not believe you were Editor at the time his paper must have been submitted, its publication was not your responsibility, but perhaps

you can guess, for my benefit, at what the Editorial Board saw in his paper that made them agree to publish it, without considering my paper in the same light. Perhaps it is relevant to comment that, despite the assertions he made about my criticism here and in the 1977 paper with Singer, I was never shown either paper (for the sake of commenting on its accuracy, whether or not the Editor accepted my comments) before those papers were published!

Thus, although you mention that some members of the Board felt it would certainly be looked on as appropriate if I submitted a "complete manuscript that presented additional data", I am not sure what this means. The subject of discussion in these last few efforts by Machol and me is the appropriateness of the Machol-Singer methodology. Does "additional data" therefore refer to a more extensive presentation of some of the shortcomings of their method, as exemplified by their tables, but which I only alluded to (for the sake of brevity) in the manuscript I submitted? I could easily write a more extensive critique in that vein, since, as I mentioned, this issue of the proper calculation of character frequencies was not completely covered in my previous paper, and has been described in misleading terms by Machol. If "additional data" means that I must go out and conduct a study of my own by collecting character frequencies of new genera, I must obviously decline since I have no such study in which I am participating at the moment. But Machol did not present the results for any new genera in this last paper of his.

Of course, I realize that you cannot commit Mycologia for the Editorial Board to the publication of an unseen manuscript. However, it would be foolish of me to embark on the writing of a paper that you have indicated some members of the Board felt would be appropriate for consideration, if enough other members already have such negative views (not so much because they do not find appropriate content in my paper, but perhaps because they are sick of the whole controversy) that the complete manuscript you mention would in reality have little chance of publication. If you find it fitting to give your frank view about this, I would appreciate it. I thought I remembered seeing short articles in Mycologia devoted to corrections, comments, and further explanations; so I also wondered whether there had been a change in editorial policy in the recent past, to publish such items in the NEWSLETTER; and, if so, why Machol's article was not published there.

I hope I am not being too much of a bother, since I realize someone in your position must have a large number of authors

with whom to correspond, and this is not the most important item you have to handle. I really do appreciate the consideration you have given to my paper, and your helpful comments.

Sincerely yours,

Jack Kiefer

J. Kiefer

JK:do'r

(5)

MYCOLOGIA

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Managing Editor
The New York Botanical Garden
Bronx, New York 10458

May 12, 1981

Dr. Jack Kiefer
Department of Statistics
University of California, Berkeley
Berkeley CA 94720

Dear Dr. Kiefer:

Thank you for your letter of 5 May; I am sorry my April mailing to you was delayed, but I make no apologies for the Postal Service. Your letter is difficult to respond to since it touches events that took place before I became editor, and I am not privy to the records of correspondence and review processes that went on earlier. The Editorial Board voted on your manuscript, but provided no or only brief comments (with one exception; copy enclosed). It is important that you not feel you are a "bother" in this matter; any query from any author is handled by me as if it were the only query extant.

As the Editorial Board was not explicit in proposing that an expanded account be written (with "additional data"), I do not see this as a viable course of action. That being so, I am forced to try to "second guess" what a previous editor had in mind, and this I fail in miserably. I do not have access to any correspondence or reviews relative to any prior papers on the subject published in MYCOLOGIA, but knowing the former editor, I am confident she followed established procedure. I suspect that the decision to publish Machol's article in MYCOLOGIA was arrived at after consultation (perhaps not as extensively as I sought when handling your manuscript, but certainly sufficiently broad to get a concensus view). In any case, I think it is not appropriate to "guess" about past events, but I think it is appropriate for me to say that I feel Machol's article was better suited to the NEWSLETTER than to MYCOLOGIA.

At the time I was about to become editor, the Board was considering another somewhat similar matter involving disagreements between authors. The Board's decision was clear (I participated in it as a Board member), and the complainant was given opportunity to publish an explanation/rebuttal in the NEWSLETTER although she had wanted access to pages in MYCOLOGIA. I think a precedent was then established, and it is my opinion that the Board adhered to that prior decision in treating your manuscript. The Board has considered your paper to be in the nature of a "letter-to-the-editor," as I stated earlier, and this springs from a decision last year to ask that such notes be put in the NEWSLETTER. This does represent a change in editorial policy, although one that has gradually evolved rather than having been started de novo. Our policy now is clearly stated. When manuscripts or notes appear to be specific criticisms of a

published paper, or appear to be rebuttals to comments, we with permission contact all parties, and share all correspondence and manuscripts. This allows all parties to be aware of the matter, and prevents the situation that appears to have arisen in your case, namely, publication of Machol's article but not yours.

In the interests of airing fully all of my involvement with your article, I enclose copy of a late-arriving letter from a Board member. His letter needs no comment, only to have it recorded that his was the only vote favoring acceptance. I am afraid I could not fly in the face of the clear majority and ignore their decision.

I hope the foregoing comments respond adequately (they certainly are candid from my point of view) to your queries. The problem remaining is to find a solution.

I can, of course, with your permission, resubmit your manuscript to the Editorial Board, along with copy of your May 5th letter, and copies of the Machol article to which you respond. You have my assurance that I am willing to do this at once, if you wish. Perhaps the Board would reverse its decision, but I have no evidence that it would. The important point is that at the moment the Board (majority) views your paper as more appropriately carried in the pages of the NEWSLETTER. Submitting your paper directly to the NEWSLETTER editors is the other alternative, and as I said before, I will be glad to endorse such a move.

These are the only alternatives I see, and I will be glad to follow up on whichever route you choose. I believe I have responded candidly, and while not favorably, I suppose, at least you know that I am willing to go farther. I favor the NEWSLETTER route because I know from experience it receives as wide distribution as the journal, and may, in fact, be more avidly read.

Please let me know if I can be of further service.

Sincerely yours,

T.W. Johnson, Jr.

(7)

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DEPARTMENT OF STATISTICS
BERKELEY, CALIFORNIA 94720

20 May 1981

Dr. T.W. Johnson, Jr.
Editor-in-Chief, Mycologia
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Dr. Johnson:

Many thanks for your letter of May 12th and for the patience and understanding you have shown in answering my inquiry about possible courses of action I might take with my paper "On taxonomy -- acknowledgement and explanation."

I herewith accept your kind offer to resubmit my manuscript to the Editorial Board, along with my letter of May 5th that you proposed to send along. I choose this course of action because it is not clear that the members of the Editorial Board had full knowledge of the background in this case, many aspects of which are outlined in my May 5th letter. Obviously, Mycologia has every right to change its editorial procedures and make the NEWSLETTER the vehicle for comments about previous publications. However, as is pointed out in the quotation you sent me from a letter from one Editorial Board member, a doctrine of fairness would imply that, in a particular scientific controversy, all authors be treated in the same manner. Since Machol's 1980 paper, which commented on my previous article in a manner exactly suitable for the NEWSLETTER, was published in Mycologia itself, it would seem only just to give my paper the same treatment, although the change in policy presumably came into force between the times when our two papers were submitted.

If mine were the first note in response to a major article of his, the NEWSLETTER would now be the proper place for it; but this is, rather, the last in a series of back-and-forth comments by the two of us.

(8)

The shortness of my article may help to make it seem like a "Letter to the Editor". However, it does contain brief remarks adding to the content of my previous paper, and not merely answering Machol. It is surely simplest for the reader if he can find all entries in this argument within the same publication. The comment you quoted from the Editorial Board member was generous in finding my article "objective (i.e., it is not a personal attack)." Machol's article is highly personal, so I would hope I deserve at least this temperate reply. But I would not have submitted the paper at all, had I not believed that he now emphasizes a matter that was vague in the original Machol-Singer paper, the remarkable manner of computing frequencies by giving equal weight to subtaxa of greatly different sizes. The Census Bureau and other sample takers have long guarded against making this error, but I believe it is important to point it out to potential users of the Machol-Singer methodology.

Enclosed, then, is the paper I am resubmitting, which I assume you will send to the Editorial Board as you offered to do, together with my May 5th letter and perhaps the present one. Again, my thanks for your articulate explanation of the circumstances, and your fair consideration of my position. I will abide by the Board's decision without taking more of your valuable time.

Sincerely yours,

Jack Kiefer

J. Kiefer

JK:do'r

(9)

ON TAXONOMY - ACKNOWLEDGEMENT AND EXPLANATION

J. Kiefer
Department of Statistics
University of California
Berkeley, California 94720

Machol (1980) has responded to the criticism in Kiefer (1975, 1979) of the approach of Machol and Singer (1971, 1977) for assigning a lower-level taxon to one of several higher-level taxa. The reader of the conflicting statements in some of these papers deserves further explanation and, where it can be achieved, agreement. I will refer to the papers as M, K-I, K-II, MS-I, and MS-II, respectively.

I acknowledge the correctness of Machol's statement (M, bottom of p. 1222 - top of p. 1223) that I was incorrect in asserting (p. 375 of K-II) that the MS tables "contain an unbelievably large number of entries that are simply impossible." The convention of MS-I, p. 763, indeed yields the questioned frequency for Gerronema. Its extension to differing specimens of a species (not explicitly described on p. 763 of MS-I, but in accordance with the use of judgment recommended on p. 765) allows one to obtain M's calculation for Ripartites. For the latter, I would have realized that this genus with 5 species could have a fraction .150 of its species (not a multiple of 1/5) with adnate hymenophore, had I checked that 2 of Singer's 4 specimens of R. strigiceps preserved at the Farlow Herbarium had this

characteristic, and that 2 had it on one side. Thus, the calculation of .150 indeed conforms to the MS technique, if one applies it even to fractions of individual specimens in a species. The MS determination of character frequencies was criticized on p. 375 top of K-II because the results cannot readily be checked by another taxonomist, since MS also employed estimates of character frequencies when counts were not at hand and they believed the consequent error "unlikely to be significant". We are not told when such estimates, or the calculation employed for Ripartites, was used.

The method of calculation relating to Gerronema was discussed in K-II, pp. 370-371. Additional explanation may be helpful. The method is described on p. 763, MS-I, with reference to the two subspecies of Galerina, one consisting of 157 species with clamps, the other of 8 without clamps. MS choose 1/2 rather than 157/165 (or some similar fraction) for the genus clamp frequency. Although, as MS states, species counts are dependent on unequal levels of floristic exploration, the 8 and 157 are perhaps somewhat representative of relative population sizes in the two subgenera. A random species of Galerina may thus have chance closer to 157/165 than to 1/2 of having clamps. To me this is more persuasive for using 157/165 than are the several arguments MS gives for using 1/2 (and which justify the Gerronema computation), especially in the context of the probabilistic computations they employ. Even if one thinks in terms of the

subgenera, one should weigh their sizes in computing genus frequencies. But M is right in stating that the frequencies I called "impossible" are correct according to the MS rules (extended for Ripartites as described above), and I apologize for the error.

The rest of my criticism stands. M states that K-II is "an extraordinary polemic" that contains "many ad hominem comments" and attributions to MS of "remarkable assertions they did not make"; that statistical independence is not assumed by them; that my discussion of some of the history of numerical taxonomy, that MS does not mention but which coincides or contrasts with aspects of their own developments, is "irrelevant"; that my comments are "technical quibbles". It would serve no purpose for me to refute M's assessment with repetition of my earlier explanation. Let the reader decide.

LITERATURE CITED

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MYCOLOGIA
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T.W. JOHNSON, JR.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

8 May 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernard:

Would you be willing to review the enclosed manuscript for MYCOLOGIA?

"The mycoflora of wood chips intended for use as mulch" by H. S. Hoover-Litty

If so, please prepare two copies of your comments, one an unsigned copy that can be sent to the author (reviewers who prefer not to remain anonymous may sign both copies of their appraisal). Minor corrections and suggestions may be placed directly on the manuscript using a soft lead pencil.

The following points should be considered in your review:

1. Is the material new and worthy of publication in MYCOLOGIA?

2. Is the presentation clear and logical?

3. Are the conclusions justified by the evidence?

4. Could the paper be shortened substantially without interfering with its merit?

5. Have the tables been prepared as clearly and concisely as possible? Could any be deleted? Added?

6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated. Kindly return the paper with your comments promptly.

Should you anticipate not being able to review this paper within two or three weeks, of if you prefer not to review it at all, please return the manuscript to me without delay so that it can be sent to another reviewer.

Thank you for your assistance.

Hope you can help with this one;
it is a general one. Many thanks.

Sincerely yours,

Terry

T.W. Johnson, Jr.

Comments on "The mycoflora of woodchips....." by

Helana S. Hoover-Litty

This paper is concise, well organized, and amply documented with pertinent information summarized in the form of tables and graphs. The data, although requiring considerable space, are indispensable. However, table IV (p. 18-23) is poorly reproduced (in this copy), and many genus and species names are unclear. Unless the original copy submitted is more satisfactory, this table may need to be retyped. Figs. 1-6 (graphs, p. 24-29) are numbered and identified on the reverse of the pages bearing the graphs, and each figure also has its corresponding legend on a separate page immediately preceeding the graphs to which it refers.

B. Lowy

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

30 March 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernard:

Would you be willing to review the enclosed manuscript for MYCOLOGIA?

"A method for production of Volvariella bombycina fruit bodies in culture"
by S. L. Flegler

If so, please prepare two copies of your comments, one an unsigned copy that can be sent to the author (reviewers who prefer not to remain anonymous may sign both copies of their appraisal). Minor corrections and suggestions may be placed directly on the manuscript using a soft lead pencil.

The following points should be considered in your review:

1. Is the material new and worthy of publication in MYCOLOGIA?

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6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated. Kindly return the paper with your comments promptly.

Should you anticipate not being able to review this paper within two or three weeks, of if you prefer not to review it at all, please return the manuscript to me without delay so that it can be sent to another reviewer.

Thank you for your assistance.

Sincerely yours,

Terry

T.W. Johnson, Jr.

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

6-IV-1981

Dear Terry,

Apart from this being a fairly ho-hum type of notice, I can find no other reason for rejecting it.

Perhaps papers like this might be included under a special heading: Trivia.

Bernard

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

March 23, 1981

Memo: Ruling on publication; Kiefer manuscript

To: Editorial Board, MYCOLOGIA

The enclosed manuscript, submitted for publication, is in the nature of a "letter to the editor" response to a response. I am asking the Editorial Board to rule on the disposition of the paper. You will note that three publications on the matter have already appeared in the journal.

While I seek your collective decision (and will of course abide by it), I cannot refrain from making my views known. I think MYCOLOGIA must not be put in the position of serving "letters to the editor" type of communication, and I regard Kiefer's manuscript as such a "letter." Although a precedent has been set in this instance, I believe it proper to suggest that his response be submitted to the NEWSLETTER.

I have acknowledged receipt of the manuscript and informed Dr. Kiefer that I am seeking a ruling by the Board. The change of editors would seem to "allow" such a response.

Please let me know promptly how you think this manuscript should be handled. I enclose a postcard for your convenience. It is not necessary for you to return the manuscript copy; please destroy it.

Thank you very much.

Terry

T.W. Johnson, Jr.

25-III-1981

Dear Terry:

I strongly support your position to the effect that Dr. Kiefer's comments (or others of a similar nature) do not belong in MYCOLOGIA, but may have a place (at the discretion of the Editorial Board) in the MSA NEWSLETTER. This also has a precedent since the Board made a similar decision in the case of Ott vs Smith. Ott's remarks were rejected as unsuitable for MYCOLOGIA, and in 1978 Ott published his remarks in the BOT. MUS. HARVARD UNIV. under the title: Jonathan Ott's rejoinder to Alexander H. Smith." This policy should be continued.

Sincerely,

B. Lowy

Bernard Lowy
Member, Editorial Board
MYCOLOGIA

ON TAXONOMY - ACKNOWLEDGEMENT AND EXPLANATION

J. Kiefer
Department of Statistics
University of California
Berkeley, California 94720

Machol (1980) has responded to the criticism in Kiefer (1975, 1979) of the approach of Machol and Singer (1971, 1977) for assigning a lower-level taxon to one of several higher-level taxa. The reader of the conflicting statements in some of these papers deserves further explanation and, where it can be achieved, agreement. I will refer to the papers as M, K-I, K-II, MS-I, and MS-II, respectively.

I acknowledge the correctness of Machol's statement (M, bottom of p. 1222 - top of p. 1223) that I was incorrect in asserting (p. 375 of K-II) that the MS tables "contain an unbelievably large number of entries that are simply impossible." The convention of MS-I, p. 763, indeed yields the questioned frequency for Gerronema. Its extension to differing specimens of a species (not explicitly described on p. 763 of MS-I, but in accordance with the use of judgment recommended on p. 765) allows one to obtain M's calculation for Ripartites. For the latter, I would have realized that this genus with 5 species could have a fraction .150 of its species (not a multiple of 1/5) with adnate hymenophore, had I checked that 2 of Singer's 4 specimens of R. strigiceps preserved at the Farlow Herbarium had this

characteristic, and that 2 had it on one side. Thus, the calculation of .150 indeed conforms to the MS technique, if one applies it even to fractions of individual specimens in a species. The MS determination of character frequencies was criticized on p. 375 top of K-II because the results cannot readily be checked by another taxonomist, since MS also employed estimates of character frequencies when counts were not at hand and they believed the consequent error "unlikely to be significant". We are not told when such estimates, or the calculation employed for Ripartites, was used.

The method of calculation relating to Gerronema was discussed in K-II, pp. 370-371. Additional explanation may be helpful. The method is described on p. 763, MS-I, with reference to the two subspecies of Galerina, one consisting of 157 species with clamps, the other of 8 without clamps. MS choose 1/2 rather than 157/165 (or some similar fraction) for the genus clamp frequency. Although, as MS states, species counts are dependent on unequal levels of floristic exploration, the 8 and 157 are perhaps somewhat representative of relative population sizes in the two subgenera. A random species of Galerina may thus have chance closer to 157/165 than to 1/2 of having clamps. To me this is more persuasive for using 157/165 than are the several arguments MS gives for using 1/2 (and which justify the Gerronema computation), especially in the context of the probabilistic computations they employ. Even if one thinks in terms of the

subgenera, one should weigh their sizes in computing genus frequencies. But M is right in stating that the frequencies I called "impossible" are correct according to the MS rules (extended for Ripartites as described above), and I apologize for the error.

The rest of my criticism stands. M states that K-II is "an extraordinary polemic" that contains "many ad hominem comments" and attributions to MS of "remarkable assertions they did not make"; that statistical independence is not assumed by them; that my discussion of some of the history of numerical taxonomy, that MS does not mention but which coincides or contrasts with aspects of their own developments, is "irrelevant"; that my comments are "technical quibbles". It would serve no purpose for me to refute M's assessment with repetition of my earlier explanation. Let the reader decide.

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LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
College of Arts and Sciences

DEPARTMENT OF BOTANY

23-III-1981

Dr. Gilbert C. Hughes
Book Review Editor, MYCOLOGIA
Department of Botany
University of British Columbia
Vancouver, B. C., Canada V6T 2B1

Dear Dr. Hughes:

Thank you for your letter concerning my review of Wasson's book. I have found a few errors in the typescript, as follows:

p.1, par. 2, line7 : octogenarian misspelled

p.4, line 11 : quotation marks to be inserted after "Poetry," before semicolon

p.4, line 13 : quotation marks to be inserted after "weeps," before semicolon

p.4, line 15 : quotation marks to be inserted after "Mapas," before semicolon

p.4, line 17 : quotation marks to be inserted after "entheogens," before semicolon

p.4, line 21 : delete comma after "Highlands"

Yes, I would appreciate receiving a reprint order form.

Sincerely yours,

B. Lowy

Bernard Lowy

MYCOLOGIA
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GILBERT C. HUGHES
Book Review Editor
Department of Botany
University of British Columbia
Vancouver, B. C., Canada V6T 2B1

18 March 1981

Prof. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, Louisiana 70803 USA

Dear Prof. Lowy:

This is to inform you that your review of R. Gordon Wasson's THE WONDROUS MUSHROOM has been accepted for publication in Mycologia. The edited copy of the review will be sent along to the Editor-in-Chief within the next few days and should appear in the July/August, 1981, issue of MYCOLOGIA. Your review will be published as it was received, with the only change being a correction of the price for the paperback edition. I have now received a copy of the book from the publishers along with the request that the volume be reviewed in MYCOLOGIA so there is no need to delay longer on that account.

Proofs of Book Reviews are not sent to authors any more but are corrected in my office, so you will not be seeing the review again until it is in print. If you wish to order reprints of the review, please let me know and I shall send you a reprint order form.

Sincerely yours,

GC Hughes

Gilbert C. Hughes
Book Review Editor, MYCOLOGIA

GCH:ss
cc: T. W. Johnson, Jr.

19-III-'81 Just called. He wants to NY for Mycologia I 25-VIII-'80
The review for E.B. and knows no reason why it cannot also appear in Mycologia. via Atlanta Boston

The Wondrous Mushroom. Mycolatry in Mesoamerica. R. Gordon Wasson. Deluxe ed. 209pp. Illus. $525; Paperback ed. 188pp. illus. $10.95. McGraw-Hill Book Company. 1980.

In 1957 there appeared the first in a series of ethnomycological studies as notable for their novel and arresting contents and the assiduous interdisciplinary research that made them possible, as for the bookmaker's lavish care that created of each deluxe volume a work of art. The first of these studies, in two volumes, Mushrooms, Russia and History, which focused on the magico-religious use of hallucinogenic mushrooms (mostly of the genus Psilocybe) that were known to some indigenous Mexican cultures since Pre-Columbian times, gained worldwide recognition and pioneered a field of investigation that is still being vigorously pursued.

R. Gordon Wasson, the author of these works was by his own account, as mycophobic some five decades ago as most of his fellow Americans, but he had the early good fortune of being inspired and instructed by his Russian-born wife who "adored mushrooms," and who later collaborated with him in the writing of Mushrooms, Russia and History (1957), a study that provided the initial impetus for ethnomycological research in modern times. Now an octogenarian, Wasson has skillfully utilized the knowledge, pertinacity, enthusiasm and insatiable curiosity that characterize his work, to make the numerous contributions that have placed him in the first rank as the world's leading ethnomycologist.

The current volume (monograph 7) concentrates on "mycolatry" in Mesoamerica where the rediscovery and elucidation of ancient, mushroom-oriented rituals practised in Oaxaca enriched our under-

standing of a way of life cherished by an entire people. Much of the ethnomycological information now extant about the Americas and other parts of the world was gathered by Wasson in the course of his indefatigable inquiries and peripatetic travels.

A more expressive term could not have been found for the subtitle of this book than the neologism "mycolatry" which conveys the sense of awe and reverence that must have been felt by the progenitors of sacred mushroom ceremonies. It takes its place beside the author's "ethnomycology, "mycophilia," and "mycophobia" which have now become words of common parlance. This may not be the fate, however, of still another word that Wasson has now chosen to introduce. In recent years the terms 'hallucinogen, 'psychedelic, 'psychotomimetic,' and others of similar connotation have, in Wasson's opinion, been "vulgarized by hippy abuse." He therefore favors the adoption of 'entheogen' (Gr. God within us) as a more fitting and respectful designation that reflects the "deep cultural resonances evoked" by these substances. Although this is not likely to catch on like wildfire among the hoi polloi, one cannot help but wonder at the author's keen sense of propriety in dealing with the ineffable.

The text is divided into two unequal parts, "The Past" and "The Present," introduced by a 12-page "Prelude" which is a succinct expression of the author's inalterable commitment to learn and transmit to others all that he can about the "entheogenic mushrooms" and "their role in Early Man's religious life." The corpus of his original works is ample testimony to the degree of success he has attained toward the fulfillment of this aim. This is not to say that all of his concepts nor even all of his findings have met with

universal approval. Few in any field could make such a boast. There was, for example, sharp criticism in some quarters of his interpretation of "Soma", but Wasson boldly, and I believe convincingly, defended his thesis.

Of the two chapters that constitute "The Present," "A Velada in Huautla" is a review of the salient features of the 1955-'56 agapès presided over by María Sabina, the Mazatec shameness, at Wasson's behest. The 1956 session was recorded by Wasson and photographed by Richardson, and in 1957 was the source of the Life magazine article which included water colors by Roger Heim of several species of Psilocybe from the Huautla region. This resulted in the awakening of the outside world to events they considered so remarkable that the tide of the curious, the voyeurs, the quick-fix generation of the Western World, soon had to be stemmed by local authorities, and is controlled to the present day. A previously unknown village in Mazatec country became the site of a cultural disaster. Huautla paid a heavy price for its "fame."

It was not until 1974 that a complete velada (1958) was published by Wasson under the title María Sabina and her Mazatec Mushroom Velada with text and casettes. Working with Wasson's tapes, George and Florence Cowan, specialists in the Mazatec language, translated María Sabina's words, the Mazatec appearing on one page, and the Spanish and English in columns on the facing page. María's rhythmic chanting, humming, moaning, hand clapping and thigh slapping, together with her percussive utterances was transcribed in musical terms as faithfully as possible by the

ethnomusicologist Willard Rhodes. The chapter on "Traits of the Mesoamerican Velada..." is a more detailed description and analysis of the velada and its participants, concentrating principally on the characteristics and rôle of the shaman in Mazatec society.

Woven throughout the remaining 9 chapters of the text ("The Past") are themes that Wasson and others have treated before, but here most of them are amplified: there is a reinterpretation of Xochipilli, the "Prince of Flowers," imprinted with emblems of hallucinogenic plants and Psilocybe, considered in greater depth than before; much that is new is offered in a chapter on "The Flowers in Pre-Conquest Nahuatl Poetry;" "The Inebriating Drinks of the Nahua" is introduced with these words from a Nahuatl poem: "I have drunk the wine of mushrooms and my heart weeps;" old and new observations are to be found in an illuminating chapter on "Codices, Lienzos, Mapas;" "Piltzintli Child God of the Nahua, and his Christian Progeny" is a beautifully illustrated historical interpretation of the "god of the miraculous entheogens;" there is also a discussion of " Teotihuacán and the Wondrous Mushroom" as seen in Mesoamerican murals, ceramics, and stonework, and a Greco-Mexican link binding the Old World with the New in entheogenic worship; "The Mushrooms Stones of the Maya Highlands," fortifies the argument for the ecstatic symbolism expressed in these Maya sculptures; in a consideration of "The Historical Record" one finds references in Fray Diego Durán and Bernardino de Sahagún to the use of inebriating mushrooms in pre-Columbian America, with overtones of the origin of mycophobia there; and in

the final chapter there is a statement concerning certain shamanistic arcana, concluding with the recent discovery among the Quiche Maya of their ancient belief expressed in the "Popol Vuh" of the sacred link between the lightning bolt and Amanita muscaria.

But these chapters cannot be adequately treated in such spartan fashion! Each of them overflows with new ideas, intrepretations, translations, and nuances previously missed (as in the chapter on "Flowers in Pre-Conquest Nahuatl Poetry"). All the arguments presented tend to confirm the major thesis of this book, that Mesoamericans in pre-Columbian times were steeped in an ageless traditional knowledge of the all-encompassing, soul-revealing, still secret, numinous and ineffable entheogens that dominated their religious lives. There is sure to be controversy. It cannot be avoided in the consideration of so complex a system of beliefs, but the insightful treatment offered here will stand as a monument to the author's deep conviction that the tantalizing palimpsest of pre-Columbian Mesoamerican thought has at least been partly unravelled and made whole.

Wasson's sagacious choice of highly qualified collaborators and informants, whether mycologists (this volume is dedicated to the memory of the late Roger Heim), linguists, archeologists, anthropologists or musicologists, has contributed immensely to the success of his labours, and the author expresses his "boundless gratitude" to all the specialists who have made his work possible.

No detail has been overlooked in the tasteful design and meticulous execution of the limited deluxe edition of 501 copies,

of thich only 475 are commercially available. All the bookmaker's talents have been marshalled to make this a brilliant achievement. Its cost is correspondingly high, but if gold-dusted special paper, green leather spine, and a light ash-colored cloth cover made by Veronese craftsmen in a 12 1/4" X 8 3/4" format are not considered to be de riguer, then the sturdy paperback will be found equally serviceable, if not quite as aesthetically pleasing. This is a remarkable book, a substantial and memorable contribution to ethnomycology.

B. Lowy

DEPARTMENT OF BOTANY

DUKE UNIVERSITY
DURHAM, NC 27706
919-684-3715

2 March 1981

Bernard,

This letter explains my action regarding your review of Wasson's book, and I am pleased that the matter can be resolved.

It is an excellent review, and will be accepted in its entirety, but I hope you will give thought to my reaction to the first two paragraphs — they left

me feeling very "impatient" to get on with the specifics about the book at hand.

Anyway, I wanted you to know personally and directly that I was involved in resolving the difficulty.

Warmest regards.

Terry

JOHNSON

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

March 2, 1981

Dr. Gilbert C. Hughes
Book Review Editor
MYCOLOGIA
Department of Botany
University of British Columbia
Vancouver, B.C., Canada V6T 2B1

Dear Gil:

Thank you very much for your letter of 20th February, and the copy of Dr. Lowy's review of Wasson's THE WONDROUS MUSHROOM.

Nothing in your letter changes my support for the BRE policies. I agree that in this instance the Wasson book is an "exceptional" one, and I favor accepting Dr. Lowy's review, with two provisos. First, I think you must inform the publisher that you have the review and plan to publish it, even though unsolicited. The publisher should be given reasonable time to respond, and if their reply is negative, you should not go further. Secondly, I believe this must be a one-time instance, and in any future cases, unsolicited reviews should be returned to the author promptly (with suitable explanation).

The Editorial Board may wish to change BRE policies, and they will be given opportunity to do so when we meet in Indiana in August. I hope you can be there to state your views, or at least provide me with a written response. Personally, I see no reason to modify current policies, and I believe the Book Review Editor must be allowed to operate within those policies in a responsible fashion.

I have taken the liberty of reading Dr. Lowy's review, and am returning the copy. There are two or three places where it could be condensed, and I have so marked these. Chiefly, I believe the first two paragraphs are not necessary; the review would be best started at the third paragraph. I read the review as an interested mycologist, familiar with Wasson's record. I found the first two paragraphs to be unnecessary, and they really seemed to me to "interfere" with getting right to the subject, namely, what this new book is all about. As a reader, I don't want to know the history; I want to be informed about the volume at hand. The first half of the first paragraph on page 4 could also be deleted, I think.

I recommend that you accept the review, and I hope that Dr. Lowy will condense it somewhat. If he chooses not to do so, I would accept the account anyway, since he certainly is more authoritative on Wasson's work

than I. It is essential, however, that the publisher be informed before anything appears in print.

Thank you for explaining the situation. I am sure Dr. Lowy understands, and recognizes the logic of your position.

Kindest regards.

Sincerely yours,

T.W. Johnson, Jr.

cc: Dr. Bernard Lowy

Hughes

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GILBERT C. HUGHES
Book Review Editor
Department of Botany
University of British Columbia
Vancouver, B. C., Canada V6T 2B1

20 February 1981

Dr. B. Lowy
Department of Botany
Louisian State University
Baton Rouge, Louisiana 70803 USA

Dear Prof. Lowy:

In response to your letter ofr 27 January, I have sent a copy of your review of THE WONDROUS MUSHROOM by R. Gordon Wasson to Terry Johnson. After he has had a chance to read the review I shall discuss it with him and we'll reach a decision as to whether or not it will be published as it stands. I shall let you know of our decision just as soon as possible after discussing the review with Prof. Johnson.

Sincerely yours,

G.C. Hughes

Gilbert C. Hughes
Book Review Editor, MYCOLOGIA

GCH:ss

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CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

February 2, 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge LA 70803

Dear Bernard:

You now have Dr. Hughes' response to your queries about the book reviews you submitted. It appears to me that a combination of factors may have contributed to the delay.

Of course, Dr. Hughes is quite correct in his interpretation of and adherence to policies of the office of Book Review Editor. I am sure they are sound policies, designed to avoid duplication of effort, excess use of printing space (costly, at best), and to prevent difficulties arising with publishers. While I do not control or make those policies (both Dr. Hughes and myself have inherited them), I do agree with them, and support Dr. Hughes. Of course I am responsible for recommending changes in policy as they may seem necessary, but only the Editorial Board may set policy, in the final analysis. Certainly because of possible legal implications (of which Dr. Hughes and myself are understandably ignorant), it is mandatory that reviews be requested by publishers in the first instance. Dr. Hughes is quite correct in insisting that books come to him from publishers with a request for a review, and I think for sake of his own time and sanity, must not involve his office in soliciting books for review.

I am sorry I was not aware of the circumstances when you called me, or I could have stated all this over the 'phone. Dr. Hughes is charged with operating his office within guidelines and policies, and since I see no conflict in this instance, I support his decisions fully. He is expected to consult with me as problems arise (particularly ones that could develop as a result of unfavorable reviews), but beyond that, he must be allowed freedom of decision in matters of operation.

I am sure you understand the situation, and the soundness of Dr. Hughes' decisions. As a respected member of the Editorial Board, you are certainly urged to recommend changes in editorial policies, and I assure you I will see that the Board gives any such suggestions prompt and thorough consideration. Thank you very much for understanding.

Warmest regards.

Sincerely yours,

Terry

T.W. Johnson, Jr.

I phoned Terry on 3-III-81. My review has been accepted with recommendations for deletion, to be decided upon by Hughes. His response should reach me soon.

cc: Dr. G.C. Hughes

complete copy

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

3
27-I-1981

Dr. Gilbert C. Hughes
Department of Botany
The University of British Columbia
2075 Wesbrook Mall
Vancouver, B.C., Canada

Dear Dr. Hughes:

I am glad that the various inquiries that I have made concerning my review of Wasson's new book have finally reached you, and I would like to respond to some of the points in your letter.

I fully appreciate your prerogative in your capacity as Book Review Editor to establish the guide lines that determine the policy to be followed in soliciting, accepting, and publishing reviews for MYCOLOGIA. You are quite right in suggesting that without such rules your task would be made much more difficult. Consequently you should have some explanation of the circumstances that led to my submitting the review for your consideration last September.

When Wasson's new book first came to my attention, my acquaintance with his earlier books suggested that this would probably be another major work in ethnomycology. After reading the book I was convinced that "The Wondrous Mushroom" was indeed an outstanding contribution, and that our fellow mycologists would surely wish to be apprised of it. At the time I was also occupied in making arrangements for my departure to Brazil to join a mycological expedition there, but I felt that if it were at all possible I should not delay in reading the book, nor in sending a review to MYCOLOGIA at the earliest possible moment, since I anticipated an absence of approximately four months.

You may recall that I included a Brazilian fowarding address in my letter to you from New York (28-VIII-1980), with the expectation that I would eventually receive an acknowledgement of the receipt of the review. Perhaps it was unrealistic of me to expect that the review should receive rapid attention, and even be accepted for publication, but it would not have been without precedent. In 1969 I submitted a similarly unsolicited review of Wasson's "Soma" to MYCOLOGIA which was published without delay (MYCOLOGIA 61: 849-851. 1969.) However, as you observe in your letter, there is no "hard and fast policy" on this score, so I believed that I was acting in conformity with customary precedure. You also point out that in any event, reviews longer than three typewritten pages

double-spaced, "would only be considered in the case of an exceptional book or reviewer and only after the matter had been discussed with the Editor-in-Chief." As to this being an exceptional book, I believe you would find little disagreement among those qualified to have an opinion, since Wasson has long been one of the world's leading figures in his field. Don Rogers and Alex Smith, both of whom reviewed Wasson's earlier work (in MYCOLOGIA 50:147-149, and 449-452. 1958) could, among many others, testify to this. Concerning the length of a review, I again agree that certain limitations are generally desireable, but that in exceptional cases (of which this is one) the matter should be left to the discretion of the Editors. The question of my competence as a reviewer must of course be left to the judgment of others.

Having said this much, I would like to ask that you discuss the pertinent questions with Dr. Johnson. If the book is considered worthy, and the review appropriate, I would prefer to have it accepted for publication in its entirety. The importance of the work would seem to justify the additional space required.

Sincerely yours,

B. Lowy

Bernard Lowy

cc: Dr. T. W. Johnson, Editor-inChief, MYCOLOGIA.

Dear Terry,

It was good to talk with you the other day. I don't know how many years have passed since we last met!

To follow up our conversation, I am enclosing this copy of my letter to D. Hughes together with a copy of my review. The question raised by D. H. on the "exceptional" nature of the book is a highly pertinent one, and it occurs to me that there is a distinguished faculty member at Duke who could serve as a referee and give you an objective and confidential opinion regarding the stature of the author and his book, and possibly also on my competence to review

He is Dr. Weston La Barre of the Department of Anthropology, who is well acquainted with Wasson and his work, and who in all likelihood has a copy of the book in question.

I'm sorry to add this problem to your already burdonsome editorial duties, but I think you'll take it in stride!

With best regards,

Bernard

THE UNIVERSITY OF BRITISH COLUMBIA
2075 WESBROOK MALL
VANCOUVER, B.C., CANADA
V6T 1W5

DEPARTMENT OF BOTANY

19 January 1981

Dr. Bernard Lowy
Department of Botany
Lousiana State University
Baton Rouge, Louisiana 70803 USA

Dear Prof. Lowy:

I have now tried on three different occasions to reach you by telephone and have been completely unsuccessful each time. I suppose the gods have decreed that it should not be so. At any rate, I am writing this in hopes that the mail service (slow though it is) might be successful.

I do have all of the materials you sent along, beginning with your review for Wasson's Wondrous Mushroom and including the note about the Järva & Parmasto book on the Estonian Fungi. I am sorry to have been so slow in responding to these two reviews, especially the first one. However, as you will see from my comments below there is a reason for this.

I should, perhaps, begin by letting you know something of the editorial policy which pertains to present operations of the Office of the Book Review Editor of Mycologia (incidentally, it doesn't differ substantively from the policy that Prof. Korf followed). Firstly, Mycologia does not normally solicit books for review nor do we publish unsolicited reviews of books. If a publisher does not send a book to me for review, I see no reason why we should feel any obligation to review it. By the same token, reviewers of books that are received are selected by the Editor and take on the reviewing job at the request of the Book Review Editor. This is not a hard and fast policy but in practice is certain to be the best one. The problems created by several different members submitting reviews of the same book, each of them expecting his review to be published, are not the sort I would want to deal with. Another rule that must be followed by reviewers of books relates to the length of reviews. Space restrictions in the journal are such that reviews simply must be restricted to three type- written pages, double-spaced. Longer reviews would only be considered in the case of an exceptional book or reviewer and only after the matter had been discussed with the Editor-in-Chief of the journal. Finally, I should note that items listed in Briefly Noted must also be publications which are submitted for review and that this section is prepared personally by the Book Review Editor and outside reviewers are not used for these short reviews.

In light of the above, your two reviews create something of a problem. Neither of them was solicited for the journal, both of them are reviews of books that have not been submitted for review by their publishers, and, finally, your review of the Wasson book is much too long.

When I first received your Wasson review I wrote a note to the publishers asking if I might have a copy of the book for review purposes but so far

they have not considered it desirable to send me one. Until I do see a copy of the book I cannot accept your review and, in view of its length, the review will require revision even after I have a copy of the book. As for the Järva & Parmasto review, I am willing to include it as a short review but not in the Briefly Noted section. However, I'll not do this until I have seen the volume itself.

I shall be pleased to discuss this matter with you further at your convenience. I hope you can appreciate my position regarding these matters. Although you may well disagree with me on this, I can assure you that I am not prepared to actively solicit books for review in Mycologia nor do I think I should be. If authors and publishers are not interested enough in having a publication reviewed in the journal to send a copy to the Book Review Editor, then I am not going to worry too much about it. After all, the number of publications which are submitted for review is sufficiently large that we don't have to solicit publications for review in Mycologia. Likewise, I am not prepared to accept unsolicited reviews for publication except in special circumstances where I deem such acceptance warranted. Such exceptional circumstances will not occur fruquently. The Book Review Editor's job would be something of a nightmare if he had to deal routinely with unsolicited reviews from mycologists who felt a particular book should be reviewed, with reviews of books which have not been submitted for review by their publishers, and at the same time with the large number of publications which are submitted for review each year.

I am enclosing your second copy of the Wasson review so that you may revise it. I am still hoping that the publisher will send along a copy of the book (the paperback edition) and we will be able to use your review in a shortened form.

Sincerely yours,

GC Hughes

Gilbert C. Hughes
Book Review Editor
Mycologia

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B. Lowy
Botany Dept LSU

Street and No. — Rue et no.
Baton Rouge, LA 70803

City, State and Zip Code — Localite

UNITED STATES OF AMERICA — *Etats-Unis d'Amérique*

PS Form 2865, Sept. 1975

Hughes / Johnson

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

19-I-1981

Dr. T. W. Johnson
Department of Botany
Duke University
Durham, N. C. 27706

[Tel. 919 - 684 - 2870]

Dear Terry:

More than 4 months ago I sent to Dr. G. C. Hughes my review of R.G.Wasson's new book "The Wondrous Mushroom," a major contribution to ethnomycology. Since I was then about to leave for Brazil, I gave Dr. Hughes an address where he might have sent an acknowledgement of the receipt of my manuscript. Now, over a month after my return, I still have received no response although in the interim I have written to him and phoned him twice without success in reaching him.

I cannot guess Dr. Hughes's motive in ignoring my inquiries so long, but I now believe that any further attempt on my part to communicate with him would be futile, so I seek your good offices as Editor-in-Chief in trying to clarify whatever problem may exist. If Dr. Hughes received my manuscript it would be merely a routine courtesy (not to mention a professional duty) for him to acknowledge it.

I would greatly appreciate your kindness in inquiring into this incongruous situation both for my own satisfaction as well as for the assurance that others who may submit reviews to Dr. Hughes in future will not suffer similar neglect.

Sincerely yours,

Bernard

Bernard Lowy
Professor Emeritus of Botany

P.S. Copies of my notes to Dr. Hughes are enclosed.

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

9-I-1981

Dear Dr. Hughes,

We appear to be unable to communicate with each other by phone, so I am sending you another note simply to ask whether you received my review of Wasson's "Wondrous Mushroom" which I sent to you last September from New York.

I am also enclosing a copy of my review together with copies of my previous notes to you, in the event that these have not reached you.

Please be good enough to clarify this minor mystery.

Sincerely yours,

B. Lowy

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FROM B. Lowy
Botany Dept
B.R. La 70803

TO Dr. D.C. Hughes
Botany Univ. of British Columbia
Vancouver B.C. Canada

PS Form 3806 Jan. 1977

☆ GPO: 1977 — 237 - 902

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

Dr. Gilbert C. Hughes
Botany Department
University of British Columbia
Vancouver, B.C.
Canada V6T 1W5

5-XII-1980

Dear Dr. Hughes:

Before leaving New York for Brazil in September I sent you a review of Wasson's latest book "The Wondrous Mushroom," asking that you consider it for publication in MYCOLOGIA. I have just returned to the university and would like to know whether my review reached you, and if so, whether you plan to publish it. I would very much appreciate hearing from you about this.

Sincerely yours,

B. Lowy

Bernard Lowy

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

25-VIII-1980

Dr. Gilbert C. Hughes
Botany Department
University of British Columbia
Vancouver, B.C.
Canada V6T 1W5

Dear Dr Hughes:

Before leaving Baton Rouge to join a Projecto Flora Amazonica expedition in Brazil, I received R. Gordon Wasson's latest book, "The Wondrous Mushroom," and had the opportunity to read it and write the review I have enclosed for your consideration as a contribution to MYCOLOGIA.

It is an important work which I believe will interest many MSA members, and I hope that you may find my review suitable for our journal.

I expect to return to LSU in late December or early January, 1981.

Sincerely yours,

B. Lowy

Bernard Lowy
Professor Emeritus of Botany

PS - My headquarters address in Brazil after September 3 will be:

INPA - Botanica
Caixa Postal 478
Estrada do Aleixo
69.000 Manaus, Amazonas
Brasil

I plan to be at the New York Botanical Garden to confer with Dr. Ghillean T. Prance on September 2.

mailed: 7-I-81
received: 12-I-81

JON.

Dec. 6, 1981

Dr. Bernie Lowy
Department of Botany
Louisiana State University
Baton Rouge, Louisiana 70803

R. Bandoni
THE UNIVERSITY OF BRITISH COLUMBIA
DEPARTMENT OF BOTANY
3529-6270 UNIVERSITY BLVD.
VANCOUVER B.C., V6T 2B1

Dear Bernie:

I was quite surprised to receive such a prompt reply to my inquiry -- and from you. I thought that you would still be in South America! Thank you for the information on Olive's collections -- or the lack thereof. I had an idea that this might be the case with the specimens, but Olive was not very consistent about deposition of collections. I guess I should have written directly to him to start with.

I asked Gil Hughes about the book review. He indicated that nothing could be done with the review until he had recieved a copy of the book.

I hope that the collecting in the Amazon basin was productive -- best wishes for 1981.

Best regards,

R. Bandoni

Bandoni – Hughes

THE UNIVERSITY OF BRITISH COLUMBIA
2075 WESBROOK MALL
VANCOUVER, B.C., CANADA
V6T 1W5

DEPARTMENT OF BOTANY

Curator, The Herbarium
Department of Botany
Louisiana State University
Baton Rouge, Louisiana 70808

Dec. 11, 1980

Dear sir:

In connection with taxonomic studies of lower basidiomycetes, I would very much like to examine some of L. S. Olive's collections. These are as follows:

Tremella mycophaga Martin var. obscura Olive. Mycologia 38: 540. 1946. Collected Univ. of Georgia Campus, Athens, Ga. October 23, 25, 28, 1945.

T. mycophaga var. obscura. On Dacrymyces deliquescens, Athens, Georgia. Fall, 1945-46. Mycologia 39: 94. 1947.

T. mycophaga var. obscura. On Dacryomitra stipitata, Colyell Bay, La. March 9, 1947. Mycologia 40: 593. 1948.

The last two papers referred to were published while Dr. Olive was at Louisiana State University and I assume that he deposited the specimens there (no mention is made of this in the papers). Would it be possible for me to obtain a loan of any of the above that you do have?

Thank you for any help that you can give.

Sincerely yous,

R. J. Bandoni
Prof.

[604-2282-133]
BOT. DEPT. UNIV. BR. C.

5-I-'81 Phoned - Hughes out; to return call.
AM
2 PM – "

R. Bandoni
THE UNIVERSITY OF BRITISH COLUMBIA
DEPARTMENT OF BOTANY
3529-6270 UNIVERSITY BLVD.
VANCOUVER B.C., V6T 2B1

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

17-XII-1980

Dear Bob:

When Lindsay Olive left LSU (circa 1950) he left nothing in the herbarium. In fact there was no mycological herbarium when I first came here in 1951, so I assume that he retained all his collections which he may still have with him in Chapel Hill.

In spite of my official retirement I still retain my lab and office because I plan to continue working on my collections, but not teaching. I have just returned from 3 months in Amazonian Brazil and await the fowarding of my collections.

With best regards,

Bernice

B. Lowy

P.S. - I wonder whether you would make a discrete inquiry for me from a colleague of yours. Before I left for Brazil I sent Dr. G.C. Hughes (now Book Review Editor of MYCOLOGIA) a review of Wasson's new book, "The Wondrous Mushroom." I wrote to him again on 5-XII asking whether he received my review and if so whether he plans to publish it. I have not heard from him to date. Perhaps my letters (and review) never reached him. Would you be good enough to ask him about it?

MYCOLOGIA
OFFICIAL JOURNAL OF THE
MYCOLOGICAL SOCIETY OF AMERICA
PUBLISHED BY
THE NEW YORK BOTANICAL GARDEN

T.W. JOHNSON, JR.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

15 March 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernard:

Would you be willing to review the enclosed manuscript for MYCOLOGIA?

"Taxonomic position of the genus Chlorolepiota" by A. V. Sathe and S. Deshpande.

If so, please prepare two copies of your comments, one an unsigned copy that can be sent to the author (reviewers who prefer not to remain anonymous may sign both copies of their appraisal). Minor corrections and suggestions may be placed directly on the manuscript using a soft lead pencil.

The following points should be considered in your review:

1. Is the material new and worthy of publication in MYCOLOGIA?

2. Is the presentation clear and logical?

3. Are the conclusions justified by the evidence?

4. Could the paper be shortened substantially without interfering with its merit?

5. Have the tables been prepared as clearly and concisely as possible? Could any be deleted? Added?

6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated. Kindly return the paper with your comments promptly.

Should you anticipate not being able to review this paper within two or three weeks, of if you prefer not to review it at all, please return the manuscript to me without delay so that it can be sent to another reviewer.

Thank you for your assistance.

Neither author is a member of MSA, so this paper will have to be very good if it is finally to be accepted. I really need your expert advice on this one.

Sincerely yours,

Terry

T.W. Johnson, Jr.

LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
College of Arts and Sciences

DEPARTMENT OF BOTANY

20-III-1981

Dr. Terry W. Johnson, Jr.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

Dear Terry:

I am returning the manuscript you sent me, having corrected some of the more glaring errors in spelling, grammar, and punctuation. The authors certainly did a careless job on this short paper!

I do not feel sufficiently competent to judge the validity of the new family status proposed, but it is my impression that the evidence is flimsy. The photo is not of the best quality, but it looks to me a great deal like fig. 4, pl. 27 (Singer's 3rd ed.) which shows Pleurotus levis with "intermixed to irregular" trama.

I would suggest sending this to Howard Bigelow, Harry Thiers, or better still, to Rolf Singer for a valid appraisal from a practicing agaricologist.

Sincerely,

Bernard

B. Lowy

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Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

10 March 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernard:

Would you be willing to review the enclosed manuscript for MYCOLOGIA?

"Agaricostilbum: a basidiomycete" by J. E. Wright, et al.

If so, please prepare two copies of your comments, one an unsigned copy that can be sent to the author (reviewers who prefer not to remain anonymous may sign both copies of their appraisal). Minor corrections and suggestions may be placed directly on the manuscript using a soft lead pencil.

The following points should be considered in your review:

1. Is the material new and worthy of publication in MYCOLOGIA?

2. Is the presentation clear and logical?

3. Are the conclusions justified by the evidence?

4. Could the paper be shortened substantially without interfering with its merit?

5. Have the tables been prepared as clearly and concisely as possible? Could any be deleted? Added?

6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated. Kindly return the paper with your comments promptly.

Should you anticipate not being able to review this paper within two or three weeks, of if you prefer not to review it at all, please return the manuscript to me without delay so that it can be sent to another reviewer.

Thank you for your assistance.

Sincerely yours,

Terry

T.W. Johnson, Jr.

Comments on "Agaricostilbum...." by J. E. Wright et al.

This study is a significant contribution to our knowledge of a few still poorly understood genera. Whether Pilacrella and Hoehnelomyces of the Phleogenaceae are distinct may be debatable, but I believe that at least one species of Pilacrella belongs in Hoehnelomyces (Flora Neotropica Monograph 6, 1971. p. 45). A further discussion of the systematic position of these genera would be desirable but I am not suggesting that this paper be emended to include it.

The authors have presented a well documented analysis of the genus Agaricostilbum and have justified placing it in the Heterobasidiomycetes.

B. Lowy

Comments on "Agaricostilbum...." by J. E. Wright et al.

This study is a significant contribution to our knowledge of a few still poorly understood genera. Whether Pilacrella and Hoehnelomyces of the Phleogenaceae are distinct may be debatable, but I believe that at least one species of Pilacrella belongs in Hoehnelomyces (Flora Neotropica Monograph 6, 1971. p. 45). A further discussion of the systematic position of these genera would be desirable but I am not suggesting that this paper be emended to include it.

The authors have presented a well documented analysis of the genus Agaricostilbum and have justified placing it in the Heterobasidiomycetes.

B. Lowy

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

20-II-1981

Dr. T.W.Johnson, Jr.
Department of Botany
Duke University
Durham, N.C. 27706

Dear Terry:

I recommend the following for membership on the Editorial Board, to replace those whose terms expire in 1981.

G. C. Carroll, Dep't. of Biology, University of Oregon, Eugene, OR 97403.

I. K. Ross, Dep't. of Biological Sci., University of California, Santa Barbara, CA 93106.

I would support the policy of requesting authors to provide "key words" or "descriptors" to accompany their articles as a means of facilitating retrieval from indexing sources.

Sincerely,

Bernard

B. ~~Lowy~~

I phoned Terry on 3-III-'81.

MYCOLOGIA
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T.W. JOHNSON, JR.
Editor-in-Chief
Department of Botany
Duke University
Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

February 16, 1981

MEMO: Editorial Board Members

TO: Editorial Board and Managing Editor

At the Indiana meeting in August, proposals for new members of the Editorial Board will have to be made. At the end of 1981, Darrell Weber and Henry Aldrich will retire from the board, leaving two vacancies.

It is not too early to ask you to submit to me names of persons you would recommend for membership on the Board. I will be grateful if you will suggest names to me as soon as possible. Of course members retiring may be considered for an additional five-year term on the Board.

In my experience with the journal thus far, I seem to be over-working Board members in the areas of physiology and biochemistry and in ultrastructure, and I suggest that these might be priority needs. It happens that the retiring members are in these areas of expertise.

A second item needs action from the Board.

A former editor of MYCOLOGIA recommends that the journal adopt the policy of requiring authors to provide "key words" or "descriptors" to be printed along with the article. This would improve retrievability when indexing sources (such as Current Contents) are used. If such a policy is to be established, the time to do it is with the 1982 volume, when we have a new format and printer.

I would appreciate having your views on this proposal.

Best wishes.

Terry

T.W. Johnson, Jr.

The Mycological Society of America

FOUNDED DECEMBER, 1931

Office of the Secretary-Treasurer
Department of Botany
University of Rhode Island
Kingston, RI 02881
Telephone: (401) 792-2161

February 5, 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
Baton Rouge, LA 70803

Dear Bernie:

I think I can respond to the questions in your letter of January 28.

Emeritus Status requires approval by the Council, but you should easily qualify for it, so the official action is only a matter of formality. I will interpret your letter as a request to be put on the list of names of individuals requesting Emeritus Status, and this will be voted on at the Annual Meeting.

A check of the records shows that I have not received paym ent from you for 1981 dues. I will enclose a dues notice for your convenience, and I think I could justify accepting you as an Emeritus Member, pending the formal approval by the Council.

Your Brazilian material sounds very interesting. This job is keeping me away from the fungi temporarily, but I hope soon to find some time to get back to work.

Nice to hear from you, and best personal regards.

Sincerely yours,

Roger

Roger D. Goos
Secretary-Treasurer

Goos

Department of Botany
LOUISIANA STATE UNIVERSITY AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE · LOUISIANA · 70803
504/388-8485

28-I-1981

Dr. Roger Goos
Department of Botany
University of Rhode Island
Kingston, RI 02881

Dear Roger:

I am not sure that I should be directing this note to you or to someone else in the Society, so in the event that this does not come within your jurisdiction, I would appreciate your kindness in fowarding it to the "jefe" in charge.

Since July, 1980, when my retirement from LSU went into effect, I have had emeritus status, and consequently would like to take advantage of the privilege of receiving MYCOLOGIA at the reduced rate. I have tried unsuccessfully to find the receipt of the check for my membership dues, but I think I am up to date on that score. If not, the Society records will show it.

I recently returned from a Projecto Flora Amazonica expedition in Brazil, and have a lot of fungi to work on. The university has allowed me to keep both my office and lab space, so I am working here as usual, except that I have no classes to meet, a distinct time-saving advantage!

With best regards, as always,

Bernie
B. Lowy

Dr. Lowy
Dr. Shearer

MYCOLOGIA

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Durham, North Carolina 27706

CLARK T. ROGERSON
Managing Editor
The New York Botanical Garden
Bronx, New York 10458

5 February 1981

MEMO: Review of manuscript: Falck and Olson

TO : Editorial Board

We have received an unuaual manuscript for publication, and I need your advice in deciding on its disposition.

Would you please look at the enclosed copy "Plasmodiophora brassicae for teaching", and let me know whether or not the article should be considered for publication in MYCOLOGIA? I am troubled particularly by the extensive illustrations, and the little that is used of them in the presentation itself. There is, of course, the broader question of whether we wish to open the doors to strictly "teaching methodology".

Please send the manuscript, this memo, and the postcard to the addressee on the enclosed envelope (one other member of the Board). The manuscript need not be returned.

I enclose a postcard which you may use to respond. It would be helpful if you would cite major reason(s) for your decision.

Thank you very much.

Terry
T. W. Johnson, Jr.

Dr. Lowy

11-II-1981

Dear Terry:

I do not doubt that the Falck-Olson paper for demonstrating the life history of P. brassicae is valid and useful but do not believe that MYCOLOGIA is the appropriate place to publish it because: 1) This is not a research paper. 2) No new information about the organism is presented. This is merely a laboratory method for demonstrating it. 3) No new conclusions are drawn. 4) The format is ill suited to the journal. This belongs in a lab manual or in a publication such as the "Mycology Guidebook."

Sincerely,

Bernard

B. Lowy
Editorial Board MYCOLOGIA

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January 23, 1981

Dr. Gilbert C. Hughes, Book Review Editor
MYCOLOGIA
Department of Botany
University of British Columbia
Vancouver, B.C., Canada V6T 1W5

Dear Gil:

Yesterday I received a letter from Dr. Bernard Lowy regarding a book review which he sent to you but was not acknowledged. He asked me to contact you about the matter, feeling that some acknowledgment is in order.

I am sure that there must be some logical and reasonable explanation, for I have known you to be prompt and dependable in all of our dealings. If memory serves me correctly, Dr. Lowy's review would have reached Canada during the postal strike there. That being the case, I am not surprised for the delay, since I experienced a delay of two months in some of my correspondence to Canadian colleagues. Indeed, some correspondence sent by me to Canada never reached the addressee, and some replies to me never appeared here.

In any case, if you have not already done so, please contact Dr. Lowy. I am sure he would be very grateful and would appreciate knowing of the circumstances. If you think it appropriate, please let me know how this incident is resolved.

On another matter, I should remind you that manuscripts for the May/June, 1981, issue will be shipped off to Lancaster Press on or about the 30th of this month. The July/August manuscripts will be sent during the last week in March. Send any book reviews along.

Kindest regards.

Sincerely yours,

T.W. Johnson, Jr.