Hunt Institute for Botanical Documentation
5th Floor, Hunt Library
Carnegie Mellon University
4909 Frew Street
Pittsburgh, PA 15213-3890
Telephone: 412-268-2434
Email: huntinst@andrew.cmu.edu
Web site: www.huntbotanical.org

The Hunt Institute is committed to making its collections accessible for research. We are pleased to offer this digitized item.

Usage guidelines
We have provided this low-resolution, digitized version for research purposes. To inquire about publishing any images from this item, please contact the Institute.

Statement on harmful and offensive content
The Hunt Institute Archives contains hundreds of thousands of pages of historical content, writing and images, created by thousands of individuals connected to the botanical sciences. Due to the wide range of time and social context in which these materials were created, some of the collections contain material that reflect outdated, biased, offensive and possibly violent views, opinions and actions. The Hunt Institute for Botanical Documentation does not endorse the views expressed in these materials, which are inconsistent with our dedication to creating an inclusive, accessible and anti-discriminatory research environment. Archival records are historical documents, and the Hunt Institute keeps such records unaltered to maintain their integrity and to foster accountability for the actions and views of the collections' creators.

Many of the historical collections in the Hunt Institute Archives contain personal correspondence, notes, recollections and opinions, which may contain language, ideas or stereotypes that are offensive or harmful to others. These collections are maintained as records of the individuals involved and do not reflect the views or values of the Hunt Institute for Botanical Documentation or those of Carnegie Mellon University.

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

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

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

CLARK T. ROGERSON
Managing Editor
The New York Botanical
Bronx, New York 1045

26 September 1979

To: Members of the Editorial Board of MYCOLOGIA

From: M. E. Barr Bigelow

During the AIBS meetings in Stillwater, the Council of the Mycological Society of America took some actions that should be reported to members:

Terry Johnson was approved as new Editor-in-Chief of MYCOLOGIA. The New York Botanical Garden subsequently approved their choice. Terry will take over duties with the 1981 volume, probably in late June 1980. I will complete the 1980 volume, marking manuscripts for the printer and reading gal and page proofs for those issues during 1980.

The Council also approved additions to the Editorial Board: K. Wells for a second 5-year term; authorized asking O. R. Collins and L. K. Weresub to serve. Both have agreed to serve, and I am happy to welcome them to the Editorial Board. As an alternate if necessary, the Council also approved Carol Shearer. I will present her name next year when we again consider additional members.

A meeting of Clark Rogerson, Terry Johnson and myself was held. We considered a range of topics, including those brought out in your letters. No action could be taken of course, but these are summarized for your consideration.

1) Book-review editor: suggested are E. Butler, G. Carroll, L. Kohn, L. Weresub. We should have a definite opinion to present to the Council in 1980.

2) The question of providing a list of publications with biographies of deceased members gave 3 supporting, 1 against, 1 at author's discretion. What is the consensus of the Board?

3) On the question of publication of the "Whisler report," all who spoke for publication were agreed on the Newsletter, rather than the journal.

4) Concerning Brief Articles, the majority of responses gave: no limit on number per issue but the Editor should be strict about 4(5)-page limit in length.

5) A question arose about present policy on publication by nonmembers, i.e. following approval by a majority of members of the Board. With projected full membership of 15, there could be a long delay. One suggestion was approval by a small number of members - 3? Could I have opinions here?

6) Suggestions on Literature cited: a) For capitalization of important words in book titles. We suggest that the journal should continue to follow CBE Style Manual in not capitalizing.

b) Reference citations by author/year or by number. We suggest this should be left as it is, to choice of author.

7) The question of monthly vs. bimonthly issues brought out that more money as well as more assistance to both editor and managing editor would be required to produce monthly issues. Unless the number of pages per year were to be increased, there would not seem to be any advantage to monthly issues. (During the past 4 years, there has not been a large backlog of articles, nor a long delay after acceptance before publication.)

EESTI SEENTE KOONDNIMESTIK. (LIST OF ESTONIAN FUNGI), by L. Järva & E. Parmasto. Institute of Zoology and Botany, Academy of Sciences of the Estonian S.S.R. Tartu. 1980. 331 p. Price, 2 rubles 10 kopeks (paperbound).

For 15 years Drs. Järva and Parmasto and numerous of their colleagues collaborated in producing the first complete list of fungi (3277 species) known to occur in Estonia. The record covers a 200 year period from 1777 to the present, and the data appear in 2 parts. 1) A taxonomic section alphabetically records the genus and species of fungus, followed by references in the literature to its occurrence and to other pertinent information concerning it. 2) A host index with appropriate literature citations. There are 1192 entries in the bibliography. This compendium should serve as an important guide for the phytopathologist who has a need for these significant data from the Estonian S.S.R.

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

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.

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

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.

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

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.

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

12-I-1981

Dear Terry,

Now that you have the burdensome job of Editor, I wish you luck! I'll try to be succinct in my comments on manuscripts submitted to me, and to return them to you as soon as possible after their receipt.

With best wishes,

Bernard

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

5 January 1981

Dr. Bernard Lowy
Department of Botany
Louisiana State University
New Orleans, LA 70803

Dear Bernard:

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

"A new North American species: Physalacria cryptomeriae" by J. Berthier and C. T. Rogerson.

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.

I hope your leave was both profitable and enjoyable. Welcome back.

Sincerely yours,

Terry

T.W. Johnson, Jr.

12-I-1981

Comment on "A New North American Species: Physalacria cryptomeriae"

by J. Berthier & C.T.Rogerson

Apart from the minor manuscript changes I have indicated, I find nothing else to add or detract. The paper is obviously a significant contribution, and the illustrations are excellent.

B. Lowy

MYCOLOGIA

Instructions for Proofs and Page Charges

Proofs: Please read the enclosed proof carefully, making only absolutely necessary changes, and return promptly. Authors 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.

Your manuscript will be discarded upon publication unless you notify us to return it.

Page charges: The enclosed page charge form must be completed and **returned with the galley proofs to:**

Publications Office,
New York Botanical Garden,
Bronx, N. Y. 10458.

DO NOT INCLUDE REPRINT ORDER—see below

Instructions for ordering reprints

Please note that reprints are ordered directly from the printer. Therefore, purchase orders for reprints should be made out to: Lancaster Press, Inc., Lancaster, Pa. 17604. However, indicate below the number of reprints desired as well as billing and shipping instructions. RETURN THIS ORDER FORM WITHIN TEN (10) DAYS to: Lancaster Press, Inc. Please do *not* include page charges on reprint purchase order.

NOTE: Postage charges will be in addition to the prices shown below.

PRICE LIST OF REPRINTS

Copies	1–4 Pages	5–8 Pages	9–12 Pages	13–16 Pages	17–20 Pages	21–24 Pages	25–28 Pages	29–32 Pages	Covers
50	15.10	31.10	46.55	62.25	78.60	93.35	108.75	124.45	16.25
100	17.05	34.00	51.40	68.00	85.35	101.95	119.30	135.95	21.00
Add'l 100's	3.90	5.75	9.65	11.50	13.45	17.20	21.10	23.00	9.50

For 500 copies deduct 5%; for 1000 or more deduct 10%.

For more than 32 pages add cost per schedule to make total. Example: for 44 pages add cost for 32 pages and 12 pages.

Note: For any reprints requiring additional composition or changes, either in text or cover, an extra charge will be made.

REPRINT ORDER FOR ARTICLE BY: _____
(Authors)

Article Title _____

To estimate the number of pages in your article, multiply the number of galleys by 3.

Total number ordered ..... Without Covers / With Covers } **Cross out one not wanted**

Send to: .....

.....

.....

Bill to: .....

.....

.....

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

August 11, 1980

Memo: Editorial Board meeting, Tucson

To: Editorial Board

The Editorial Board (present: Barr Bigelow, Butler, Rogerson, Johnson) met on Wednesday, August 6 at the University of Arizona. The Council of MSA met on August 3, and certain decisions made there affect the publication of MYCOLOGIA. Herewith a brief report.

(1) Gilbert C. Hughes (Botany, University of British Columbia) was approved by the Council (Editorial Board vote) as Book Review Editor, to replace Dick Korf. This is a five-year appointment.

(2) Acting on the vote and suggestions solicited from the Editorial Board the Council approved three appointments to the Editorial Board: Terry Hammill (SUNY, Oswego), Dick Hanlin (University of Georgia), and Charles E. Miller (Ohio University). Two alternates were also approved. Invitations have been extended to these persons, but we have not yet had acceptances.

(3) Beginning with the January/February issue, 1982, MYCOLOGIA will adopt a larger page size (similar to BRITTONIA), a larger plate size (comparable to that of AJB), and photographs prepared with a 300 line screen. The journal will be printed by Allen Press. Negotiations will begin this fall. In addition to the format improvements accompanying the change of printer, there may be other changes, as, for example, deleting boldface type of citations and bibliography (with some cost saving). I will explore modifications with Allen Press, and seek Board approval for any that may seem feasible. If you have any suggestions for change, please let me know of them.

(4) A new format will be designed for the 1981 volume (50th anniversary) of the journal.

(5) The matter of publishing invited papers was discussed. There is no MSA policy to prevent such invitations, but authors would have to agree to the usual review procedure. Suggestions for invitation should be sent to TWJ.

(6) The Board agreed that MYCOLOGIA is not to be a forum for "letters to the editor." To avoid problems arising from unfavorable book reviews and consequent author's response, the Board agreed that the Editor would assume the responsibility for communicating with the parties involved. If necessary, the NEWSLETTER editor(s) would be asked to publish author/reviewer responses simultaneously (and in their entirety) in the NEWSLETTER, but in no case would there be one such response/rebuttal.

(7) At both the Council and Board meetings some discussion was devoted to raising the quality of the journal. A major factor is that of identifying reviewers who will act critically but fairly in recommending acceptance or rejection of papers. Several persons at the MSA business meeting volunteered to serve as reviewers, but more are needed. If you have names to suggest, please send them on to me, or contact the person(s) directly. We do need competent, conscientious, and critical reviewers.

T.W. Johnson, Jr.
8/11/80

17-VII-1980

Dear Terry:

In accordance with guide lines previously discussed regarding the consideration of manuscripts from non-members, I favor publication provided that the paper in its entirety is unanimously judged to be acceptable by the reviewers.

This is also a reminder that I shall be in Brazil from mid-August through December, so please send no manuscripts during that interval.

With best wishes for your success as Editor,

Cordially,

Bernard
B. Lowy

T.W. Johnson . Bot. Dept. Duke Univ. Durham, N.C. 27706.

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 July 1980

Memo: Manuscript from nonmembers

To: Editorial Board

I have at hand an eight page manuscript from Sprecher and Hanssen, submitted for publication. Neither author is a member of the Society, and I am required to get a vote from the Editorial Board before the paper is considered.

Enclosed is a copy of the covering letter, the page on which the summary of the article appears, and the two pages constituting the discussion section.

Would you please let me know whether or not we should accept this paper for processing (it would of course have to be sent for review as is our practice). I enclose a postcard for your convenience. If you cannot make a decision without seeing the entire manuscript, please let me know, and I will send a full copy to you.

Thank you.

Terry

T.W. Johnson, Jr.

TWJ:jd

Enclosures: Letter & mss. pages
postcard

UNIVERSITÄT HAMBURG
LEHRSTUHL FUR PHARMAKOGNOSIE
Prof. Dr. E. Sprecher

Hamburg, June 27, 1980
Fernsprecher 4123 } (040) 4123 3899
Behördennetz 09
Postanschrift: 2 Hamburg 13, Bundesstraße 43

Dr. Margaret Barr Bigelow

Editor-in-Chief "Mycologia"
Department of Botany
University of Massachusetts

Amherst, Massachusetts 01003
U.S.A.

Dear Doctor Barr Bigelow:

please find enclosed a manuscript

THE INFLUENCE OF STRAIN SPECIFICITY AND CULTURE CONDITIONS ON TERPENE PRODUCTION BY FUNGI

submitted for publication in MYCOLOGIA.

Knowing that publication in MYCOLOGIA is usually restricted to members of the Mycological Society of America, we are nevertheless very much interested to publish our paper in your journal because

- Some related papers written by R.P. Collins and coworkers appeared in MYCOLOGIA.
- Most of the Ceratocystis strains studied by us have been isolated in the U.S. by R.W. Davidson.

In view of the above-mentioned reasons we want to invite American mycologists for further cooperation.

Looking forward to a favorable vote of the majority of the Editorial Board, I remain

sincerely yours,

E. Sprecher

THE INFLUENCE OF STRAIN SPECIFICITY AND CULTURE CONDITIONS ON TERPENE PRODUCTION BY FUNGI

EWALD SPRECHER and HANS-PETER HANSSEN

Abt. f. Pharmakognosie, Universität Hamburg, Bundesstr.43
D-2000 Hamburg 13, F.R.G.

SUMMARY

The volatile oil production by several strains of Ceratocystis coerulescens (Ascomycetes) and Lentinus lepideus (Basidiomycetes) is studied. The production of mono- and sesquiterpenes, and of non terpene compounds by these fungi proves to be highly influenced by strain specificity and culture conditions.

Numerous asco- and basidiomycetes are able to produce odoriferous metabolites, under natural as well as under cultural conditions (Sprecher, 1979). With the development of new methods in chromatographic and spectrometric analysis a multitude of volatile substances could be identified in microorganisms, the latter appearing not to be inferior to higher plants in their biosynthetic abilities. However, since fungi usually do not have (and do not need) specific sites of accumulation for eliminated "waste material" of metabolism (Sprecher, 1959; 1979; 1980), the spectrum of these volatile metabolic products is only in part comparable with essential oils of the typical essential oil bearing plants like Lamiaceae or Apiaceae. On the other hand, the production of volatile metabolites by heterotrophic microorganisms can be stimulated to a remarkable extent -similar to the production of antibiotics- by selection of suitable strains and by improvement of culture conditions.

In the following, from our study on the ascomycete Ceratocystis coerulescens Bakshi (Münch) and the basidiomycete Lentinus lepideus Fries, we will present some

The monoterpene accumulation of the Ceratocystis coerulescens strain 431 was especially stimulated by proline, hydroxyproline, and histidine (Ziemsen, 1979). These studies showed also a pronounced dependence on the concentration of the nitrogen source. In any case, specific optimum curves were obtained for the different strains.

The results obtained with the organisms studied indicate that -in addition to occasionally very significant differences in strain specificity- apparently very small changes in the composition of a synthetic culture medium may result in extraordinary changes in fungal growth and production of volatile compounds. Further investigations should elucidate how much inhibition or repression mechanisms, deficiency of cofactors, or other possible mechanisms are the main reason for the different reactions observed.

Even if one species can be recognized by a number of secondary metabolites which are produced by all strains under nearly all conditions, it often happens that certain compounds are not a common feature to all strains of this species. In these cases strain specificity can lead to metabolites typical for one or a few strains only (Sprecher, 1980).

The production of all these metabolites depends on the composition of the culture medium and culture conditions. Thus, the manipulation of culture conditions of many microorganisms reveals the broad genetic potency of the species examined which by itself is composed of the entirety of more or less different strains.

In this respect the addition of certain metabolites or other products to the culture medium and the transformation of these compounds may result in very different reactions including stereospecific transformations. From studies like this certain conclusions can be drawn in regard to fungal metabolism and its enzymatic abilities (Marcus, 1978; Sprecher, 1980), which of course

have to be verified by more differentiated studies with isolated enzymes.

Metabolites produced only to a small extent on the natural substrate of the fungus can be enormously enriched with suitable strains and under appropriate culture conditions. In this way the identification of minor compounds can be facilitated.

ACKNOWLEDGMENTS

We thank Dr. R.W. Davidson (Colorado State University, Fort Collins, Colo 80521) who made the Ceratocystis coerulescens strains available to us.
This work was supported by the Deutsche Forschungsgemeinschaft.

LITERATURE CITED

Birkinshaw, J.H., and Morgan, E.N. 1950. Biochemistry of wood-rotting fungi 6. Volatile metabolic products of species of Endoconidiophora. Biochem. J. 47, 55 - 59

Collins, R.P., and Halim, A.F. 1970. Production of monoterpenes by the filamentous fungus Ceratocystis variospora. Lloydia 33, 481 - 482

———. 1977. Essential oil composition of Ceratocystis virescens. Mycologia 69, 1129 - 1136

Grosche, W. 1978. Über Biosynthese und Akkumulation flüchtiger Stoffwechselprodukte bei drei Stämmen des Ascomyceten Ceratocystis coerulescens. Dissertation, Hamburg. 179 p.

Hanssen, H.-P. 1976. Der Einfluß des Nährmediums auf das Wachstum und die Bildung flüchtiger Stoffwechselprodukte von Ceratocystis coerulescens Bakshi (Münch). Diplomarbeit, Hamburg. 66 p.

———. 1979. Untersuchungen über die Bildung flüchtiger Stoffwechselprodukte bei einigen holzzerstörenden Pilzen. Dissertation, Hamburg. 210 p.

Campus Correspondence LOUISIANA STATE UNIVERSITY

~~From:~~

Date: 16-VI-1980

To:

Dear Margaret,

Please note that from mid-August until early January, 1981, I expect to be in Brazil, so that manuscripts should not be sent to me during that interval.

Congratulations on the wonderful job you have done as editor!

Bernice Lowy

PS - My retirement becomes effective July 1 but I expect to continue working here as usual - except for teaching.

I have no comments to make apart from the corrections made on the ms. BL

Dr. B. Lowy
Mycology Herbarium, Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Editorial Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

7 June 1980

Dear Bernie,

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

Studies on Indian polypores.IV. Morphological and cultural characters of Polyporus grammocephalus Berk.
by A. B. De and A. Roy

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Note - Figs. 1+2 missing when the ms. reached here.

Received - 13-VI-1980

Returned : 16 - VI-1980

MYCOLOGIA

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

16 June 1980

To: Members of Editorial Board of MYCOLOGIA

From: M. E. Barr Bigelow/T. W. Johnson

At the AIBS meetings in Tucson, a tentative meeting time of 4:30 pm, 5 or 6 August, is suggested. Would all members of the Editorial Board inform T. W. Johnson before 25 July if they plan to be in Tucson, and if so, which meeting time is preferable.

For discussion, matters that have surfaced over the past few months include:
Suggestions for guidelines to book review editors.
Publication of letters to editor in MYCOLOGIA?
Question of content of taxonomic articles: at least two respected members have suggested that articles containing enlarged descriptions only, no new taxa nor other new information, are not fitting in the journal. How would one specify this?

Answers to earlier questions:

Most agree that lists of publications should be included with biographies. Presumably, this could be left to the author's discretion.

Acceptance of manuscripts by nonmembers of MSA -- officially urge that they become members (many will). Otherwise, agreement by 3(-5) members of the Board.

General agreement than any publication of contents of the "Whisler report" should be in the Newsletter.

- - - - -

Of necessity, the Editorial Board meeting cannot take place prior to the Council Meeting of MSA. Two matters require opinions by mail prior to the August meeting so that the Council may approve nominees.

Book Review Editor 1981-
Both G. C. Hughes and R. Hanlin have been approached and are interested in the position. Other names suggested are L. Kohn (early in professional career), R. Petersen, D. Pfister (Newsletter editor).

Members of Editorial Board 1981-86 (replacement for T. W. Johnson and two more)
A. Ahern, M. Blackwell, T. Hamill, R. Hanlin, A. Held, H. Hoch, F. Kazama, C. E. Miller, J. T. Mullins, R. Petersen, M. Powell.

Would you inform T. W. Johnson of your choices for these posts, either from the listed names or with additional suggestions.

- - - - -

As one of my final official duties as Editor-in-Chief, I wish to express my deep appreciation to all of you for your guidance in a number of issues, as well as for reviewing numerous manuscripts. Your support has lightened the burden and I am certain that you will continue to support my successor in the same fashion.

Margaret

Dr. B. Lowy
Mycology Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Editorial Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

28 March 1980

Dear Bernie,

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

Collections in the Farlow Herbarium: New species of Melanophyllum and Gastrocybe, type studies on Armillaria and Stropharia
by T. J. Baroni

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?

5. Have the tables been prepared as clearly and concisely as possible?

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

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received: 8-IV-1980

Returned 19-IV-'80

Comments on "New species of Melanophyllum and Gastrocybe ..."

by T. J. Baroni

The author states (p.9, par. 3) that "According to Singer (1975) one of the important features of the genus Melanophyllum is the olivaceous color of the spore deposit when fresh. The color of the deposit changes to fuscous or dark brownish upon dehydration." This is slightly misleading, since Singer does not use the term "olivaceous," nor does he say that the spores change to "fuscous to dark brownish" upon dehydration. He says instead (p. 466, lines 3-4): "spore print blue green, or olive to green (J-2 of Lange's chart), later often becoming fuscous purple by dehydration;" Since Singer's observations are referred to on this point, all ambiguity should be avoided by quoting his brief description verbatim.

The figures are generally quite satisfactory, and some of them are excellent, but the use of numerals on the figures to which they refer is erratic. If the numerals in figs 1-4 may be taken to establish the chosen pattern of size, then all numerals should conform to it. However, the numerals for figs. 5-9 and 15-16 are larger, and the last plate (17) bears no number. These irregularities should be corrected for the sake of uniformity.

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

Editorial Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

19 January 1980

Dear Bernie,

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

Release of airborne basidiospores from the pouch fungus, Cryptoporus volvatus
by T. C. Harrington

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?

5. Have the tables been prepared as clearly and concisely as possible?

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

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received 25-I-1980. Returned 28-I-1980.

Comment on "Release of Airborne Basidiospores....." by

T. C. Harrington

In this interesting paper, I would only draw attention to the following minor points. In fig. 1 the inclusion of a scale would be appropriate, and the figure number is missing from the chart with graphs.

B. Lowy

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

Editorial Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

1 December 1979

Dear Bernie,

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

Type studies in the genus Coprinus (Agaricales): Disposition of C. elongatipes
by W. W. Patrick, Jr. and A. H. Smith (8 ms pp)

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?

5. Have the tables been prepared as clearly and concisely as possible?

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

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Accepted: 10-XII-1979.

Returned: 11-XII-1979

TYPE STUDIES IN THE GENUS COPRINUS (AGARICALES): DISPOSITION OF C. ELONGATIPES

W. W. PATRICK JR.

Matthaei Botanical Gardens, University of Michigan, Ann Arbor, Michigan 48109

AND

ALEXANDER H. SMITH

University Herbarium, University of Michigan, Ann Arbor, Michigan 48109

SUMMARY

The Colorado type and only known collection of *Coprinus elongatipes* is redescribed and taxonomically re-evaluated. The reciprocate characteristics of the closely related *Coprinus* section *Auricomi* and *Psathyrella* series *Subatratae* are discussed, and evidence is presented for inclusion of *C. elongatipes* in the latter where it falls within the circumscription of a taxon presently known as var. *microspora* of *Psathyrella circellatipes*.

Thirty four years ago the second author, with Dr. L. R. Hesler, described several new or unusual melanosporous agarics from North America (9). One of the new taxa was *Coprinus elongatipes*, a fungus found by Hesler high in Rocky Mountain National Park in Colorado. In conjunction with the monographic study of North American Coprini taking place, we have analyzed and critically re-evaluated the position of this species in light of current generic

Dr. B. Lowy
Mycology Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Editorial Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

18 October 1979

Dear Bernie,

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

South Indian Agaricales VIII. Agaricochaete indica sp. nov.
by K. Natarajan and N. Raman (Brief Article)

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received: 27-X-1979

Returned: 2-XI-'79

Dr. B. Lowy
Mycology Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

30 August 1979

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

The species of Eichleriella (Tremellaceae) of the U.S.S.R.
by K. Wells and A. Raitviir

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received: 5-IX-1979

Returned: 4-IX-1979

Comment on "The species of Eichleriella ...."

by K. Wells & A. Raitviir

The only point I would call attention to in this fine paper is that some collections of E. alliciens are referred to (p. 6, lines 1, 15, 27) as originating in Central America. It has been collected from Mexico and elsewhere in the Americas, but I know of none reported from Central America.

B. Lowy

7-IX-1979

THE SPECIES OF EICHLERIELLA (TREMELLACEAE) OF THE U.S.S.R.

K. Wells

Department of Botany, University of California, Davis, California 95616

and

A. Raitviir

Institute of Zoology and Botany, Tartu, Estonian S.S.R., U.S.S.R.

Running head: Wells and Raitviir: Eichleriella

SUMMARY

The genus Eichleriella is defined to include those tremellaceous species forming a basidiocarp with an abrupt to reflexed margin, a smooth hymeniun or with fertile pegs or spines, a basal layer of thick walled, brownish hyphae, and a vinaceous tint upon drying. The species of Eichleriella known from the U.S.S.R. are described and illustrated. E. alliciens, which is defined to include E. incarnata, is shown to occur in the Far Eastern Region (i.e., Khabarovskiy Kray and Primorskiy Kray). Several collections of E. leucophaea, which is redefined to exclude E. schrenkii, are described from Tadzhikistan and Turkmenistan (i.e., Tadshikskaya S.S.R. and Turkmen S.S.R.). E. deglubans, which is tentatively included in the genus, is evidently widely distributed throughout the U.S.S.R. The species of Eichleriella in the U.S.S.R. are known to occur only on decaying angiosperm wood.

This report is a continuation of our joint studies of the resupinate species of the Tremellaceae of the U.S.S.R. Previous reports have treated the genus Exidiopsis (Raitviir and Wells, 1966; Wells and Raitviir, 1977) and the genera Bourdotia and Basidiodendron (Wells and Raitviir, 1975).

The method of study is as described earlier (Wells, 1969) and the terminology of the basidium follows Martin (1957) and Wells (1964). The recommendations of the U.S. Board on Geographic Names (Bloom, 1970) are followed where possible, and the capitalized color names are those of Ridgway (1912).

All drawings were made with the aid of a Zeiss Drawing Apparatus and were made in most cases of structures displaced from their position in intact basidiocarps.

Eichleriella Bres., Ann. Mycol. 1: 115. 1903.

Basidiocarps resupinate with abrupt margins, which are free or become reflexed upon drying; coriaceous to soft coriaceous, usually with vinaceous tint; hymenia smooth or with fertile pegs or spines; in section generally

consisting of a basal layer of distinct hyphae with thickened, brownish walls, either parallel to the substrate or ascending, an intermediate layer, or layers, of thick walled, clamped hyphae that are interwoven or ascending, and the hymenium of basidia and dikaryophyses; cystidioles present or absent; dikaryophyses simple to branching, sometimes with thickened walls; basidia ovate, obovate, clavate, to pyriform with enucleate stalk, always with basal clamp, forming (2-)4 hypobasidial segments; epibasidia always present, tubular; basidiospores cylindrical curved to allantoid, capable of germinating by repetition.

Type species: Eichleriella incarnata Bres. (Donk, 1958)

As defined here the genus Eichleriella is restricted to those tremellaceous species with relatively thick, resupinate, coriaceous basidiocarps with abrupt margins. The margins of the several species are either adnate or free in fresh specimens. Generally those with adnate margins seem to become reflexed upon drying, evidently because the hyphae near the abhymenial surfaces are more rigid than those forming the hymenium and subhymenium. Additional observations of fresh collections will be needed, however, before a more accurate characterization of the margins of the basidiocarps can be given.

The presence of a basal layer of thick walled, brownish hyphae adjacent to the substrate, which is generally better developed near the margins, and the fact that portions of well preserved specimens have a vinaceous tint are characters that should serve to separate Eichleriella from the closely related species of Exidiopsis (Joh.-Ols. ex Bref.) A. Möller. The presence of sterile spines distinguishes the species of Heterochaete Pat. from Eichleriella, but further studies of Heterochaete and other tropical taxa are needed before Eichleriella deglubans can be classified with confidence.

In addition to the species treated in this report, we would include within our concept of Eichleriella, E. leveillianum (Berk. et Curt.) Burt and E.

schrenkii Burt. Although Eichleriella subleucophaea McNabb (McNabb, 1969), E. macrospora (Ell. et. Everh.) Martin, and Exidiopsis griseobrunnea Wells et Raitviir (Wells and Raitviir, 1977) are macroscopically similar, except for differences in color, they lack a basal layer of hyphae with brownish, thickened walls. Eichleriella gelatinosa Murrill is also to be excluded (Wells, 1961).

The generic concept adopted here is essentially as described by Bresadola (1903), Bourdot and Galzin (1927), and Burt (1915). Earlier authors (Bourdot and Galzin, 1927; Burt, 1915; Martin, 1952; Donk, 1958) listed Sebacina sect. Hirneolina Pat. and Hirneolina (Pat.) Bres. as synonyms of Eichleriella; however, the type species of Hirneolina is Sebacina hirneoloides Pat. [= Heterochaete hirneoloides (Pat.) Wells], which is believed (Wells, 1969) to be more appropriately included in the genus Heterochaete.

(Fig. 1)

1. Eichleriella alliciens (Berk., et Cke.) Burt, Ann. Missouri Bot. Gard. 2: 746. 1915. Fig. 1

Stereum alliciens Berk. et Cke., J. Linn. Soc, Bot. 15: 389. 1876.

Eichleriella incarnata Bres., Ann. Mycol. 1: 116. 1903.

Hirneolina incarnata (Bres.) Bres., apud Sacc., Sylloge Fung. 17: 208. 1905.

Hirneolina crocata Pat., Bull. Soc. Mycol. France 40: 31. 1924.

Eichleriella mexicana Burt, Ann. Missouri Bot. Gard. 13: 334. 1926.

Hirneolina ubatubensis Viégas, Bragantia 5: 242. 1945.

Exidiopsis alliciens (Berk. et Cke.) Wells, Mycologia 53: 354. 1961 (1962).

Basidiocarps coriaceous, arising as small circular to elongate patches, enlarging to form irregular structures several centimeters in length, light to dark buff usually with vinaceous or grayish tint; surface smooth to finely granulose; margins abrupt, adnate but sometimes pulling away from the substrate on drying, then somewhat recurved, abhymenial surface brownish; drying

to an ochraceous to buff, loose crust with vinaceous tint (Pale Ochraceous-Buff to Pinkish Buff, central portions varying to Light Cinnamon-Drab); in section 150-600(-1000) µm, consisting of a basal layer of distinct, brownish, thick walled hyphae, interwoven, sometimes loosely interwoven or in fascicles, an intermediate layer of interwoven hyphae, becoming thick walled, sometimes compact and parallel to the substrate, walls brownish in some regions, and terminating in an ascending layer from which the hymenium of fertile hyphae, dikaryophyses, and cystidioles develops, basal layer sometimes absent near center of basidiocarp but obvious near margins, intermediate layer of varying width, hypobasidia in a well defined zone of 30-45 µm, covered by a layer of dikaryophyses 15-30 µm in section, portions of substrate sometimes present in basal and intermediate layers; hyphae of basal layer with clamps, walls up to 2 µm in thickness and brownish, contents not staining, 2.5-5 µm in diam; hyphae of intermediate layer with clamps, becoming thick walled, less often with brownish walls, contents staining, 2.5-4.5 µm in diam, less often with some hyphal segments spherical or irregular up to 14.5 µm in diam; dikaryophyses branching near apices, nodulose, 1-4 µm in diam, usually distinct and abundant but becoming indistinct in older specimens; cystidioles subclavate, subfusiform, to subcylindrical, sometimes attenuate and branching at apices, becoming devoid of contents and less often slightly thick walled, sometimes projecting in resoaked specimens, possibly due to shrinkage, 35-65 X (1.5-) 2.5-11 µm, elements intermediate in form and size between cystidioles and dikaryophyses common; fertile hyphae 2-4(-5) µm in diam; probasidia at first cylindrical to narrow clavate or narrow fusiform; hypobasidia with basal clamp, with (2-)4 segments, obovate, clavate, ovate, rarely subglobose, often guttulate, (12-)13-20 X (6.5-)7.5-12(-13) µm; epibasidia flexuous, subcylindrical, 2.5-3 µm in diam, up to 42 µm in length; basidiospores allantoid, typically bluntly rounded at apex and more attenuate at base, guttulate, 12-16 X 3.5-5.5 µm, capable of germinating by repetition.

On decaying angiosperm wood. Known from Central and South America, Europe, and Vietnam.

Type locality.--Brazil.

Illustrations.--Bresadola, G. 1903. Ann. Mycol. 1: pl. 3, Fig. 1. Bresadola, G. 1932. Icon. Mycol. 23: pl. 1118, Fig. 1. Burt, E. A. 1915. Ann. Missouri Bot. Gard. 2: 771, Fig. 10. Viégas, A. P. 1945. Bragantia 5: 242, pl. 6. Wells, K. 1961 (1962). Mycologia 53: 350, Fig. 13.

Specimens examined.--R.S.F.S.R., Khabarovskiy Kray, Selikhin, on decaying angiosperm wood, TAA 15526; Yagdyn'ya, on Acer mono Maxim., TAA 15912; Primorskiy Kray, Zapovednik Ussuriyskiy, on decaying angiosperm wood, TAA 14937.

The type collection of Stereum alliciens (Herbarium Berkeley 1879; in K) and the two specimens (Herbarium J. Bresadola 4, 36; in S) on which Bresadola (1903) based his description of E. incarnata were also examined as were a specimen from Munich, Germany (Oberwinkler 1907) and several collections from Central and South America.

E. alliciens is characterized by the vinaceous, resupinate basidiocarps, usually with abrupt margins, the thin walled, branching dikaryophyses, and the oval to obovate basidia that are without an enucleate stalk. The basal layer of brownish, thick walled hyphae is usually quite conspicuous, especially near the margins. The hyphae of the intermediate zone are generally of uniform diameter, never torulose as in E. leucophaea.

Because of the apparent temperate distribution of this species in the U.S.S.R. and Western Europe and the tropical and subtropical distribution in North and South America, there is some doubt that Stereum alliciens, which is based on a collection from Brazil, and E. incarnata, described from specimens collected in Poland, should be combined. The basidia (mean: 19.08 X 11.69 µm) of the Central and South American specimens studied tend to be somewhat larger than the basidia (mean: 15.57 X 10.13 µm) of the collections examined from Western Europe and the U.S.S.R. Also, the basidiospores (mean: 14.73 X

6.07 μm) of the American specimens are slightly broader than the basidospores (mean: 14.59 X 4.29 μm) of the collections from Europe and Asia. Interfertility studies between specimens from these two regions might be most useful in resolving the question of speciation here.

(Fig. 2)

2. Eichleriella leucophaea Bres., Ann. Mycol. 1: 116. 1903. Fig. 2

Hirneolina leucophaea (Bres.) Bres. apud Sacc., Sylloge Fung. 17: 209. 1905.

Exidiopsis leucophaea (Bres.) Wells, Mycologia 53: 352. 1961 (1962).

Basidiocarps coriaceous, arising as small irregular to orbicular patches 1-2 mm in diam that can enlarge and become confluent to form irregular basidiocarps several centimeters in dimension; surface smooth and pruinose or finely granulose, sometimes cracking; margins abrupt, usually lighter (to Tilleul-Buff or nearly white), becoming reflexed, exposed abhymenial surface buff to dark reddish brown (Light Ochraceous-Salmon to Russet or Fuscous); drying to an ochraceous to light gray, brittle crust, often with a faint vinaceous tint (Pallid Mouse Gray, Pale Olive Gray, Pale Gull Gray, Pearl Gray); in section (250-)300-1350 μm, in the central portion consisting of a distinct basal layer of thick walled, brownish hyphae usually parallel to the substrate and often closely packed, less often interwoven, giving rise to an ascending layer of thick walled, torulose hyphae and a hymenium of fertile hyphae and dikaryophyses, near the margins an additional layer of interwoven, thick walled brownish hyphae, sometimes arranged in fascicles or irregular, is often present adjacent to the substrate; there is often a distinct horizontal zone of mineral granules in the ascending layer of the basidiocarp; hypobasidia in a zone 40-65 μm in width and covered by a distinct layer of dikaryophyses 25-60 μm in section; basal hyphae and hyphae of marginal abhymenial zone with walls 0.5-2 μm in thickness, brownish, contents usually not staining, with clamps, 2-4(-5) μm in diam; hyphae of ascending layer (2.5-) 3.5-15(-20) μm in diam, walls 1.5-7 μm in thickness, many elements torulose

(i.e., walls irregular in thickness and lumina irregular in diam), with clamps, in lower regions walls becoming brownish and lumina not staining, walls sometimes roughened or incrusted with mineral crystals; dikaryophyses usually abundant, branching and nodulose near apices, 1-3 µm in diam near apices, unbranched basal portions 3-7 µm in diam, becoming indistinct in some specimens; fertile hyphae 2-4.5(-6.5) µm in diam, with thickened walls to base of developing basidia; probasidia initially cylindrical to narrow clavate, then fusiform; hypobasidia with basal clamp, with (2-)4 segments, oval, elongate-oval, ovate to obovate, often guttulate; 16.5-20-25(-31) X (8-)9.5-13-16 µm; epibasidia tubular, somewhat flexuous, enlarging slightly apically, up to 65 µm in length, 2.5-4 µm in diam; basidiospores allantoid, guttulate, (15-)16-18-21 X 6-7.5 µm, capable of germinating by repetition.

On decaying angiosperm wood. Known from Poland, Bulgaria (Pilát, 1937) and France.

Type locality.--Poland.

Illustrations.--Bresadola, G. 1903. Ann. Mycol. 1: pl. 3, Fig. 2. Bresadola, G. 1932. Icon. Mycol. 23:, pl. 1118, Fig. 2. Wells, K. 1961 (1962). Mycologia 53: 350, Fig. 12.

Specimens examined.--Tadzhikskaya S.S.R., Zapovednik Tigrovaya Balka, on Populus euphratica Oliv. f. pruinosa (Schrenk) Nevski, TAA 96698. Turkmen S.S.R., Arvaz (Kopet-Dag Mountains), on Berberis iberica Stev. et Fisch., TAA 55071; Aydere, on Berberis sp., TAA 54932; Dushak (Kopet-Dag Mountains), on Colutea gracilis Freyn et Sing. ex Freyn, TAA 55677, 55687.

E. leucophaea is well characterized by the thick, vinaceous basidiocarps with abrupt margins, the torulose hyphae of the ascending layer, and the relatively large, allantoid basidiospores. The margins are usually reflexed in the dried specimens exposing the brownish, thick walled, basal hyphae.

Re-examination of the holotype of E. schrenkii Burt. (H. von Schrenk, Feb. 9, 1914; in FH) and several specimens supplied by Drs. R. L. Gilbertson

and H. H. Burdsall, Jr. has shown that this species is unlike E. leucophaea. The basidiocarps of E. schrenkii are gregarius and remain small, 1-6 mm in diam, and lack the torulose hyphae in the ascending layer. Gilbertson et al. (1976), who followed Wells (1961) in designating their collections of E. schrenkii as Exidiopsis leucophaea (Bres.) Wells, described and illustrated a number of collections from Arizona. E. schrenkii is known only from the desert regions of southwestern United States but is evidently relatively common in this region, especially on Prosopis juliflora (Sw.) DC. (Burt, 1915; Gilbertson et al., 1976).

(Fig. 3)

3. Eichleriella deglubans (Berk. et Br.) Reid, Trans. Brit. Mycol. Soc. 55: 436. 1970. Fig. 3

Radulum deglubans Berk. et Br., Ann. Mag. Nat. Hist., Ser. 4, 15: 32. 1875.

Radulum kmetii Bres., Atti Imp. Regia Accad. Roverto, Ser. 3, 3: 102. 1897.

Eichleriella kmetii (Bres.) Bres., apud Bourd. et Galz., Bull. Soc. Mycol. France 25: 30. 1909 (1910).

Hirneolina kmetii (Bres.) Sacc. et Trott., Sylloge Fung. 21: 451. 1912.

Basidiocarps arid, drying soft coriaceous, arising as small orbicular patches approximately 1 mm in diam that develop marginally and become confluent to form basidiocarps several cm in longer dimension; surface finely granulose to pruinose, often cracking to expose the lighter subhymenium, older portions with scattered or clustered spines that often arise from elongated to circular elevated portions of the basidiocarp; spines of same color as the hymenium or with darker apices, sometimes fimbriate; margins abrupt or, rarely, indeterminate, often fibrillose, lighter, adnate, or becoming reflexed on drying to expose the dark (near Benzo Brown) abhymenial surface, usually without spines; on drying forming a light to dark buff, grayish buff, ochraceous buff, often with pinkish tint, coriaceous layer (Pallid Vinaceous-Drab,

of varying width or absent, giving rise to an interwoven ascending layer, sometimes in fascicles, terminating in the hymenium of fertile hyphae, dikaryophyses, and, rarely, cystidiole-like structures, the basal prostrate layer may compose most of the trama or the trama may be composed essentially of ascending hyphae, heavy mineral deposits often present in the upper regions of the ascending layer; spines consisting of a central ascending core of hyphae diverging to form a typical hymenium laterally, apices of spines sterile and usually fimbriate; hyphae of basal, prostrate layer with irregularly thickened, brownish walls, distinct, becoming devoid of contents, 2.5-6.5 µm in diam, older portions generally lacking clamps; hyphae of interwoven ascending layer with scattered clamps, walls becoming irregularly thickened, with brownish tint, 2-5 µm in diam; dikaryophyses simple to short branched, nodulose near the apices, thin walled, projecting little beyond the hypobasidial level, sometimes incrusted with minerals, 1-4 µm in diam; cystidiole-like elements rarely present, subcylindrical, subclavate, or subfusiform, 33.5-85.5 X 2-7.5 µm; fertile hyphae 2.5-5 µm in diam; probasidia initially cylindrical, becoming narrow fusiform to narrow clavate, with basal clamp; hypobasidia clavate, elongate-stalked, to pyriform, developing (2-)4 hypobasidial segments and, usually, an enucleate stalk, often guttulate, apical segmented portions (i.e., hypobasidia) 23-40 X 9-12.5-15(-17) µm, enucleate stalks, when present, 2-8.5-20 µm in length, 2-3.5-5.5(-7.5) µm in diam in median region, expanding apically; epibasidia tubular, up to 21 µm in length, 4-5.5 µm in diam; basidiospores blunt cylindrical curved, guttulate, (14-)15-17-21 X 6.5-7.5-9 µm, capable of germinating by repetition.

On decaying angiosperm wood. Known from North America, Europe, and Morocco (Malençon, 1952).

Type locality.--England.

Illustrations: Bourdot, H. and A. Galzin. 1927 (1928). Hymen. France, p. 47, Fig. 24 (as E. spinulosa); Burt, E.A. 1915. Ann. Missouri Bot. Gard.

2: 771, Fig. 11 (as E. spinulosa); Malençon, M. G. 1952. Bull. Soc. Mycol. France 68: 303, Fig. 1, C,D (as E. spinulosa); Pilát, A. 1957. Sborn. Nár. Mus. v Praze, Rada B, Prir. Vedy 13B: pl. 23a,b, 24a,b, ~~13B: pl. 23a,b, 24a,b.~~ Christiansen, M. P. 1959. Dansk Bot. Ark. 19: 33, Fig. 25 (as E. spinulosa). Specimens examined.--Armenian S.S.R., Kirovakan, on Tilia cordata Mill., TAA 15151, on Fagus orientalis Lipsky, TAA 15183. Estonian S.S.R., Haapsalu, on Fraxinus excelsior L., TAA 15580; Orajõe, on Salix caprea L., TAA 5795; Puhatu Biological Station, near Lihula, on Corylus avellana L., TAA 3493, on Fraxinus excelsior, TAA 5617; Saaremaa, Sorve, on Sorbus intermedia (Ehrb.) Pers., TAA 7482; Tidriku near Rakvere, on decaying angiosperm wood, TAA 5685. R.S.F.S.R. Kamchatskaya Oblast' (Kamchatka Peninsula), Kozyrëvsk, on Populus tremula L., TAA 12824; Krasnoyarskiy Kray, Badzhey, on Populus tremula, TAA 9847; Krasnyy Les, on decaying angiosperm wood, TAA 19755; Stavropol'skiy Kray, Karachayevo-Cherkesskaya Avtonomnaya Oblast', Teberdinskiy Zapovednik, on Lonicera sp., TAA 53292; Tyumenskaya Oblast', Polyarnyy Ural (Polar Ural Mountains), on Sorbus aucuparia L.; Krasnosel'kup, on Padus avium Mill., TAA 17108. White Russian S.S.R. (Belorusskay S.S.R.), Brestskaya Oblast', Zapovednik Bielovezhskaia Pushtsha, on Fraxinus excelsior, TAA 19149.

The soft coriaceous, vinaceous basidiocarps, the clavate to pyriform basidia that usually develop an enucleate stalk, and the blunt cylindrical curved basidiospores adequately characterize this species. Spines, which are laterally fertile and usually arise from circular to elongated elevated portions of the basidiocarps, are found in most, but not all, collections. The margins of the younger and developing basidiocarps are usually lighter in color, fimbriate, and remain adnate. The margins of older fruiting bodies are more often abrupt and reflexed in dried specimens exposing the brownish, thick walled, basal hyphae.

Based on the available specimens, E. deglubans is the most common and widely distributed species of Eichleriella in the U.S.S.R. and occurs on the decaying wood of a variety of anigosperm species.

Since no nomenclatural changes are required, we are retaining E. deglubans in the genus Eichleriella in order to describe the variety of specimens available. The distinctive clavate basidia usually with enucleate stalks are so unlike those of the other species included here in Eichleriella, that there is some doubt that this species is closely related to E. alliciens and species with similar basidia.

E. deglubans has usually been designated Eichleriella spinulosa (Berk. et Curt.) Burt (Burt, 1915; Bourdot and Galzin, 1927; Martin, 1952; Pilát, 1957; Wells, 1961), most listing Radulum deglubans Berk et Br. as a later synonym. Reid (1957, 1970) has shown, however, that the type of Radulum spinulosum Berk. et Curt. has smaller basidia and basidiospores, forms completely sterile spines, and possesses cylindrical sterile elements in the hymenium. Accordingly, he (Reid, 1970) transferred R. spinulosum to the genus Heterochaete. We have examined the type of R. deglubans (J. Keith, 30-I-1874; in K) and the type of R. spinulosum (Peters 4543; in K) and agree with Reid's diposition. The clavate basidia of R. spinulosum upon segmentation do not, in our opinion, form an enucleate basal stalk, as in E. deglubans. Because he described and illustrated clavate basidia with an enucleate stalk, we believe that McNabb (1969) had in hand E. deglubans rather than Heterochate spinulosum; but we have not examined specimens studied by him.

ACKNOWLEDGMENTS

We wish to thank the Directors of the Royal Botanic Garden, Kew, the Farlow Herbarium, Cambridge, and the Naturhistoriska Riksmuseum, Stockholm, for the loan of type specimens and Dr. F. Oberwinkler for the loan of a specimen of E. alliciens. We are again especially indebted to Dr. E. Parmasto for the use of his many collections. The senior author thanks Dr. R. J. Bandoni in whose laboratory portions of this study were conducted and Drs. R. L. Gilbertson and H. H. Burdsall, Jr. for the use of several specimens of E. schrenkii.

The senior author wishes to express his appreciations to the National Academy of Sciences for arranging an exchange visit to Tallinn, Estonian S.S.R. This study was made possible by National Science Foundation Research Grants GB 12924 and DEB 77-25345.

LITERATURE CITED

Bloom, L.A., chairman. 1970. Official standard names, Gazetteer 42, 2nd ed. U.S. Board on Geographic Names. U.S. Army Topographic Command, Washington, D.C. 7 vols.

Burdot, H., and A. Galzin. 1927 (1928). Hyménomycètes de France. Sceaux. 761 p.

Bresadola, J. 1903. Fungi polonici. Ann. Mycol. 1: 65-131.

Burt, E.A. 1915. The Thelephoraceae of North America. V. Tremellodendron, Eichleriella, and Sebacina. Ann. Missouri Bot. Gard. 2: 731-771.

Donk, M.A. 1958. The generic names proposed for Hymenomycetes--VIII. Auriculariaceae, Septobasidiaceae, Tremellaceae, Dacrymycetaceae. Taxon 7: 164-178, 193-207, 236-250.

Gilbertson, R.L., H.H. Burdsall, Jr., and E.R. Canfield. 1976. Fungi that decay mesquite in southern Arizona. Mycotaxon 3: 487-551.

_____, and A. Raitviir. 1975. The species of Bourdotia and Basidiodendron (Tremellaceae) of the U.S.S.R. Mycologia 67: 904-922.

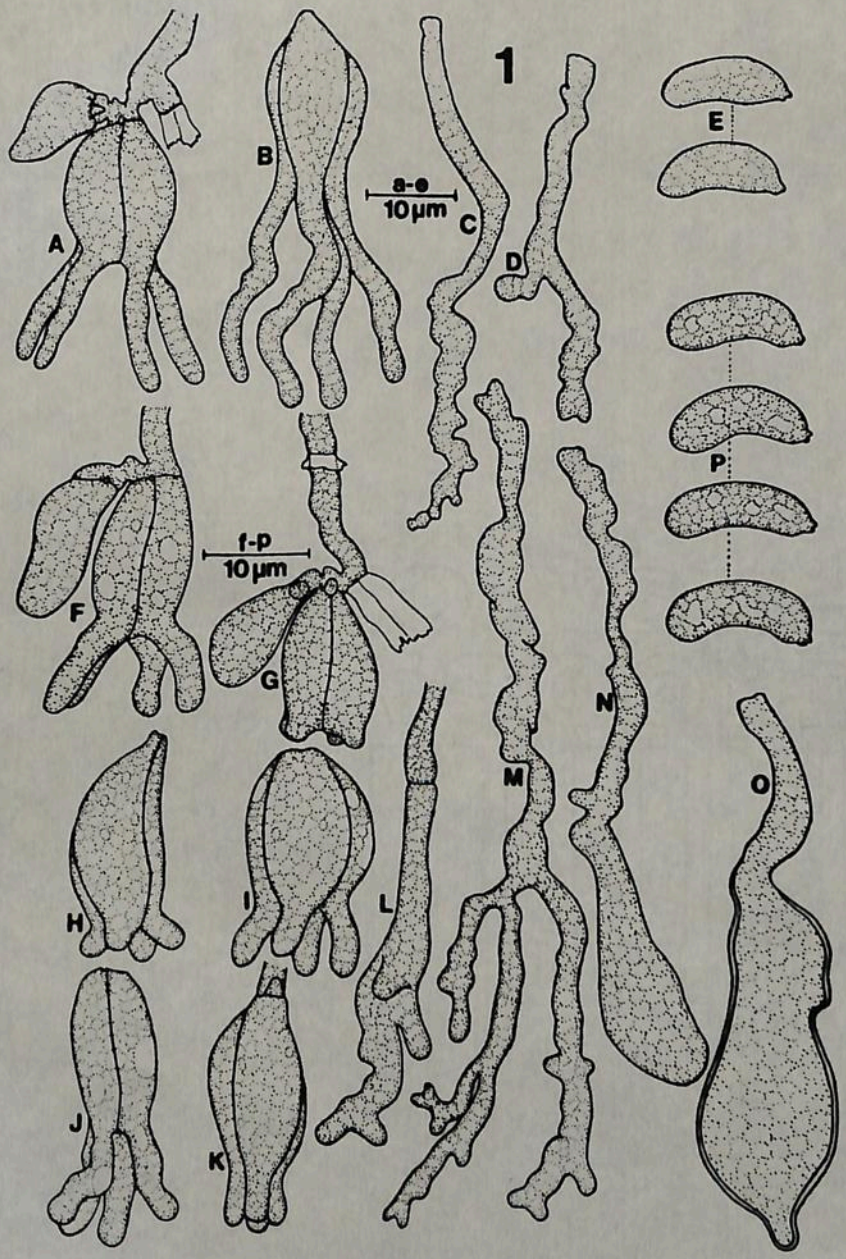
_____, and _____. 1977. The species of Exidiopsis (Tremellaceae) of the U.S.S.R. Mycologia 69: 987-1007.

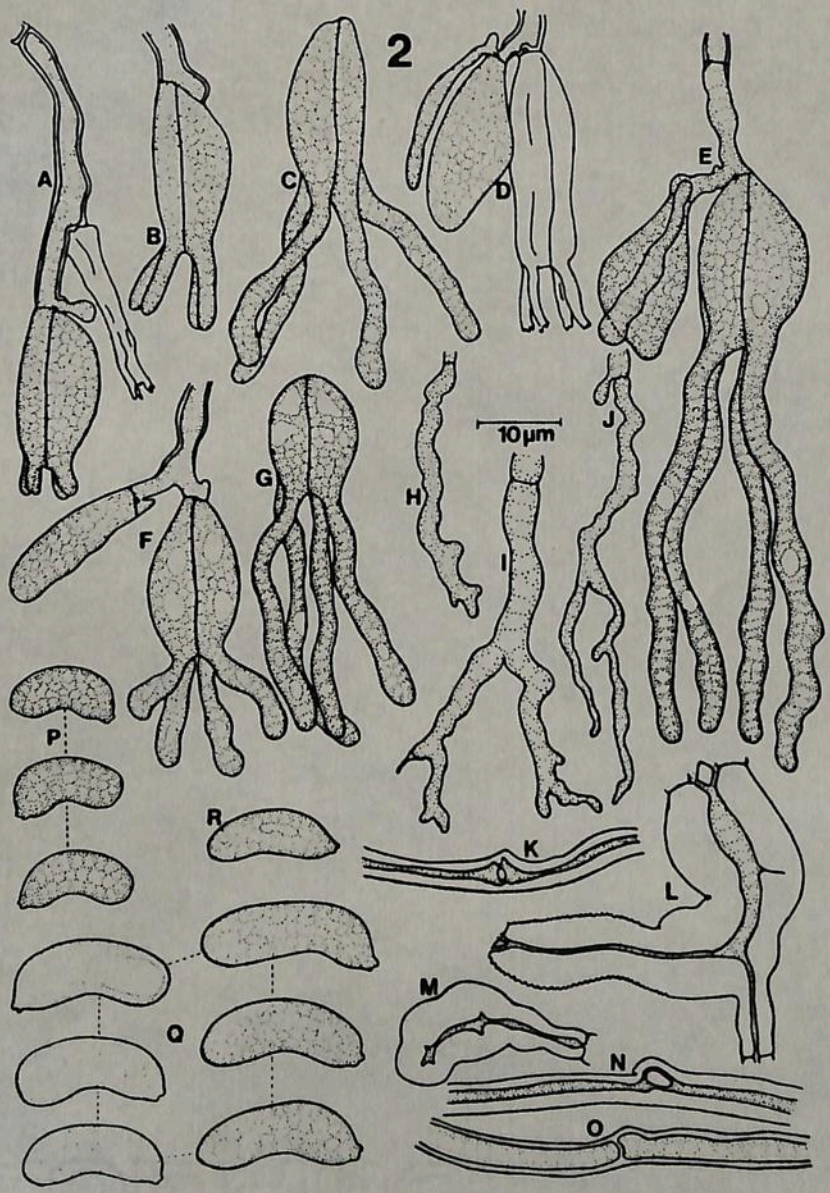
LEGENDS FOR FIGURES

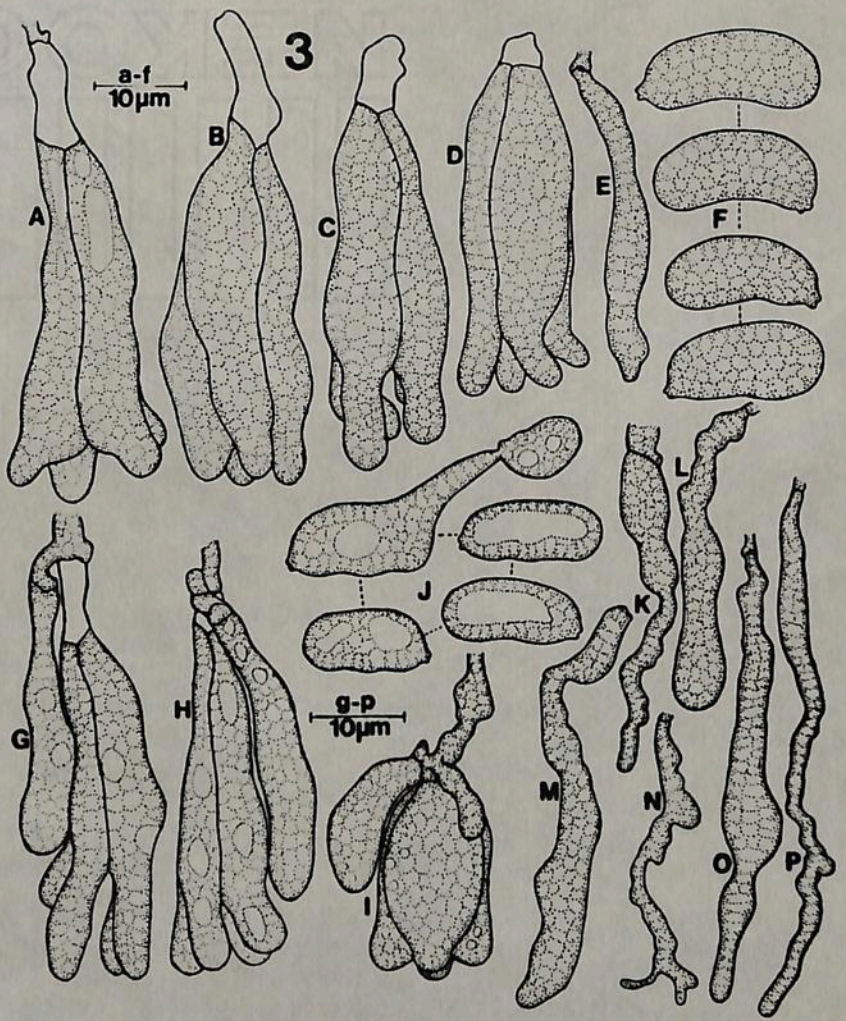
Fig. 1. Eichleriella alliciens. A,F,G. Segments of fertile hyphae. B, H-K. Basidia (A,B from Herbarium G. Bresadola 4; F from TAA 15526; G-K from TAA 14937). C,D,L-O. Dikaryophyses and cystidioles (C,D from Herbarium G. Bresadola 4; L-N from TAA 14937; O from TAA 15526). E,P. Basidiospores (E from Herbarium G. Bresadola 4; P from TAA 15526).

Fig. 2. Eichleriella leucophaea. A,B,D-F. Segments of fertile hyphae. C,G. Basidia (A,B from Herbarium G. Bresadola 19; C,D,F,G from TAA 55071; E from TAA 55677). H-J. Dikaryophyses (H,J from TAA 55071; I from TAA 55677). K-M. Hyhphae from ascending layer (from Herbarium G. Bresadola 19). N,O. Hyphae from basal layer (N from Herbarium G. Bresadola 19; O from TAA 55677). P-R. Basidiospores (P from Herbarium G. Bresadola 19; Q from TAA 54932; R from TAA 55677).

Fig. 3. Eichleriella deglubans. A-D. Basidia (from J. Keith, 30-I-1874). G-I. Segments of fertile hyphae (G,H from TAA 17108; I from TAA 15580). F,J. Basidiospores (F from J. Keith, 30-I-1874; J from TAA 17108). K-P. Dikaryophyses (K-O from TAA 15580; P from TAA 17108).


1
A
B
a-e
10µm
C
D
E
P
F
f-p
10µm
G
H
I
J
K
L
M
N
O


2
A
B
C
D
E
F
G
H
I
J
10µm
K
L
M
N
O
P
Q
R


3
a-f
10µm
A
B
C
D
E
F
g-p
10µm
G
H
I
J
K
L
M
N
O
P

State of New Jersey
GLASSBORO STATE COLLEGE
GLASSBORO, NEW JERSEY 08028

LIFE SCIENCE DEPARTMENT

July 2, 1979

Dr. Margaret Barr Bigelow,
Editor-In-Chief
MYCOLOGIA
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

Dear Dr. Bigelow:

I am suggesting that you consider Dr. Lekh Batra as a member of the Editorial Board for MYCOLOGIA. I have discussed that matter with Dr. Batra, and he has expressed an interest in being appointed to the Editorial Board. Dr. Batra has not held an office in the Mycological Society of America although he has been a member for twenty-five years.

Dr. Batra seems to be well qualified for this position. He has published numerous articles dealing with the Hemiascomycetes and Discomycetes in journals such as Mycologia, Science, Scientific American, and the American Journal of Botany. He has also edited a 320 page book, INSECT-FUNGUS SYMBIOSIS: NUTRITION, MUTUALISM AND COMMENSALISM which is being released this month by John Wiley and Sons, and it promises to be outstanding. Dr. Batra has reviewed articles for MYCOLOGIA as well as several books.

Sincerely yours,

Elizabeth J. Moore

EJM/pp
cc: Members of Editorial Board

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

22-VI-1979

Dear Margaret,

Enclosed are my comments on the 2 papers you sent me, together with brief answers to some of the questions you ask in your 7 June memorandum.

Also included is a suggestion for a minor editorial policy change, which I have put in the form of a motion to be presented at the next meeting of the Ed. Board, which I cannot attend.

I would appreciate your kindness in circulating the suggestion (6 copies enclosed) and making the motion in my absence.

Cordially thanks,

Bernie Lowy

A Motion To Alter Editorial Policy

A mild editorial preference has been disregarded by many authors over the years. Under "Literature cited" (Instructions to Authors), authors are given the choice of citing references either by number "(preferred since it is less costly) or by author-date."

Because of increasing publication costs it might now be appropriate, with the advice and consent of the Editorial Board, to suggest the following change in editorial policy in the interests of economy as well as uniformity.

I propose the following as a motion to be considered at the next meeting of the MSA Editorial Board:

In "Instructions to Authors" under the heading "Literature cited" substitute for the first two sentences* the following sentence:

Cite references in the text by number and arrange them alphabetically by author.

The remainder of the paragraph is to remain unchanged.

*The two sentences presently read as follows: "Cite references in the text by number (preferred since it is less costly) or by author-date. Arrange references alphabetically by author regardless of the system used."

B. Lowy

original - BL

A Motion To Alter Editorial Policy

A mild editorial preference has been disregarded by many authors over the years. Under "Literature cited" (Instructions to Authors), authors are given the choice of citing references either by number "(preferred since it is less costly) or by author-date."

Because of increasing publication costs it might now be appropriate, with the advice and consent of the Editorial Board, to suggest the following change in editorial policy in the interests of economy as well as uniformity.

I propose the following as a motion to be considered at the next meeting of the MSA Editorial Board:

In "Instructions to Authors" under the heading "Literature cited" substitute for the first two sentences* the following sentence:

Cite references in the text by number and arrange them alphabetically by author.

The remainder of the paragraph is to remain unchanged.

*The two sentences presently read as follows: "Cite references in the text by number (preferred since it is less costly) or by author-date. Arrange references alphabetically by author regardless of the system used."

B. Lowy

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

11 June 1979

Dr. B. Lowy
Mycological Herbarium, Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Dear Bernie,

I am indeed sorry that I neglected to inform you of the progress of the manuscript by Dubovoy and Muñoz. It was reviewed by a geneticist, revised, and accepted for publication in MYCOLOGIA. It is in the July-August issue, and I've recently read galley proof. The department down there agreed to pay for costs of publication, so that's the reason why I forgot about informing you.

Along with Petersen's manuscript for review, and my general memo to members of the Editorial Board, I also include a copy of a brief article submitted for publication by Liu Bo, from mainland China. He is not a member, and would be willing to join MSA, but cannot get exchange to do so. I would like to publish the ms, if acceptable, as a contribution to communication. Clark agrees with me, and suggested that if we had a couple of confirmatory answers from Editorial Board members, we could do so. Therefore, will you let me know if you would be agreeable to publish? And, would you kindly serve as reviewer? I have only the originals of figures, separately, and will have to assemble them into two small plates; unfortunately when I xeroxed them I see one was reversed.

Thanks very much.

Sincerely,

Margaret

M. E. Barr Bigelow

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

7 June 1979

To: Members of Editorial Board of MYCOLOGIA

From: M. E. Barr Bigelow

I would like to thank you for your help and advice on several topics and for reviewing manuscripts during the past year.

I plan to meet with members of the Editorial Board during the AIBS meetings in August. Last year's meeting was most productive and I hope that you will all be present. By necessity, it will be held after the Council Meeting of MSA, so there are two matters which should be discussed or approved by mail:

1) Suggestion(s) from the Editorial Board to the Council for Editor-in-Chief, 1980-85: — If this comes to a vote, I can't nine for Johnson. BL
Terry Johnson
~~Mel Fuller~~ has declined to be nominated.
Are there other names to put forward?

2) List of members for 5-year term on the Editorial Board.
(K. Wells term ends in 1979)
From last year's Council-approved list: O. R. Collins, R. D. Goos, C. E. Miller, R. Petersen, L. Weresub, R. Humber
Other members suggested by Council: H. H. Burdsall, D. R. Reynolds, D. J. S. Barr
One volunteer: J. Ginns

Other matters to be considered at our meeting: Please see attached sheet
(If you are not able to attend, please give opinions by mail.)

Book-review editor: R.P. Korf's term ends in 1981. We need to consider names to forward to the MSA Council in 1980. — Lulla Weresub

A few remarks and suggestions from members:
Brief articles -- too many and these too long in some cases. Can we come up with some ruling that might help? — 5-page limit including Lit. cit.

Question of monthly vs. bimonthly issues, to speed publication. (Additional assistance would surely be needed.) — At authors discretion

Inclusion of lists of publications with biographies.

Publication of results of Whisler report. — Sorry, that I know nothing about it!

Are there other items to be discussed?

BL

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

11 June 1979

Dear Bernie,

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

Gloeomucro, gen. nov. and a note on Physalacria concinna
by R. H. Petersen

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

I see he forgot legends - have asked for them.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received: 14-VI-1979

Returned: 22-VI-79

Comments on "Gloeomucro, gen. nov. ....." by R.H. Petersen

p.5 The footnote "Colors ....." may be eliminated by placing it parenthetically following par.1.

The footnote "E = ..." should appear on p.2 where these notations are first used.

BC

Comments on "Two new species ....." by Bo Liu

For indexing purposes it should be noted that the author's last name is Liu, although given first in the paper, according to established Chinese custom.

Figs. 1 & 3 refer to Tulostoma reticulatum and figs. 2 & 4 to T.costatum.

p.2, l.2 Following "....Tulostoma opacum Long" it would be appropriate to cite the author's paper since no other reference is made to the literature. ie Long, W.H. 1944. Studies in the Gasteromycetes X. Seven new species of Tylostoma. Mycologia 36: 318-339. This of course should be under Literature Cited.

I recommend the publication of this paper.

Two new species of the genus Tulostoma from China

Liu Bo

(Department of Biology, Shansi University,
Taiyuan City, the People's Republic of China)

This paper discuses two new species of Tulostoma which collected from Inner Mongolia Autonomous Region, the People's Republic of China.

1. Tulostoma reticulatum Liu sp. nov. Fig. 1, 2

Sporocapo depresso-globoso, 9-15mm alto, 13-25mm lato. Exoperidio pulverulento, nec toto secedente. Endoperidio albido, membranaceo, levi. Ore subfibrilloso, 1.5-3mm diam. Stipite 1-5cm alto, 0.25-0.4cm crasso, subbrunneo. Sporis globosis vel subglobosis, 5.1-7μ diam. Episporio flavo-browneo, reticulato.

Sporophore having sporocarp, stipe and swollen base. Sporocarp depressed-globose, 9-15mm high by 13-25mm wide, firmly attached to stipe apex. Exoperidium granular, persistent or partially deciduous in age. Peridial sheath a broad band of hyphae and sand, 5-11mm wide, persistent. Endoperidium membranous, white, smooth. Mouth subfibrillose, with very scanty fibrils, plane or slightly raised, circular to elliptical, 1.5-3mm wide. Collar inconspicuous, about 1mm from stipe. Stipe light brown, striate to sulcate, terete, with a few fibrillous or lacerate scales, 1-5cm long by 2.5-4mm thick, with a swollen base. Occasionally volva present, but inconspicuous. Gleba ferruginous. Capillitium hyaline, thick-walled, even, 3.8-9.5μ thick, ends rounded, sparsely branched, septa transverse or oblique, rare, stronly swollen, colored. Spores globose to subglobose; some spores apiculate, 5.1-7μ in diam. Epispore 1.5μ thick, light yellowish brown, reticulate.

Habitat: Solitary or gregarious in open, unshaded sandy soil.

Distribution: Sonid Right Banner, Inner Mongolia Autonomous Region, People's Republic of China. Sept. 14. 1978. Li Zongying & Li Jizan, 3 plants in Herb. Coll. no. 4 (Herb. no. 3456); Sept. 15. 1978. Li Zongying & Li Jizan, 10 plants in Herb. Coll. no. 5 (Herb. no. 3453); Sept. 15. 1978. Li Zongying & Li Jizan, 35 plants in Herb. Coll. no. 6 (Herb. no. 3460, Type).

Gloeomucro, gen. nov. and a note on Physalacria concinna.

Ronald H. Petersen

Botany Department, University of Tennessee, Knoxville, TN 37916

Summary

The type specimen of Myxomycidium pendulum belongs in Mucronella, but the residue of Myxomycidium is renamed Gloeomucro. Physalacria concinna is described from the Appalachian Mountains and its fruitbody ontogeny is traced.

Massee (1901) proposed Myxomycidium pendulum based on a specimen from Tasmania. In subsequent years several additional species were added, and Kobayasi (1963) has furnished a key to the taxa in the genus.

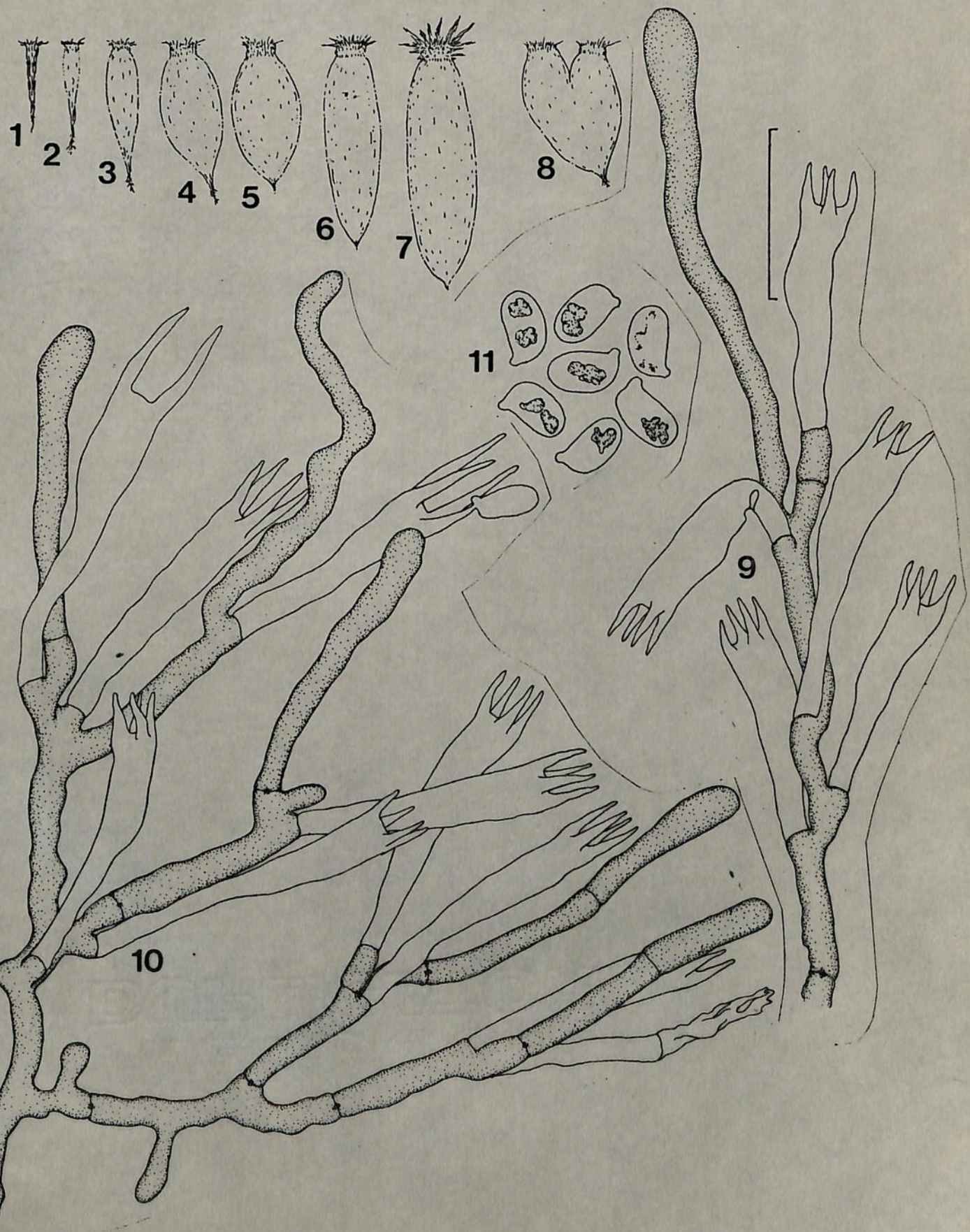
A recent collection of a member of the genus (see below) led to questions concerning possible heterogeneity within the group. In turn, collections made previously in Washington (state) and Australia were compared with the type specimen of M. pendulum at Kew. All these collections were contaxic, and all bore amyloid spores, undifferentiated tramal hyphae and fruitbody morphology of Mucronella. Because the type specimen of the type species of Myxomycidium belongs in a previously published genus (Mucronella), the name Myxomycidium must be included as a synonym of Mucronella. A description of the species follows:

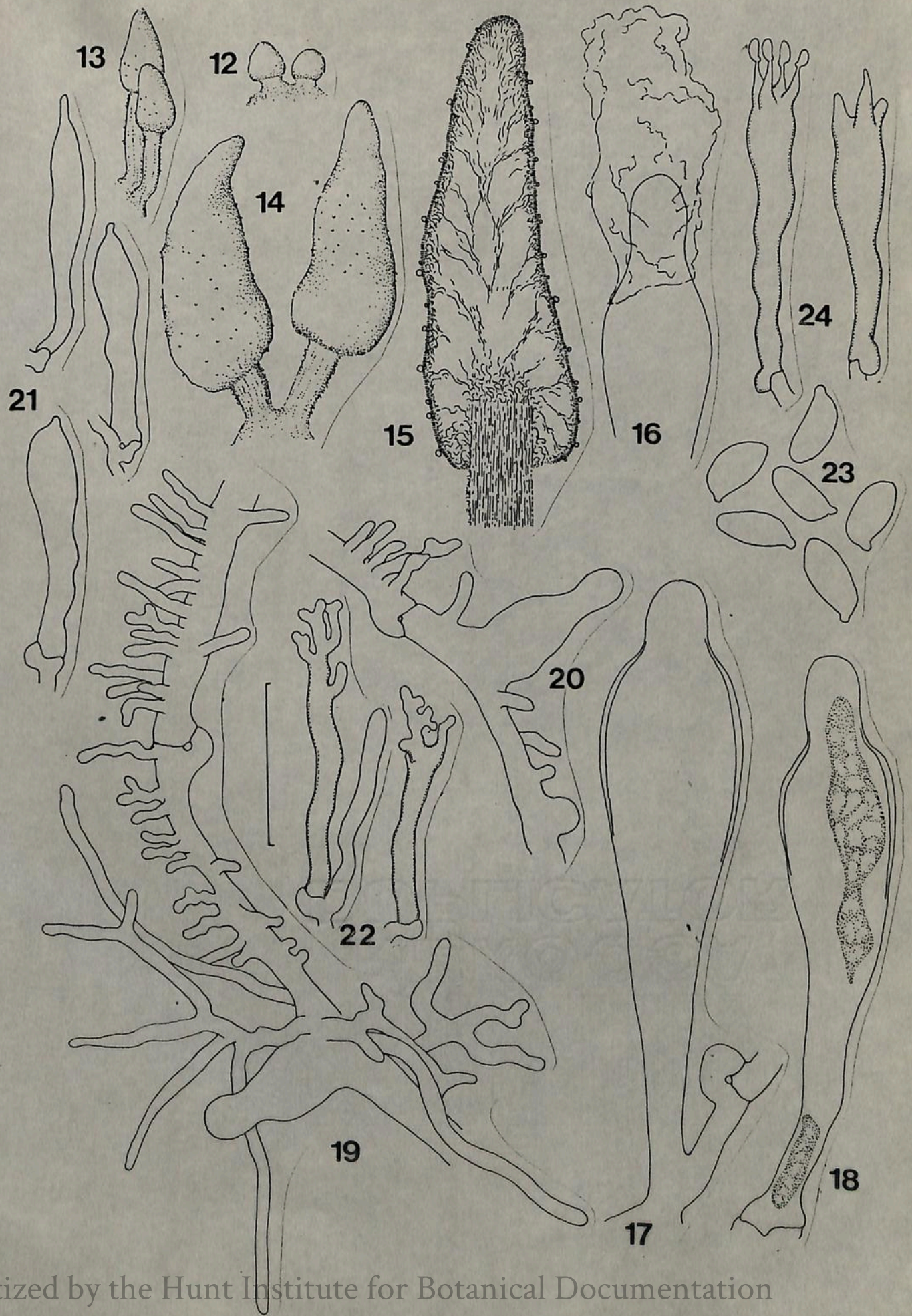
Mucronella pendula (Massee) Petersen, comb. nov.

Bas.: Myxomycidium pendulum Massee. Kew Bull. 1899: 180. 1901.

= Mucronella alba Lloyd. Mycol. Notes 61: 880, fig. 1509. 1919.

Fruitbodies up to 8 mm long, up to 3 mm thick, single, gregarious or occasionally caespitose or fasciculate in 2's or 3's, arising ageotropically





LSU
LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE, LOUISIANA 70803

College of Arts and Sciences
Department of Botany

24-VII-1978

Dear Margaret,

I am sending the enclosed paper for your consideration as a "Brief Article" in Mycologia. In spite of the numerous language corrections that were necessary, I think the paper is sound. Dr. Dubovoy was a student of Dr. Raper's at Harvard and she earned her Ph.D. there in 1973 working on the genetics of Schizophyllum.

When I spoke to Dr. Dubovoy at the Tampa meetings, she mentioned the paper she was preparing and said she would like to submit it to Mycologia for publication. I subsequently heard of her death (she was 34 years old and declined rapidly following a nearly fatal lab fire in Mexico) and have just now received the manuscript that was evidently completed by one of her students Alfredo Muñoz. This is the background pertaining to the enclosures.

If you or another reader also consider the paper acceptable, you may want to return it to Mexico for retyping. Whatever your decision may be, please let me know of it when you are ready. I'll also be interested in hearing what action you and the Editorial Board decide to take on the Ott-Bigwood commentaries concerning which I wrote you last week.

With best regards,

Bryant
B. Lowy

a "... sexuality Schizophyllum commune Fr."

within about a month I expect to be in Mexico City. Could you please tell me whether or not this paper has been accepted?

BL
30-V-'79

Dr. B. Lowy Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003 22 May 1979

Dear Bernie,

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

Notes on Podaxis argentinum from North America
by K. H. McKnight and M. Stransky (Brief article)

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received: 29-V-1979 Returned: 30-V-'79

Comments on " .....Podaxis argentinum ....." by McKnight & Stransky

In comparing P. argentinum and P. pistillaris, the presence or absence of a germ pore and the size of spores appear to be critical. The authors do not indicate spore size for P. pistillaris, only stating that "small spores" (p.4, l.20) are characteristic of P. argentinum. Spore measurements of these and related species could easily be summarized in tabular form for comparison with those given for P. argentinum (or those presumed to belong to this species).

Although Fig. 8 shows spores of P. pistillaris, only approximate magnifications are given and it is disconcerting that all 5 SEM figures of spores are at different magnifications.

Since the authors only "tentatively" (p.1, l.9) identify the Bahama collection as P. argentinum the title to this note might more appropriately read: "Podaxis argentinum Speg. in North America ?"

B. Lowy
29-V-'79

Dr. B. Lowy Edit. Board
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003 21 May 1979

Dear Bernie,

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

A new Steccherinum (Aphyllophorales, Stecchèrinaceae) from Alaska
by J. P. Lindsey and R. L. Gilbertson (Brief Article)

OK as is

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received 29-V-79

Returned 30-V-79

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

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

19 April 1979

Dear Bernie,

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

Formation of secondary sclerotia in sporophores of Typhula
by A. A. Christen (Brief article)

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

Received 25-IV-'79

Returned 1-V-1979

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

10 March 1979

Dear Bernie,

Recently I received a letter from Mel Fuller, saying that he had decided he could not be considered for editor of Mycologia. He cited, with good reason, the vast amount of extra mycological labors that he has been involved in for the past several years. He believes that he must now remain with teaching and research for a time.

This leaves us still with Terry Johnson, and I believe that he would be an excellent person for editor. However, I will ask if you have any other candidates to suggest? Within the next two months I will be preparing a memo on this and other topics, but wanted to get some input first. I will be interested to know of other possible persons.

Sincerely,

Margaret

M. E. Barr Bigelow

Margaret — If Terry Johnson is willing to take the job I think your search has ended.

BC
20-III

Dr. B. Lowy, Edit. Board
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

10 March 1979

Dear Bernie,

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

A method for production of Mutinus caninus fruit bodies in culture
by S. L. Flegler (Brief article)

If so, please prepare two copies of your criticisms, 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. Kindly return the paper with your comments promptly.

The following points should be considered:

✓ 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 value?

5. Have the tables been prepared as clearly and concisely as possible?

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

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor-in-chief

OK as is.

BL

Received: 16-III-'79

Returned: 20-III

Dr. B. Lowy Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

7 January 1979

Dear Bernie,

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

A new species of Physalacria from south India
by K. V. Chandrashekara and K. Natarajan (Brief Article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 11-I-1979 Returned: 15-I-'79.

Comment on "A New Species of Physalacria ..." by
Chandrashekara and Natarajan

Line 2 of the Latin description refers to the height of the basidiocarp as "ad 2 mm." In the English version (line 3) this becomes "up to 1.5 mm tall." The data should correspond.

A few stylistic and other corrections have been made in the ms.

BL

Dr. B. Lowy Edit. Board
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003 25 November 1978

Dear Bernie,

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

Psathyrella typhae from cattails and pure culture
by E. L. Steward and D. A. Johnson (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

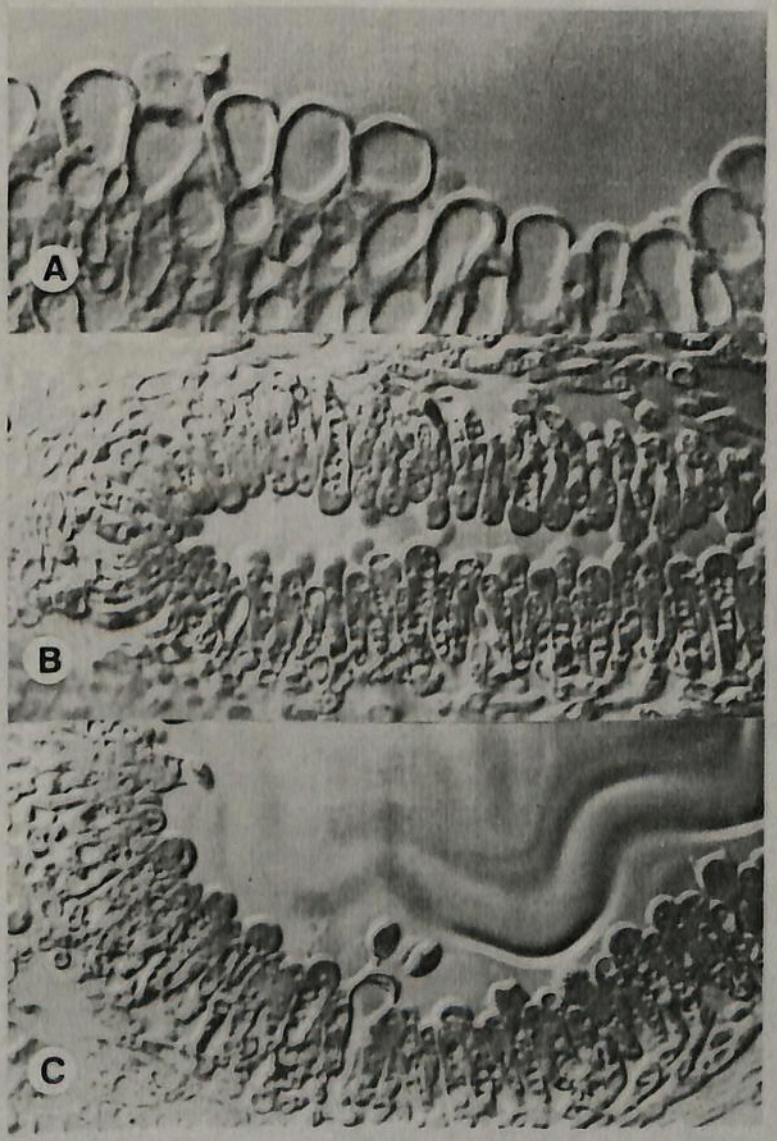
Sincerely,

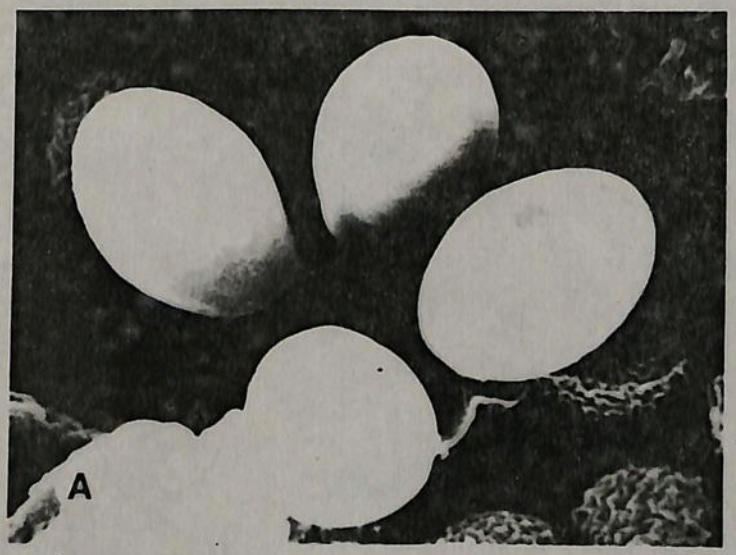
Margaret

M. E. Barr Bigelow
Editor

The excellent illustrations alone make this brief paper worth publishing. RC

Received 1-XII-1978 Returned: 4-XII-'78





Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

7 January 1979

Dear Bernie,

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

Temperature studies on Podaxis pistillaris

by S.M. Khan, D. A. Khan and R. H. Kurtzman, Jr. (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

M. E. Barr Bigelow
Editor

Received: 11-I-1979. Returned: 15-I-'79.

Comment on "Temperature studies on Podaxis pistillaris"
by Khan, Khan and Kurtzman.

Figures 1-4 should each bear the appropriate figure numbers.

18-VII-1978

Comments on Ott and Bigwood's statements

The vituperations of Ott and Bigwood, neither of them mycologists, are an unprecedented display of contumely against two of the world's most noted mycologists. Bigwood identifies himself (in an announcement for the "2nd International Conference on Hallucinogenic Mushrooms" held in Olympia, Washington Oct. 27-30, 1977) as a "photographer and ethnobotanist," and Jonathan Ott as a "freelance writer and chemist." Mr. Bigwood, in a previous circular also included among his credentials: "former President of Church of the Tree of Life." Both men presume to pass judgments on mycological matters, including among other innovations, the modest proposal that the rules of priority be changed to suit their pleasure. Quoting Ott: "I submit to the mycological world that there are occasionally flagrant situations where the rule governing priorities in nomenclature need not be observed." Ott now has the distinction of having provided the mycological world with the most risible statement of the century. Ott's insistence that Wasson "discovered the Mexican mushroom cults" is patently absurd, yet even on this nondebatable point he marshals "arguments" to show that Smith is "quibbling." A surprising example of sensationalism is Ott's citation of the CIA files to prove - nothing germane, except the already well known stupidity and deviousness of that agency. His use of that raw information (which in any case remains to be verified) in an attempt to smear Singer is vile. I do not intend to make a critical review of all of Ott's extravagances. That must be left to the judgment of Singer and Smith - should they consider it worth while. To sum up my opinion of the statements of Ott and Bigwood (particularly the former), I find them filled with innuendo, conjecture, hearsay, absurdities - and venom.

Every journal has the right to establish its criteria for the acceptance or rejection of articles submitted for publication. It is a place for reasoned arguments pro and con, but it is not obliged to publish unfounded, inflammatory allegations. It is part of the human condition to make honest mistakes, and Smith incorrectly cited some page numbers. This is the only "substance" I can find in Ott's attack. As a member of the Editorial Board, for the reasons I have stated, I believe that Ott's "Rejoinder" as well as Bigwood's "Comments" are unacceptable in their present form for publication in <u>Mycologia</u>.

B. Lowy

Dear Margaret,

I only recently returned from Guatemala/Mexico and have your request for an opinion on the Ott and Bigwood statements. A great deal more could be said - and probably will be - after Ott publishes his book, of which his remarks are a preview.

Since I'll not be at the meetings, I'll comment (as you suggest) on the agenda.

1-2. - My comments of 1975 still hold. (copy enclosed)

3. - As above, with same terms of appointment.

<u>Note</u>: Since I never became one, the function of a "Councilor" (of which there are now 5) has been a mystery to me. I presume it is honorific, but if so, I wonder what contribution (is any required?) JERRY W. RIFFLE has made to warrant his inclusion as a candidate? It bewilders me.

Bernie

18-VII-1978

Comments on Ott and Bigwood's statements

The vituperations of Ott and Bigwood, neither of them mycologists, are an unprecedented display of contumely against two of the world's most noted mycologis Bigwood identifies himself (in an announcement for the "2nd International Conference on Hallucinogenic Mushrooms" held in Olympia, Washington Oct. 27-30, 1977) as a "photographer and ethnobotanist," and Jonathan Ott as a "freelance writer and chemist." Mr. Bigwood, in a previous circular also included among his credentials "former President of Church of the Tree of Life." Both men presume to pass judgments on mycological matters, including among other innovations, the modest proposal that the rules of priority be changed to suit their pleasure. Quoting Ott: "I submit to the mycological world that there are occasionally flagrant situations where the rule governing priorities in nomenclature need not be observed." Ott now has the distinction of having provided the mycological world with the most ris ble statement of the century. Ott's insistence that Wasson "discovered the Mexica mushroom cults" is patently absurd, yet even on this nondebatable point he marshals "arguments" to show that Smith is "quibbling." A surprising example of sensational is Ott's citation of the CIA files to prove - nothing germane, except the already well known stupidity and deviousness of that agency. His use of that raw informati (which in any case remains to be verified) in an attempt to smear Singer is vile. I do not intend to make a critical review of all of Ott's extravagances. That must be left to the judgment of Singer and Smith - should they consider it worth while. To sum up my opinion of the statements of Ott and Bigwood (particularly the former I find them filled with innuendo, conjecture, heresay, absurdities - and venom.

Every journal has the right to establish its criteria for the acceptance or rejection of articles submitted for publication. It is a place for reasoned argume pro and con, but it is not obliged to publish unfounded, inflammatory allegations. It is part of the human condition to make honest mistakes, and Smith incorrectly cited some page numbers. This is the only "substance" I can find in Ott's attack. As a member of the Editorial Board, for the reasons I have stated, I believe that Ott's "Rejoinder" as well as Bigwood's "Comments" are unacceptable in their presen form for publication in Mycologia.

B. Lowy

Copy To Rolf Singer: 19-VIII-1982.

18-VIII-1978

Comments on Ott and Bigwood's statements

The vituperations of Ott and Bigwood, neither of them mycologists, are an unprecedented display of contumely against two of the world's most noted mycologis Bigwood identifies himself (in an announcement for the "2nd International Conference on Hallucinogenic Mushrooms" held in Olympia, Washington Oct. 27-30, 1977) as a "photographer and ethnobotanist," and Jonathan Ott as a "freelance writer and chemist." Mr. Bigwood, in a previous circular also included among his credentials "former President of Church of the Tree of Life." Both men presume to pass judgments on mycological matters, including among other innovations, the modest proposal that the rules of priority be changed to suit their pleasure. Quoting Ott: "I submit to the mycological world that there are occasionally flagrant situations where the rule governing priorities in nomenclature need not be observed." Ott now has the distinction of having provided the mycological world with the most ris ble statement of the century. Ott's insistence that Wasson "discovered the Mexica mushroom cults" is patently absurd, yet even on this nondebatable point he marshals "arguments" to show that Smith is "quibbling." A surprising example of sensational is Ott's citation of the CIA files to prove - nothing germane, except the already well known stupidity and deviousness of that agency. His use of that raw informati (which in any case remains to be verified) in an attempt to smear Singer is vile. I do not intend to make a critical review of all of Ott's extravagances. That must be left to the judgment of Singer and Smith - should they consider it worth while. To sum up my opinion of the statements of Ott and Bigwood (particularly the former I find them filled with innuendo, conjecture, hearsay, absurdities - and venom.

Every journal has the right to establish its criteria for the acceptance or rejection of articles submitted for publication. It is a place for reasoned argume pro and con, but it is not obliged to publish unfounded, inflammatory allegations. It is part of the human condition to make honest mistakes, and Smith incorrectly cited some page numbers. This is the only "substance" I can find in Ott's attack. As a member of the Editorial Board, for the reasons I have stated, I believe that Ott's "Rejoinder" as well as Bigwood's "Comments" are unacceptable in their presen form for publication in Mycologia.

B. Lowy

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

12 June 1978

To: Members of the Editorial Board, MYCOLOGIA

From: M. E. Barr Bigelow

Attached is a manuscript copy "Jonathan Ott's Rejoinder to Alexander H. Smith." I would appreciate your considered opinion on publication of this, in whole or in part.

A second manuscript, in the form of a letter to the Editor, and also intended for publication in MYCOLOGIA, is included for your comments.

Neither writer is a member of the Mycological Society.

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

12 June 1978

To: Members of the Editorial Board, MYCOLOGIA

From: M.E. Barr Bigelow

First, let me thank each one of you for your considerable efforts in reviewing numerous manuscripts during the past year. Your suggestions and comments are extremely helpful.

Would a meeting of the Editorial Board on Monday 21 August 1978, at 7:30 PM, be possible? I recognize that the AIBS Plenary Session is held on that evening, but suitable times are few. (Saturday and Sunday, MSA Council meetings; Tuesday MSA Social; Wednesday Workshop in Aeromycology.) Are there any other suggestions about timing?

Should you be unable to attend AIBS, would you provide input on the following matters on the agenda? Any additional suggestions or topics will be appreciated.

1. Suggestions for names of additional Editorial Board members. (Modification submitted by J. D. Rogers to C. T. Rogerson and approved by the New York Botanical Garden for 15-member editorial board, elected by members of MSA, each with a 5-year term, to be reached in 1982; starting with 6 in 1977, 8 in 1978, 10 in 1979, 12 in 1980, 12 in 1981, 15 in 1982 and thereafter.) Three members will be needed this year. I would propose S. Bartnicki-Garcia for a second term.

2. Consideration of criteria for selection of the next Editor-in-Chief (to begin working in 1980). Any nominations? Approximate time required runs from 15 to 25 hours per week. Suggestions made: willingness to accept the position, ability to meet requirements.

3. Book review editor. Who should appoint, editor or editorial board? What should be the term of appointment?

4. Concept of sectional headings of articles in issues indices to journal.

5. Is distribution of articles published a reflection of interests in MSA membership?

6. Implementation of any changes accepted by the MSA Council at their meetings on 19 and 20 August.

Margaret

[Published Dec. 1978 as a separate paper in Botanical Mus. Harvard Univ. 11 p.]

Jonathan Ott's Rejoinder to Alexander H. Smith

You will allow me, of course, to reply to Dr. Smith's Brief Article (Mycologia 69: 1196-1200) on the 'hallucinations of those who study the hallucinogenic agarics,' by which he means Dr. Gastón Guzmán and me. As I am not a mycologist, and since my contribution to the two papers under discussion (Mycologia 68: 1261-7; 1267-72) was strictly chemical and ethnological, I leave to Dr. Guzmán to comment on Smith's mycological observations, if he feels it worth his while. The ad hominem attack on us is, so far as I know, unprecedented in your pages, and I consider this a matter of utmost gravity.

Smith refers to a footnote on page 31 of my Hallucinogenic Plants of North America (1976) and says that in it I mistakenly refer to Mycologia 50: 163-164. I made no such reference. He then goes on to say that I "obviously" meant to cite Mycologia 50: 141-142. I obviously meant nothing of the kind. My reference was to (and I quote):

(163,164)

Throughout my book such references (and they occur on almost every page) are to my bibliography that comes just before my index: clearly Smith must have read (or rather misread) of my book only the footnote on page 31. What reason had he to think I referred to Mycologia 50: 163-164? Smith makes an embarrassing (for him) mistake. At the end of his paper, he even warns your readers to check my writings for accuracy, and says in his third papragraph that my application of "slipshod" to Singer is a fortiori applicable to me! Smith's careless haste to

denounce me boomerangs on him, and I am afraid your careful readers will link him inevitably with Singer's slipshoddiness. But Mycologia is a serious journal and Smith a celebrated mycologist. He owes me an apology. Your readers will note that Smith makes no effort to clear Singer of my charge of "slipshod."

Smith also takes me to task for saying that R.G. Wasson "discovered the Mexican mushroom cults." But Wasson was the first outsider (1) to attend a Mesoamerican shaman's night-long singing ritual, (2) to tape the singing, transcribe the Mazatec text, have it translated into Spanish and English, and publish it (Wasson, et al. 1974) with musical score and explanatory notes, comment, and superb photos (this is the first time a shamanic performance has been given such full-dress treatment), (3) to ingest the mushrooms, and (4) to describe accurately the subjective effects of the singing and the hallucinations. Smith is quibbling, and the alleged inaccuracies between him and me are all on his side: he fails to point out a single one that is mine.

Smith speaks with marked respect of Wasson's presentation to the "English-speaking world" of the hallucinogenic fungi as "thorough and scholarly". But he has changed his mind about Wasson. Donald P. Rogers gave a "thorough andscholarly" review of the Wassons' Mushrooms, Russia and History (1957) in Mycologia 50: 147-148. Surprisingly, you published a second review of it by Smith (Mycologia 50: 449-452), which he must have volunteered and which was a concentrate of nitpicking. Written in anger, it was unworthy of its distinguished author, and it elicited a sharp rejoinder from Heim (1958b). I welcome Smith's present change of mind. Wasson when he invited Heim to join him in Mexico chose wisely: a world-renowned scientist of the broadest gauge, de haute classe,

quick to perceive the far reaches of ethnomycology. It is surely an oversight - perhaps with a touch of parochialism - that leads Smith, in his attack on me, to ignore Heim when he says that Wasson brought hallucinogenic mushrooms to the attention of the English-speaking world. Heim and the Wassons, jointly and severally, were addressing the whole world in innumerable papers, lectures, and broadcasts, in a number of languages, and in the superb Mushrooms, Russia and History and Les Champignons Hallucinogènes du Mexique (1958). Heim and the Wassons recovered the mushroom rite in the nick of time, just as it was disappearing, and they gave to the world (not merely the English-speaking element in it) this exciting and important chapter in the history of mycology, in the history of culture.[1]

Why did Smith not go to the heart of my footnote, the gravamen of my charge? Why his concern over two minutiae, where he was twice wrong? I was justifying my use of the name Psilocybe Wassonii Heim rather than P. muliercula Singer and Smith, even though under the rules of nomenclature the latter has priority. As we are dealing with the birth of ethnomycology, it is important that we nail down the facts before they are lost forever. I dare to hope that in the interests of scholarship, Smith, Singer, and others, perhaps by correcting me, will tell us what happened at that time. I have no wish to malign either of these senior mycologists.

The Wassons turned their attention to Mexico in the fall of 1952 and they made their first field trip there in 1953, their second in

1. I take this opportunity to inform your readers that I have started to assemble data for a history of the early years of ethnomycology. One chapter will deal with Singer's "field trip" to Mexico in the summer of 1957. I will here try to reconstruct that "field trip" from many sources, chiefly Mexican, but including also CIA files that have lately become available to me.

1954. As they were not mycologists, they sought the collaboration of Roger Heim, the Director of the Laboratoire de Cryptogamie, later also Director of the Muséum National d'Histoire Naturelle for almost 15 years, and for a term the President of the Académie des Sciences. Incidentally, he specialized in tropical and subtropical fungi, and was for many mycologists the leading authority on them. Heim appears to have seized at once the importance of "ethnomycology", a term the Wassons were the first to use. In 1955, the Wassons made their famous breakthrough, uncovering (pace Smith) the secret rituals that had been going on for centuries, doubtless millennia, among the native peoples of Mesoamerica, in which mushrooms had served as the mediators between the Indians and their gods. Heim, on the Wassons' invitation, joined them in the field in 1956 and they made many trips together thereafter. Heim's first progress report appeared in the Comptes rendus of the Académie des Sciences on February 20, 1956 (Heim, 1956a), relating to the Wassons' trip in 1953. On March 12, 1956 he published a second paper (Heim, 1956b) covering the Wassons' expeditions in 1954 and 1955. He also began to publish papers in his Revue de Mycologie and many others journals.

In remote Tucumán, Argentina, where Rolf Singer was stationed, he caught these two papers of Heim's and thenceforth read avidly his publications on the subject. In 1956 or early in 1957, he received a grant from the "Bertram and Roberta Stein Neuropsychiatric Research Program, Inc. (Not for Profit)" of Chicago to study the psychotropic fungi of Mexico. In the spring of 1957, Singer flew from Argentina to the U.S.A.; later from U.S.A. to Mexico. I hope he will amplify the record by supplying us with the dates. I gather from Mexican sources that this was

Singer's first trip to Mexico, and it lasted all of a fortnight. It would be helpful if he would supply us with his itinerary. He was not in Mexico again for twelve years, when he made a second, equally brief trip. I shall appreciate it if Singer will say whether this is correct. There are lacunae in my data: I have encountered a note dated "July" (1957) that indicated a "Dr. Singer" went into the offices of the Aerovías Rojas, in the city of Oaxaca, and asked one of the Rojas brothers (Guillermo or Jaime) about flights to a series of remote mountain villages in the south of Oaxaca where the Heim-Wassons had been. These villages were inaccessible to Cessna aircraft and Singer thereupon gave up: the trips would have taken too long. Is this reported trip to Oaxaca accurate? If so, will he give us the date? From Mexico City he went to Huautla de Jiménez, where he stayed some days. There he quickly contacted two of the Wassons' informants, the Mazatec Isauro Nava García and Dr. Salvador Guerra Beltrán. Wasson's article in Life had appeared on 13 May 1957 and Singer had pasted Heim's water-color illustrations of the mushrooms in a notebook, with Heim's descriptions of them from his early papers. I have been told that this notebook served as Singer's sole guide.

Singer returned to Mexico City and thence made a day-long trip to San Pedro Nexapa where Heim had found Psilocybe aztecorum Heim. Singer had no success there though he spoke to the Heim-Wassons' informants. Singer went to Tenango del Valle in a large company, again for a one day excursion. Heim had been there in 1956 and had bought a species of hallucinogenic mushroom in the market-place. The Wassons had obtained the same mushroom in 1955, and Heim described it provisionally as Psilocybe mexicana var. brevispora in one of the papers

that Singer had constantly before him (Heim, 1956b). Singer purchased the same species of mushroom in July 1957. (N.B. Singer did not collect this mushroom in the field, and knew of it only from the work of the Wassons and Heim).

I understand that Singer made a further "field trip" outside of Mexico City, one that took him where Heim and the Wassons had not been: Acapulco. How long it lasted I do not know, but it must have proved mycologically fruitless as Singer did not report on it even to Dr. Stein, his financial backer. Of the fortnight in Mexico, it seems as though Singer spent more than half of the nights in Mexico City and Acapulco, and perhaps one night in Oaxaca City. I may be wrong and if Singer will correct me, documenting the corrections, I shall be grateful. Except for Acapulco, if I am right, Singer trod carefully in the very footsteps of Heim and the Wassons, talking with their informants.[1] The only hallucinogenic mushrooms that he found and that were used by the natives were species that the Heim-Wasson team had collected. He neither ingested the mushrooms, nor attempted to observe their use by the Indians. The Wassons and Heim recognized the vital importance of doing exactly this.

As a field trip representing original work, Smith and Singer, both mycologists of vast field experience and being possessed of an ample sense of humor, will have to agree that those two weeks in Mexico were rather hilarious.

On 18 November 1957, Heim published in French a full description, thoroughly accurate, of the Psilocybe that he found in Tenango del

1. So closely did Singer follow these footsteps that he overtook them. He met Gordon Wasson for the first and only time in San Andres, Oaxaca, a remote village in the Mazatec country.

Valle and simultaneously announced his intention to name it after the Wassons. It is inconceivable that Singer and Smith did not see this paper. But with the aid of special grants from the National Science Foundation and the University of Michigan Herbarium, two articles, one by Singer and the other by Singer and Smith appeared out-of-order in the pages of Mycologia 50: 239-261, 262-303, preceded by the Latin description of Psilocybe muliercula Singer and Smith in the previous issue (50: 141-142). The Latin description appeared on 4 April 1958, and Heim's Latin description of P. Wassonii followed on 29 April. During the fall of 1957, Singer and Smith were sitting in Ann Arbor panting with impatience over the slow appearance of Heim's careful papers. This I learn from the CIA evidence that I have seen recently. Singer and Smith had no knowledge that their activities were under CIA surveillance. At least there is no evidence in the CIA documents that they did.[1]

For the record, Smith will surely see the need to justify the use of taxpayer's money (the NSF grant) and the Herbarium funds to further his unseemly haste to gain for himself and Singer a spurious priority at the expense of Heim and the Wassons.

In the light of the circumstances surrounding Singer's "field

1. The documents to which I refer are files of the "Geschichter Fund for Medical Research, Inc.". Thanks to a recent Freedom of Information Act judgment, we now know this to have been a cover for the CIA project called MKULTRA. The goal of this project was to obtain information about mind-altering drugs. Because Singer had gone to Mexico in search of a novel hallucinogen, he and Smith were placed under CIA surveillance. The Wassons and Heim suffered the same fate. Unbeknownst to them, a member of their 1956 expedition to Mexico was a CIA "contractor". Recent disclosures about MKULTRA have drawn attention to the deliberate poisoning of unwitting subjects with LSD and other drugs. This despicable activity is perhaps not so deplorable as the wasting of taxpayer's money to spy on European and American scientists, with the goal of adapting the fruits of privately-funded research to the perverse ends of the CIA.

trip" in Mexico, I submit to the mycological world that there are occasionally flagrant situations where the rule governing priorities in nomenclature need not be observed. The values that weigh in the instance under discussion will be compelling for those mycologists who have in mind the honor of their profession. Now that the facts are coming out, they will choose to call this species Psilocybe Wassonii Heim.

Nothing that I have said impugns in the slightest the outstanding ability in mycology of Smith or Singer. I do impugn Singer's professional manners and Smith's also as Singer's accessory. I have been told that Heim and the Wassons were deeply hurt by the episode but, pursuant to a wise old saw (which I choose not to quote in your columns), they remained silent.

In perusing the primary writings on the Mesoamerican mushroom cult, I have been struck by a conspicuous fact: we think of Heim and the Wassons as making up the whole team. But they were only the leaders and their writings are lavish in giving credit to others for collaboration in these important discoveries, all of these others being highly respected scholars in their own right. Singer and Smith are nowhere mentioned. Singer's whirlwind trip served him only as an acte de présence, to enable him to say he had been on the scene, even though after the fact. He could just as well have pillaged the Heim-Wasson writings in Ann Arbor.

Against this background, we read with horror the following in Rolf Singer's Agaricales in Modern Taxonomy (1962 ed., p.544; 1975 ed., p. 539):

> Studies carried out between 1957 and 1960 by Heim, Singer, V.P. and R.G. Wasson, have shown that the species concerned are of great ethnological and - what is more important - physiological and medicinal interest... (Italics mine)

This statement makes any informed reader unutterably sad. Uninvited, Singer has injected himself into the middle of the Heim-Wasson team. He leads his readers to think that he made a contribution to the ethnological, physiological, and medicinal discoveries, to which he contributed nothing, just nothing. If I am wrong here, I am sure Singer will tell us what he contributed. He has falsified the dates of the major discoveries, saying that they took place "between 1957 and 1960". The major ethnological discoveries were made in 1953, 1954, and especially 1955 and 1956. They were reported in Wasson's Life article in the issue of May 13, 1957. Singer, in remote Tucumán, was a nobody in those events. The "physiological and medicinal interest" in these Heim-Wasson discoveries began in the same years and they have been pursued, perhaps in expanding volume, ever since, with no definitive results to this day. The important chemical problems, which Singer neglects to mention, had already been placed by Heim in the capable hands of Drs. Hofmann, Kobel, Brack, and others in the Sandoz Laboratories in Basel, well before Singer was on the scene, where they were shortly solved and the results published. The reader aware of the circumstances (including the details of Singer's "field trip") is acutely embarrassed for Rolf Singer: that a man of his considerable ability in mycology is the author of a statement constituting his own lasting, ineradicable memorial to his own gross misbehavior vis à vis Heim and the Wassons. I suspect that Dr. Smith now regrets the company he kept in Ann Arbor in the fall of 1957, and if I am right, I respect him for his change of heart.

Literature Cited

Guzmán, G. and J. Ott. 1976. Description and chemical analysis of a new species of hallucinogenic Psilocybe from the Pacific Northwest. Mycologia 68: 1261-7.

---------- et al. 1976. Psychotropic mycoflora of Washington, Idaho, Oregon, California, and British Columbia. Mycologia 68: 1267-72.

Heim, R. 1956a. Les champignons divinatoires utilisés dans les rites des Indiens Mazatèques, recueillis au cours de leur premier voyage au Mexique, en 1953, par Mme Valentina Pavlovna Wasson et M R. Gordon Wasson. Comptes rendus Acad. Sci. 242: 965-968.

-------- 1956b. Les champignons divinatoires recueillis par Mme Valentina Pavlovna Wasson et M R. Gordon Wasson au cours de leurs missions de 1954 et 1955 dans les pays mije, mazatèque, zapotèque eu nahua du Mexique méridional et central. Comptes rendus Acad. Sci. 242: 1389-1395.

-------- 1957. Sur les Psilocybes hallucinatoires des Aztèques et sur le microendémisme des Agarics utilisées par les Indiens du Mexique à des fins divinatoires. Comptes rendus Acad. Sci. 245: 1761-1765.

-------- 1958a. Diagnose latine du Psilocybe Wassonii Heim, espèce hallucinogènes des Aztèques. Rev. Mycol. 23: 119-120.

-------- 1958b. Sur une grave erreur d'interpretation. Rev. Mycol. 23: 354-360.

-------- and R.G. Wasson. 1958(9). Les champignons hallucinogènes du Mexique. Muséum National d'Histoire Naturelle, Paris. 322 p.

Ott, J. 1976. Hallucinogenic plants of North America. Wingbow Press, Berkeley, California. 162 p.

Singer, R. 1958. Mycological investigations on teonanácatl, the Mexican hallucinogenic mushroom. Part I. The history of teonanácatl, field work and culture work. Mycologia 50: 239-261.

---------- and A.H. Smith. 1958. Mycological investigations on teonanácatl, the Mexican hallucinogenic mushroom. Part II. A taxonomic monograph of Psilocybe, section Caerulescentes. Mycologia 50: 262-303.

----------. 1958. New species of Psilocybe. Mycologia 50: 141-142.

Singer, R. 1962. The Agaricales in modern taxonomy. Cramer, Weinheim, Germany.

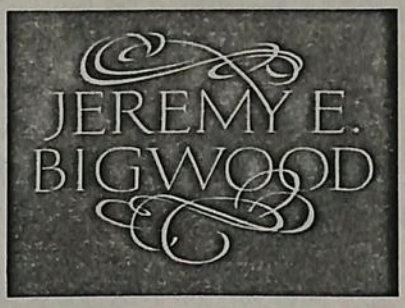
---------- 1975. The Agaricales in modern taxonomy. Cramer, Weinheim, Germany.

Smith, A.H. 1958. Mycologia 50: 449-452. (Review of Mushrooms, Russia and History)

Wasson, R.G. 1957. Seeking the magic mushroom. Life. May 13, 1957.

---------- et al. 1974. María Sabina and her Mazatec mushroom velada. Harcourt Brace Jovanovich, New York. 282 p.

Wasson, V.P. and R.G. Wasson. 1957. Mushrooms, Russia and history. Pantheon Books, New York. 2 vols., 435 p.



06 IV 78

To the Editors:

I would like to comment on Alexander Smith's recent Brief Article "Comments on Hallucinogenic Agarics and the Hallucinations of those who Study Them" (Smith, 1977) in which he takes issue with the work of Jonathan Ott and the mexican mycologist Dr. Gaston Guzman-Huerta. While Smith's piece seems more like a bad review than a scientific article, it is nonetheless provocative and revealing.

Smith seems eager to malign Ott for reasons that are obscure. He sallies forth, singling out a mere footnote from Ott's carefully written book (Ott, 1976) and hacks it apart for alleged inaccuracies. But Smith demonstrates only that he has misread Ott's text.

As anyone who gives Ott's book more than a cursory glance will realize, the numbers in parentheses refer to the alphabetized bibliography at the end. In his footnote on page 31, Ott correctly cites two papers from MYCOLOGIA Volume 50. Yet Smith would have us believe that Ott: 1) cited the wrong paper, 2) had the page numbers wrong, and 3) did not know who the authors were. It should be ovious to even the most myopic of mycologists who is wrong here. Yet, after constructing this strained series of alleged errors, Smith accuses Ott of being "slipshod"! Surely an apology is due Ott.

Smith also grasps at straws in attempting to fault two papers by Dr. Guzmán-Huerta and Ott (and unnamed co-authors) published in MYCOLOGIA Volume 68 (1261-7 & 1267-71). He challenges these authors' assertion that theirs was the first report of Psilocybe semilanceata from Washington, saying he himself collected it in 1941. Yet, he then admits that he did not report the collection and points to no report earlier than Guzmán-Huerta's and Ott's. What then, is wrong with the statement in the Guzman-Huerta and Ott paper?

Smith calls the illustration of Psilocybe pelliculosa "an outright atrocity" and, since he does not like a photograph of the same species, he states that it "could be either P. pelliculosa or P. silvatica." Besides being a non sequitur, this remarkable statement is not so much conjecture as inveterate fault-finding.

1323 E. Bay Drive
Olympia, WA
98506 943-6437

Like Guzmán-Huerta and Ott, I also wonder why Singer and Smith in their 1958 paper ignored P. semilanceata. Smith states that the toxicity of this species was not demonstrated until 1967 (actually, Hofmann and Heim published this finding four years earlier (Hofmann, Heim and Tscherter, 1963)), implying that he and Singer would have no reason to consider it. Remember, these authors were describing bluing species of Psilocybe at a time when the chemistry of psilocybian mushrooms was unknown. Yet, they unaccountably ignored P. semilanceata var. caerulescens Cooke which, whether or not we accept its varietal (or specific) rank, turns blue and was reported in 1902 to be toxic in a, although outdated, well-known text (McIlvane, 1902). Smith has given us no reason to cease wondering about the omission of this most cosmopolitan species of bluing Psilocybe from his 1958 paper.

Smith disagrees with Guzmán-Huerta in the matter of P. Subaeruginascens and P. venenata. He and Singer in 1958 placed these species in synonymy "with admitted reservations," namely, that they had not seen type material of the latter (Singer & Smith, 1958)! Guzmán, in his paper with Ott, bases his opinion on personal study of the type material of both species (Guzmán-Huerta and Ott, 1976). Who is being slipshod here?

One has less reason to trust Smith's judgment in this matter when examining the statements he and Singer made in their 1958 paper on the toxicity of P. venenata, which they admittedly didn't study. These authors claim that P. venenata "caused 10 deaths in 1929 alone" (Singer & Smith, 1958), citing Imai's paper (Imai, 1932). Singer and Smith merely misread the introduction to this paper, which attributed not a single death to this mushroom. I can find no evidence attributing a fatality to this mushroom, and would appreciate it if Smith would point to one. It would appear that Smith's supercilious opening statement about the "low level of accuracy developing in the literature on hallucinogenic mushrooms" comes from personal experience, his 1958 paper with Singer being perhaps the earliest example of this lamentable trend.

What with Smith's inability to understand Ott's bibliographic citations, his trifling nit-picking and fruitless fault-finding, his ineffective and circuitous mishandling of questions raised by Ott in regard to his 1958 paper, it would appear that he has failed to give us reason to mistrust Ott's work, as he warns us to do. Smith has, however, given the perceptive reader abundant reasons to mistrust his own work. What is going on here? Nothing in Ott's work or in Smith's "Comments" explains the latter's eagerness to criticize the former at all cost.

In closing, I cannot help but wonder at the wisdom of allowing Smith's personal attack on Ott to be published under the guise of a "Brief Article" in a serious scientific journal.

Guzmán-Huerta, G. and Ott, J. 1978. Description and chemical analysis of a new species of hallucinogenic Psilocybe from the Pacific Northwest. Mycologia 68: 1261-1267.

Hofmann, A., R. Heim and H. Tscherter, 1963. Presence de la psilocybine dans une espece europeanne d'Agaric, le Psilocybe semilanceata Fr. Comptes rendus Acad. Sci. 257: 10-12.

Imai, S., 1932. On Stropharia caerulescens, a new species of poisonous toadstool. Trans. Sapporo Nat. Hist. Soc. 12: 148-151.

McIlvaine, C., 1902. One thousand American fungi. Bowen-Merrill, New York.

Ott, J., 1976. Hallucinogenic Plants of North America. Wingbow Press, Berkeley.

Singer, R. and A. H. Smith, 1958. Mycological investigations on teonanacatl, the Mexican hallucinogenic mushroom. Part II. A taxonomic monograph of Psilocybe, section Caerulescentes. Mycologia 50: 262-303.

Smith, A. H., 1977. Comments on hallucinogenic agarics and the hallucinations of those who study them. Mycologia 69: 1196-1200.

cc: Dr. Gastón Guzmán-Huerta, México City, México.

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

DEPARTMENT OF BOTANY

27-VII-1978

Dear Margaret,

Sometimes I even misspell the word misspell! In my comments of 18-VII, the last line of the first paragraph has the non-word "heresay," which should read "hearsay."

Yours,

Bernie

Fifth Annual

Aspen Mushroom Conference

August 13 — 18, 1978

Sponsored by Beth Israel Hospital, Denver
Rocky Mountain Poison Center, Denver
Colorado Mountain College, Glenwood Springs, Colorado

LSU
LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE, LOUISIANA 70803

College of Arts and Sciences
Department of Botany

4-V-1978

Dear Margaret,

Just a note to let you know that if you have a ms. to send, it should reach me not later than 20-V. I am leaving the university for Guatemala on 27-V and do not expect to return before 10-VIII.

Best regards,

Bernie Lowy

Smith on Ott 1977

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

ALEXANDER H. SMITH

University of Michigan Herbarium, Ann Arbor, Michigan 48109

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

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

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

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

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

25 September 1978

To: Members of Editorial Board of MYCOLOGIA

From: M. E. Barr Bigelow

Meeting of Editorial Board members, 24 August 1978.

Present: H. E. Aldrich, S. Bartnicki-Garcia, E. E. Butler (new member, see below), C. T. Rogerson, M.E. Barr Bigelow

1. New members of Editorial Board.

Council of MSA directed that four disciplinary areas be represented: taxonomy - morphology; physiology - biochemistry; cytology - genetics; ecology - pathology.

A list of potential members, submitted to, added to, and emended by the Council members, was considered, ranked, and approved by the Council 19 August. The Editor was directed to ask the first three members to serve for a 5-year period:

1. E. E. Butler - present at meetings, asked, accepted.
2. J. Kwon-Chung - letter 29 August, accepted.
3. V. Ahmadjian - letter 29 August, accepted.
4. O. R. Collins
5. R. D. Goos
6. C. E. Miller
7. R. Petersen
8. L. Weresub
9. R. Humber

I suggest that we retain this list and those names added in #2, as well as any additional suggestions that may occur during the year, to present to the Council in 1979.

2. Additional members suggested 24 August:

H. H. Burdsall - forest pathology
D. R. Reynolds - ascomycete cytology, taxonomy
D. Barr - Chytridiomycetes taxonomy, physiology

Other names were suggested, but these were nonmembers.

3. The members of the Editorial Board present acted as an advisory committee to suggest possibilities for the next Editor-in-chief (to be presented to the Council of MSA in 1979 for approval; to begin work July 1980 on the 1981 volume of Mycologia). The editor was requested to approach two persons: M. T. Fuller and T. W. Johnson, Jr. for their reactions. Both have reacted favorably and have indicated willingness to be considered as Editor-in-chief. Now may I have some indication from the Board as to choice, or indeed, whether some other person(s) should also be considered.

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

4. Discussion of Ott and Bigwood manuscripts.
   a) Not to allow "letter to editor" of Bigwood.
   b) To invite Ott to submit correction only for publication in Mycologia or in the Newsletter.
   (Ott has replied (19 Sept.), with acceptance of the idea of correction and will submit this shortly.)

5. Review articles on specific fungi or areas of research: board approves in principle but retains right to review and reject if necessary. Board members may suggest authors; editor would invite.

6. Book review editor: suggested that we consider a 5-year term, but not one that changes at the same time as does term of Editor. (R. P. Korf began his term in 1972, second term then 1977-81.
(Presumably the Board suggests, Council approves.)

7. Sectional headings for articles in Mycologia:
Consensus that it is not necessary.

8. Distribution of articles by field:
Consensus that this would not be fair. Any member of the Board can suggest strongly to potential authors publication in Mycologia.

9. Proposal that full titles of Brief Articles be included in the Table of Contents of each issue, and in Index. This is especially useful for abstracting. Agreed; this system will begin in the January-February 1979 issue.

These minutes, although written shortly after return from the meetings, were held to get replies from several letters. Please send any suggestions, corrections, etc. to me for action or consideration.

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

11 April 1978

Dear Bernie,

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

Dictyophora multicolor, new to Guam by W. R. Burk and D. R. Smith (Brief article)

Sorry to send you another so soon, but you are too efficient!

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 17-IV-1978

Returned: 18-IV-1978

Comments on "Dictyophora multicolor ...." by Burk & Smith

The indusium of D. multicolor, which is described as having "... finer polygonal meshes ..." than D. indusiata, is virtually invisible in the figure, so it cannot serve to illustrate this diagnostic feature. The phalloid may be the species indicated, but the figure might just as well illustrate D. indusiata or a closely related species. The photograph should therefore clearly show the indusium. A ruler with a cm scale would also be more useful than the knife in the photo.

The authors state that D. indusiata (Pers.) Fischer var. typica Kobayasi forma aurantiaca Kobayasi "may be D. multicolor." Was the type examined, or is this opinion based upon Kobayasi's description?

I believe that the paper in its present form does not merit publication in MYCOLOGIA.

B. Lowy

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

29 March 1978

Dear Bernie,

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

Amanita phalloides and mushroom poisoning in Nigeria by B. A. Oso

Sorry about the marking on the ms. - only one copy was sent me originally, and the first reviewer marked generously.

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 4-IV-1978

Returned 4-IV-1978

AMANITA PHALLOIDES AND MUSHROOM POISONING IN NIGERIA

B.A. OSO

Department of Botany,
University of Ibadan,
Ibadan, Nigeria.

SUMMARY

There was a case of mushroom poisoning in which 12 people died in parts of the Imo State of Nigeria in July, 1977. Following Newspaper reports, a visit was paid to the area and people in the locality were interviewed on the incident. Specimens of the mushroom were collected and brought into the laboratory where they were examined and described. The mushrooms was shown by chromatographic studies to contain the amanita toxin. Features of the mushroom, symptoms of the poisoning and results of the chromatographic studies all show the fungus to be Amanita phalloides.

Comments on "Amanita phalloides and mushroom poisoning in Nigeria" by B.A.Oso

The extensive surgery already performed on this ms hardly allows room even for the application of a band-aid. In any case, it would be superfluous, and could only result in producing an incomrehensible palimpsest.

The suggested changes generally clarify the text, but I do not agree with the deletion of the descriptive paragraph on pp 4-5, which is critical. (For format, the author should refer to recent numbers of MYCOLOGIA.) In many particulars, the description is so unlike Amanita phalloides (ie pileus grey or light brown; gills becoming brown; umbo; pileus margin reflexed; gills "turning blue" ! etc.) that its identification as this species may seriously be doubted. A good photograph of one or more friuting bodies should be an integral part of the paper.

The ms should be returned to the author and resubmitted (in duplicate), incorporating the corrections and suggestions.

The paper is informative and of interest, and if the ms is properly prepared, it should be suitable for publication in MYCOLOGIA.

B. Lowy

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

31 August 1978

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

Stilbene dye labeling of basidiospores of wood decay fungi
by E. L. Schmidt and D. W. French (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 6-IX-'78 [Comment: Sans tache] Returned: 8-IX

Dr. B. Lowy Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01003

31 August 1978

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

Life-like preservations of some higher fungi
by J. W. Perry et al. (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 6-IX-78

Returned: 8-IX

Table 1

Schedule for preservation of fleshy fungi:

Treatment	Time (minimum)
FAA or FPA	24 hours
30% ethanol (EtOH)	several hours
50% EtOH	several hours
70% EtOH	several hours
100% EtOH	several hours
100% EtOH	12 hours
75% EtOH/25% Tertiary butyl acohol(TBA)	12 hours
50% EtOH/50% TBA	12 hours
25% EtOH/75% TBA	12 hours
100% TBA	12 hours
100% TBA	several hours
100% TBA saturated with paraplast	24 hours
100% paraplast (sealed)	24 hours
100% paraplast (open to oven atmosphere)	24 hours
Remove specimen, drain off excess paraplast in oven	1 hour

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

RICHARD P. KORF
Book Review Editor
Plant Pathology Herbarium
Cornell University
Ithaca, NY 14853

15 June 1978

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

Dear Bernie,

The following book has been received for review in MYCOLOGIA. Would you be willing to review the volume for the journal? You would, of course, keep the book.

ATLAS DE MICOLOGIA BASICA, by Miguel Ulloa and Richard T. Hanlin. 1978. Editorial Concepto, Mexico 13, D.F., Mexico. xxix + 158 p., 37 pl. Price, not stated.

If you would not be willing to review this, can you suggest another suitable reviewer that I might be able to approach? You are probably a better judge of this than I am.

If you are prepared to review the book, please drop me a letter and I shall send the book on to you immediately. Rising costs of publication have forced us to adopt a policy that reviews must be brief. I shall be forced to reject reviews that are longer than three double-spaced pages; I will welcome reviews that are a page long. Please follow recent book review formats in MYCOLOGIA for preparing your review. Of course everything including title, etc. must be double-spaced.

You may sign your review with just your name, or with your name and institutional affiliation. MYCOLOGIA will not assess page charges, but since we have not been able to program the printer as to procedure, you doubtless will receive a page-charge sheet from him along with the galley proofs; just ignore the page-charge sheet. You may, of course, order reprints if you want them.

Content of the review is solely the prerogative of the reviewer. Occasional reviews have been too personal or too biting, so I may make suggestions for change; whether you accept my advice is your decision. Please avoid interjecting unpublished contrary conclusions. Make sure your readers know whether you think the volume is worth their purchasing (or having their library find the money to buy it).

If you agree to review the book, you should plan to have the review back in my hands within six weeks of receipt of the book. If you cannot adhere to that time schedule, please suggest another reviewer instead. Long delays are unfair to authors and to publishers.

I look forward to hearing your decision.

Cordially,

Richard P. Korf
Book Review Editor, MYCOLOGIA

rpk/me

LSU
LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE, LOUISIANA 70803

College of Arts and Sciences
Department of Botany

14-VII-1978

Dear Dick,

I have just returned from Guatemala/Mexico and have your invitation to review the "Atlas de Micología Básica" by Alloa & Hanlin. I've been reviewing mycological works (also ethnomycological) for Economic Botany for about the last 10 years and think I can keep within the space-time limits you mention for Mycologia. As to "personal" or "biting" comments, occasionally some animadversions are in order, as in my review of Zoberi's opus in Economic Botany 28: 240. 1974.

If you have not already farmed out the book, I'll be glad to review it.

Yours,
Bernie

Proof returned to NY: 29-IX-78

To RBK: 27-VII-1978
for MYCOLOGIA

ATLAS DE MICOLOGIA BASICA, by M. Ulloa and R. T. Hanlin. Editorial Concepto, S. A., Mexico 13, D. F. 1978. xxix + 158 p, 37 pl. Price not given, paperbound.

This illustrated lab manual for introductory mycology has as its salient feature a graphic section with 18 plates of attractive drawings by Dr. Ulloa showing over 100 species of common fungi with which every tyro should be acquainted. In addition there are 233 photomicrographs, for the most part excellent, which should well serve their didactic purpose. Bibliographies conclude each brief chapter, following a systematic presentation of major taxa, and references are limited to the most essential that should be found in a working library. Diagnostic characteristics of orders and representative genera are given, together with a few spartan comments generally appropriate in a lab manual. A few of these however, may be more of a challenge than the student imagines. The directions for handling Auricularia auricula say (in translation): "Place a small portion of the basidiocarp in water and look for basida under the microscope." Unless special instructions are given here, the student may look a lot but find little. I also note with surprise that the best the authors can offer as a commentary on Amanita caesarea, which stands in the highest rank among delectable wild fungi, are these laconic words: "No es venenosa." It is not poisonous. This is rather like saying of a celebrated beauty, that she is not ugly. It may be strictly true, but it is also somewhat myopic, and beginners deserve a more discriminating appraisal. Hypomyces lactifluorum and Ustilago maydis, commonly eaten in Mexico, are also passed over without comment on their savory properties.

In my copy, it was disconcerting to find that after a few turns of the pages, the plates, which occupy the end of the book, began to separate from

the binding due to faulty gluing. An alphabetical index of genera and species lists about 550 names, and a very competent job of proofreading was done throughout the text. An English edition of the manual would be ~~very~~ useful and could most be strongly recommended for adoption in virtually any introductory course on taxonomy of the fungi. - B. Lowy, Botany Department, Louisiana State University, Baton Rouge.

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

2 August 1978

Dr. B. Lowy
Mycology Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

Dear Bernie,

Thank you for forwarding, with your editorial comments, the manuscript by Dubovoy and Muñoz. I am sending it to a reviewer with genetics background also, since I do not feel competent to judge the contents. I will inform you of his comments when these are available.

I do not find that Dr. Dubovoy was a member of MSA, one of the prerequisets for publication in MYCOLOGIA. Do you know if her department would consider paying costs of publication of the article?

Thank you also for your helpful opinions on editorial board matters. I am sorry that you will not be able to be present at the first of our meetings in Athens -- in fact only half of the editorial board will be there, so it will be a matter of opinions and suggestions, and not definite decisions.

I was quite entertained by the copy of the 1975 letter -- had heard rumors of the furor, and of the consequences, but had not any details. I should say that Howard has carefully dissociated himself from any editorial matters!

About your question on nominations for MSA Council -- we always get as many people as possible together to suggest names, sometimes come up with a winner, others not. I assume that others do the same, and like you, I cannot imagine why Jerry Riffle is one of choices.

Will send out a summary of editorial board discussion after the meetings.

Sincerely,

Margaret

M. E. Barr Bigelow

LSU
LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE, LOUISIANA 70803

College of Arts and Sciences
Department of Botany

23-VIII-1978

Dear Margaret,

I'm sorry that I failed to bring up the question in Tampa, but if the Debovay-Muñoz paper I sent you last month is accepted for publication, I believe that her Botany Department would be willing to pay the publication costs. However, I feel a certain responsibility in the matter, and if it develops that they can't pay the full costs, I'll contribute toward them myself. Of course, this is to be kept confidential.

Yours,
Bernard

VNIVERSIDAD NACIONAL AVTNOMA

INSTITUTO DE BIOLOGIA

June 19, 1978.

Dr. Bernard Lowy
Lousiana State University
Baton Rouge, Lousiana 70803

Dear Dr. Lowy:

Last year, during Tampa Congress, my professor Dr. Celia Dubovoy talked with you about the possible publication of one of our papers in "Mycologia".

How you perhaps know, Dr. Dubovoy died on that time, and now I would like that you read and criticize our paper for its publication or tell me which recommendations you have for it.

I want to say you that the paper has not the Mycologia fashion[1] but common style[2] yet.

I'll be gratefull with your opinion about the paper.

With my best regards.

Yours truly,

Alfredo Muñoz

Alfredo Muñoz Rivas

Alfredo Muñoz Rivas
Instituto de Biología
Departamento de Botánica
Apdo. Postal 70-233
México 20, D.F.

'pmr.

cc: Dr. Celia T. Dubovoy

LSU
LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE, LOUISIANA 70803

College of Arts and Sciences
Department of Botany

Mr. Alfredo Muñoz Rivas
Instituto de Biología
Departamento de Botánica
Apdo. Postal 70-233
México 20, D.F.

24-VII-1978

Dear Mr. Muñoz:

Your paper on "....sexuality of Schizophyllum commune Fr." written jointly with Dr. Dubovoy, and postmarked 7-VII-1978, has just arrived. I recall very well speaking with Dr. Dubovoy about the possibility of publishing her paper in Mycologia and I encouraged her to send it to me. I deeply regret her tragic death.

I have made certain linguistic changes in the text and shall foward the corrected manuscript to the Editor-in-Chief with my recommendation that it be published as a "Brief Article."

It is normal procedure for every paper submitted to Mycologia to be sent to 2 readers for criticism, and if any significant changes are suggested, the author is notified. You will receive word of the final acceptance or rejection of the paper from the office of the Editor-in-Chief.

With best wishes,

Sincerely yours,

B. Lowy

Bernard Lowy
Editorial Board of
Mycologia

1- 9pp + bibliography

LSU
LOUISIANA STATE UNIVERSITY
AND AGRICULTURAL AND MECHANICAL COLLEGE
BATON ROUGE, LOUISIANA 70803

College of Arts and Sciences
Department of Botany

24-VII-1978

Dear Margaret,

I am sending the enclosed paper for your consideration as a "Brief Article" in Mycologia. In spite of the numerous language corrections that were necessary, I think the paper is sound. Dr. Dubovoy was a student of Dr. Raper's at Harvard and she earned her Ph.D. there in 1973 working on the genetics of Schizophyllum.

When I spoke to Dr. Dubovoy at the Tampa meetings, she mentioned the paper she was preparing and said she would like to submit it to Mycologia for publication. I subsequently heard of her death (she was 34 years old and declined rapidly following a nearly fatal lab fire in Mexico) and have just now received the manuscript that was evidently completed by one of her students, Alfredo Muñoz. This is the background pertaining to the enclosures.

If you or another reader also consider the paper acceptable, you may want to return it to Mexico for retyping. Whatever your decision may be, please let me know of it when you are ready. I'll also be interested in hearing what action you and the Editorial Board decide to take on the Ott-Bigwood commentaries concerning which I wrote you last week.

With best regards,

Barnett

B. Lowy

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

6 August 1978

From M. E. Barr Bigelow

To Members of the Editorial Board

At the Saturday, 19 August, meeting of the MSA Council, one of the items of business should be appointment of three members to the Editorial Board. While the President and Council members may well have persons in mind, the members of the Editorial Board should have input also. The following list is compiled from suggestions of members of the Editorial Board and myself. Do you have any reservations about any of these, or do you wish to add to the list? (I will be in Amherst through 17 August.)

S. Bartnicki-Garcia (2nd term)*

V. Ahmadjian - lichens

J. Kwong-Chung - medical mycology

R. Humber (volunteer) - Entomophthorales, Myxomycetes

M. Larsen - forest pathology; thelephores

R. Petersen - Basidiomycete taxonomy; nomenclature

E. Trione - physiology; plant pathology

*unless elected to the Council of MSA. To avoid having anyone assume dual responsibilities, other names suggested are excluded from this list.

3-IV-1978

Comment on note by Grand and Lodge

This note, if amplified, might be appropriate for a State Academy of Science publication, but I do not think it is suitable for MYCOLOGIA.

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

27 March 1978

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

Occurrence of Boletus piedmontensis in North Carolina and Georgia
by L. F. Grand and D. J. Lodge (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

1. Is the material new and worthy of publication in MYCOLOGIA? (No!)
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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received 31-III-1978.

Returned 13-IV-'78

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

13 March 1978

To members of the Editorial Board of MYCOLOGIA

Last summer in Tampa, I inquired of those who presented General Lectures to members of the Second Mycological Congress, whether they would desire publication of their texts in MYCOLOGIA. Dr. Sparrow was most pleased to have his Presidential Address published, and fortunately had nearly completed revision of the text before his death. This address is now in press.

When Drs. D. C. Smith and H. Whisler were approached, they were hesitant. Dr. Whisler decided against publication because of the visual nature of his presentation. Dr. Smith has now sent me his manuscript. He states in his letter that he has some reservations on the merits of publishing the lecture, and that he would not be offended if it were not. I enjoyed reading it, and would like to see it in print. However, this is not in the usual vein of contributions to MYCOLOGIA, as you will recall. Do you have strong feelings about publication of such a lecture? Would you please inform me, so that I may in turn inform Dr. Smith?

Sincerely,

Margaret

M. E. Barr Bigelow

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

21-III-1978

Dr. Margaret E. Barr Bigelow
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Mass., 01002

Dear Margaret:

Unfortunately, I missed Dr. D.C.Smith's address at the Congress, but heard several enthusiastic reports about it. The fact that the paper may not conform to the usual pattern of publications normally accepted by Mycologia, would not deter me from including it if it was an outstanding contribution that simply had a novel approach. I recall a paper possibly in this category published by Ralph Emerson in Trans. Br. Mycol. Soc. 60: 363-387. 1973. It was illustrated with a number of striking cartoons, a device not generally associated with the presentations routinely found in that journal. I would favor the publication of a worthy paper by a distinguished (and in this case, apparently a quite innovative) botanist, even though it deviated considerably from tradition. I believe that we should maintain sufficient flexibility to allow for such cases, subject only to the judgment of the Editorial Board and to the final decision of the Editor-in-Chief.

Sincerely yours,

Bernard

Bernard Lowy
Member, Editorial Board

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

4 February 1978

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

Pseudocolus fusiformis: synonymy and distributional records (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 13-II-1978

Returned. 15-II-'78

Pseudocolus fusiformis: synonymy and distributional records.

William R. Burk

University of California, Library, Santa Barbara, CA 93107

To the distributional record of Pseudocolus fusiformis (Fisch.) Lloyd (cited as P. javanicus) by Burk (1976), North Carolina was added by Blanton (1976) for the United States along with a clarification of the valid epithet for this small phalloid. Two additional synonyms ~~will be~~ added to Blanton's list along with a are here review of the distributional records for this fungus outside of the United States.

The following 2 names should be included as synonyms of P. fusiformis:

= Pseudocolus jaczewskii IU. Voronov, Izv. Kavkaz. Mus. 11: 196-203; Fig. 1. 1918.

= Anthurus trifidus Harmand, Hariot, & Patouillard. Bull. Mus. Hist. Nat. (Paris) 8: 132. 1902.

Voronov's (1918) basis for describing P. jaczewskii was on specimens found around the Batum Botanical Garden in South Colchis in August 1917. The phalloids were found in a grove of Eucalyptus amygdalina Lab, and in an old bamboo stand. All of the 23 specimens he examined had 3 arms. Voronov stated that P. jaczewskii was very close in description to Colus javanicus Penzig and C. garciae Moeller. However, based on the descriptions in the literature, he could not identify his specimen with either name. He therefore erected the name P. jaczewskii ~~named after~~ commemorating A.A. Jaczewski. Vasil'kov (1954) stated that P. jaczewskii was obviously a synonym of P. fusiformis (cited as A. javanicus (Penzig) Cunningham). Pilát (1958) also

Comment on "Pseudocolus fusiformis ....." by William R. Burk

In a number of places the author indicates that the phalloid was collected beneath oak, beech, bamboo, Eucalyptus or other vegetation, which is appropriate and useful when such data are available. It would therefore seem pertinent to empahsize what is stated on the herbarium label in the figure, that P. jaczewski Woronow was collected "ad terram in cetreto," ie on the ground beneath citrus. This information could logically be inserted on p.2, line 3, following the word "....collected....."

B. Lowy

Editorial Board

Dr. B. Lowy
Dept. of Botany, Mycological Herb.
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

23 January 1978

Dear Bernie,

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

Taxonomic significance of septal ultrastructure in the jelly fungi
by R. T. Moore

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?

5. Have the tables been prepared as clearly and concisely as possible?

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

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 26-I-1978 Returned: 30-I-1978

TAXONOMIC SIGNIFICANCE OF SEPTAL ULTRASTRUCTURE IN THE JELLY FUNGI

Royall T. Moore

School of Biological and Environmental Studies,
The New University of Ulster,
Coleraine,
County Londonderry,
Northern Ireland.
BT52 1SA

Comments on "Taxonomic significance of Septal Ultrastructure in the Jelly Fungi" by Royall T. Moore

---

Why should a long discarded synonym for Auricularia auricula (Hooker) Underwood ie A. auricula-judae (Fr.) Schroet. be used? (pp. 5,6)

Three types of dolipore-parenthesome septa have been described in the fungi, one of them (vesiculate parenthesomes) confined to the single family Filobasidiaceae, originally believed to have had close affinities with the smuts. Now Tremella brasiliensis and T. mesenterica are shown by the author to have similar septation - in addition to sharing a Cryptococcus-like stage in the monokaryon phase. The extensive taxonomic rearrangement suggested, including the establishment of 2 new suborders (Tremellineae and Exidiineae) is based upon these limited observations. Although the author's arguments are cogent (except perhaps where he relies somewhat on support from precedent - pp. 13,14), the critical question is whether evidence is presently sufficient to justify the suggested taxonomic rearrangement. Most heterobasidiomycetous genera still await EM studies for confirmation of their septal types. Also, some points such as the regularity of occurrence and significance of imperforate, multiperforate or pauciperforate parenthesomes; presence or absence of a granule in the dolipore; "microtubular connections,") need to be evaluated after more data are available on a broader assemblage of genera and species. Additional variations may very well be discovered.

The author's paper does an important service in underscoring the need for continued EM studies of the heterobasidiomycete complex. His proposals for a new taxonomic approach certainly merit publication.

Dr. B. Lowy
Dept. of Botany
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

6 November 1977

Dear Bernie,

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

Notes on clavarioid fungi. XV. by R. H. Petersen

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received 10-XI-1977

Returned 14-XI-'77

Dr. B. Lowy Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

6 November 1977

Dear Bernie,

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

The type of Boletus fumosipes Peck by C. B. Wolfe, Jr. and R. H. Petersen
(Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 10-XI-1977

Comments on "Notes on clavarioid fungi" by Ronald H. Petersen

p. 1 - Referring to the author's opening sentence, it may be questioned whether a taxonomic character, however abstruse, is "farther removed from the senses" than is a simpler character.

p. 6, par. 4 - The propriety of using the French here is doubtful. One of the words, incidentally, is misspelled.

p. 14, penultimate par. - The meaning of the last sentence is fuzzy. Can "the branched habit in Ramariopsis" be either logical or illogical? It may be logical or illogical to place taxa into related or unrelated categories, based upon specific criteria. The sentence should be reworded.

These relatively minor points do not affect the main purpose of the paper. Whether his suggestions are eventually accepted or rejected, the author has done a service in stating his arguments and has pointed out both advantages and disadvantages in his proposed revision.

The paper on "The Type of Boletus fumosipes Peck by C.B.Wolfe, Jr. and R.H.Petersen is suitable as it stands for publication as a brief article.

Note: The numeral "2" on the illustration apparently dropped off in transit.

B. Lowy

Phone No. : 31703

DR. P. D. BADHE.

DEPT. OF BOTANY,
Nagpur University Campus,
Amravati Road,
NAGPUR-10.

Date. 22/..9...... 1977.

To,

Dr.B.Lowy,
Member, Editorial Board, Mycologia,
Department of Botany,
Louisiana State University,
Baton Rouge, Louisiana. 70803
U.S.A.

Dear Dr.Lowy,

Thanks for your letter dated 8.IX.1977. I have very carefully corrected the manuscript (considering your corrections which are mostly in the nature of punctuation and typological mistakes), of my article 'Bearing of Cytological, Developmental and Morphological characters on the Integration of Lichens with Fungi.'

However, I failed to understand the exact position of my article for publication in MYCOLOGIA. I may please be informed that, after appropiate revision, could it be published in MYCOLOGIA? I have made it very clear that as the article is about Classification and Integration of lichens with Fungi it should be published in MYCOLOGIA only. I am prepared to make necessary changes if found essential by the Editorial Board.

The note on Page14: I could not read some of the cross references because of the paucity of literature. However, inspite of the difficulties to procure literature, I managed to read them, and shall accordingly produce them in the manuscript.

I shall highly appreciate, if informed immediately regarding the same, so that I can send manuscript to you for publication.

With high sense of regards,

Sincerely yours,

P.D.Badhe

(P.D.Badhe)

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

10-IV-1977

Dr. P. D. Badhe
Post Graduate Botany Department
Nagpur University
Nagpur 440.010 (M.S.)
India

Dear Dr. Badhe:

I am sorry that in my last letter I did not stress the official policy of MYCOLOGIA, the pertinent part of which I now enclose. As I clearly suggested, however, the corrections I made in your manuscript were done with the idea that this might increase your chances of having it accepted in another journal, and this is still my view. I hope that you may be successful in acting upon it.

Sincerely yours,

B. Lowy

Bernard Lowy
Member, Editorial Board, MYCOLOGIA

Phone No. : XXXX

Dr.P.D.Badhe,
M.Sc.,Ph.D.
C.S.I.R.Pool Officer

DEPT. OF BOTANY,
Nagpur University Campus,
Amravati Road,
NAGPUR-10. (440 010)
M.S., India.

Date. Aug.22....1977.

To,

The Editor-in-Chief - Bernard Lowy,
Louisiana State University,
Baton Rouge,
Louisiana 70803 (U.S.A.)

Sir,

Please find enclosed a manuscript (in triplicate) entitled "Bearing of Cytological,Developmental and Morphological Characters on the Integration of Lichens with Fungi" by P.D.Badhe, for favour of publication in your highly esteemed journal 'Mycologia".

I should mention here that I am not a member of American Mycological Society. But my research, since 1970, is on Lichenological and Mycological Studies. I was member of American Bryological & Lichenological Society from 1971-73 and published some articles on Lichen Chemistry in the'Bryologist' since 1972. In this research article, lichenological and mycological problems with respect to classification of lichens and fungi have been discussed. Many research articles on the above topic had been published in Mycologia. I, therefore, consider this article should also be published in'Mycologia'.

However, if the members of the Editorial Board after approving this article, wish that I should become member of the Society, I may please be informed.

Awaiting to hear from you.

With regards,

Yours sincerely,

Encl:As above.

P.D.Badhe

(P. D. Badhe)

P.S.: Please, write the title of the article in reference No. 28 and 32 as I could not get, in available literature, here. Thanks.

Received 6-IX-77

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

8-IX-1977

Dr. P.D.Badhe
Post Graduate Botany Departmen
Nagpur Iniversity
Nagpur 440.010 (M.S.)
India

Dear Dr. Badhe:

I regret that your manuscript in its present form can not be published in MYCOLOGIA. I have made some corrections for your consideration, so that if you resubmit it to some other journal, there may be a greater possibility of its acceptance. The errors (I have marked some of the more obvious ones) are mostly in the nature of spelling, punctuation, and the like, so it is important to carefully proofread your manuscript. I am not certain of the intent of your note at the bottom of p. 14. If this means that the papers indicated were not available to you, then there is a question of the propriety of including such titles in the bibliography.

Sincerely yours,

Bernard Lowy
Member, Editorial Board, MYCOLOGIA

From : DR. P. D. BADHE
M.Sc., Ph.D.
Post Graduate Botany Department,
NAGPUR UNIVERSITY,
NAGPUR. - 440-010 (M.S.).
INDIA

3 copies MS returned with corrections
12 p. + illus.

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

28 September 1977

To: Members of the Editorial Board of MYCOLOGIA

From: M. E. Barr Bigelow, Editor-in-Chief

An inquiry by a member of MSA requires an opinion from members of the Editorial Board. This concerns the possible publication of five review-type papers presented by panelists at IMC2. Some members of the panel are not members of MSA. Should we accept these manuscripts? If so, on a nonreviewed basis? or as regularly refereed articles? yes.

Papers read at an International Mycological Congress by non-MSA members might constitute another category of those acceptable for publication, subject to the usual review procedures.

BL

Dr. B. Lowy . Edit. Board
Dept. of Botany, Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002 12 September 1977

Dear Bernie,

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

Battarea phalloides by H. J. Dittmer (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

I'm pleased to report that the Council of MSA has agreed to a second term on the Editorial Board for you.

Received: 19-12-'77 Returned: 20-12-'77.

Comments on "Battarea phalloides (Dicks.) Pers. ..." by H.J.Dittmer

The report is of interest, but a number of errors (mostly indicated in ms.)must be corrected before publication. The author has apparently not seen some of the literature cited, including Lloyd and Maublanc et Malençon. Lloyd's "Synopsis of the known phalloids" has no reference to Battarraea for the good reason that it is not a phalloid.

Although Lloyd and others have taken liberties with the spelling of the genus name, Persoon published it as Battarraea and this remains the correct designation.

A single, carefully cropped photograph (fig. 3 would do) should be sufficient to illustrate this note.

Since the fungus is infrequently reported, it would be appropriate to include spore measurements.

B. Lowy

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

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

22 August 1977

Dear Bernie,

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

Laeticorticium lombardiae (Aphyllophorales, Corticiaceae), a newly recognized segregate from the L. Roseum-complex by M. J. Larsen and R. L. Gilbertson

(Brief Article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?

5. Have the tables been prepared as clearly and concisely as possible?

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

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 4-IX-'77

Returned: 6-IX-'77

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

18 July 1977

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Dear Bernie,

I trust that you had a good trip and are ready for the rest of summer, IMC2, etc. As you are no doubt aware, your term of office on the Editorial Board of MYCOLOGIA expires in 1977. Would you consider a second term? I would be pleased to submit your name to the Council of MSA, and to be assured of your availability for another term.

At this time, as I prepare the annual report on MYCOLOGIA, I wish to thank you personally for your capable reviews and comments. They are much appreciated.

Sincerely yours,

Margaret

M. E. Barr Bigelow

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL
UNIVERSITY

25 VII 1977

Dr. Margaret Barr Bigelow
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

Dear Margaret,

Thank you for your kindness in inviting me to serve another term on the Editorial Board of MYCOLOGIA, a responsibility that I shall gladly accept.

Sincerely yours,

B. Lowy

Dr. B. Lowy Edit. Board
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

18 July 1977

Dear Bernie,

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

Variation, distribution, ethnomycological data and relationships of Psilocybe azectorum, a Mexican hallucinogenic mushroom by G. Guzmán

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received 26-VII-'77 returned 29-VII-'77

Comments on "Variation .....of Psilocybe aztecorum, ..."
by G. Guzman

p. 2, par. 2: "It is now known that P. aztecorum is a strongly hygrophanous fungus, with a brown-yellowish to brown-gray pileus that soon fades to milk white, with rhizoids at the base of the stipe, and a sublignicolous to lignicolous habitat. None of these features was reported by Heim or Singer and Smith."

The last statement is not entirely accurate. Singer & Smith (Mycologia 50: 281) refer (in part) to the pileus as "....slightly viscid, hygrophanous, milk white with yellowish disc...." Only the rhizoids and (sub)lignicolous characteristics are not mentioned. The author (p. 2 bottom and p. 3, par. 1) mentions additional color variations of the pileus, but then states (p. 6, line 3):"The variations in the color and form of the pileus and stipe .... seem to be of no taxonomic value...." If this is the case, one wonders why others should be taken to task for not stressing them.

p. 5, par. 3: The author, after indicating that spore size is among "the principal features added in the emendation of P. aztecorum," reiterates his statement on p. 2, broadening it to include spore size among the characters that "Heim (1957), Heim & Wasson (1958) and Singer & Smith (1958) did not consider...." The author's intention must be to stress the discrepancy in spore size reported by these authors, since he cites the measurements given by each of them and compares these with his own.*

p. 6, par. 1: "....P. bonetii Guzman (1968)...." This reference should be in the bibliography even though it is cited 3 lines below in the synonymy.

These points would seem to merit reconsideration by the author, since they might be misinterpreted by others.

---

* There is a good deal of overlapping in the data on spore size as given by all the authors cited.

Comments on "Variation .....of Psilocybe aztecorum, ..."
by G. Guzman

p. 2, par. 2: "It is now known that P. aztecorum is a strongly hygrophanous fungus, with a brown-yellowish to brown-gray pileus that soon fades to milk white, with rhizoids at the base of the stipe, and a sublignicolous to lignicolous habitat. None of these features was reported by Heim or Singer and Smith."

The last statement is not entirely accurate. Singer & Smith (Mycologia 50: 281) refer (in part) to the pileus as "....slightly viscid, hygrophanous, milk white with yellowish disc...." Only the rhizoids and (sub)lignicolous characteristics are not mentioned. The author (p. 2 bottom and p. 3, par. 1) mentions additional color variations of the pileus, but then states (p. 6, line 3):"The variations in the color and form of the pileus and stipe .... seem to be of no taxonomic value...." If this is the case, one wonders why others should be taken to task for not stressing them.

p. 5, par. 3: The author, after indicating that spore size is among "the principal features added in the emendation of P. aztecorum," reiterates his statement on p. 2, broadening it to include spore size among the characters that "Heim (1957), Heim & Wasson (1958) and Singer & Smith (1958) did not consider...." The author's intention must be to stress the discrepancy in spore size reported by these authors, since he cites the measurements given by each of them and compares these with his own.*

p. 6, par. 1: "....P. bonetii Guzman (1968)...." This reference should be in the bibliography even though it is cited 3 lines below in the synonymy.

These points would seem to merit reconsideration by the author, since they might be misinterpreted by others.

---

* There is a good deal of overlapping in the data on spore size as given by all the authors cited.

Singer, R. and A. H. Smith, 1958-A. New species of Psilocybe. Mycologia 50: 141-142

Singer, R. and A. H. Smith, 1958-B. Mycological investigations on Teonanácatl, the Mexican hallucinogenic mushroom, II. Mycologia 50: 262-303.

Wasson, R. G., 1957. En busca del hongo mágico. Revista Life. May. Mexico, D. F.

FIGURES

Figs. 1-6. Psilocybe aztecorum var. aztecorum. (1: Guzmán 16557; 2: Guzmán 16562; 3: Guzmán 16518; 4: Guzmán 16553; 5: Guzmán 16561; 6: Guzmán 16558).

Figs. 7-13. Psilocybe aztecorum var. aztecorum. 7: Spores (Type). 8: Basidia, one on the right monosporic (Type). 9: Pleurocystidia (Type). 10: Cheilocystidia (Type). 11: Cheilocystidia (Guzmán 7483). 12: Spores (Guzmán 16557). 13: Spores, on the left abnormal (Guzmán 1234-B). All x 2000, except that No. 11 is x 1500.

Figs. 14-16. Psilocybe aztecorum var. aztecorum. (14 y 16: Guzmán 16553; 15: Guzmán 16562).

Figs. 17-20. Psilocybe aztecorum var. aztecorum. 17-19: Habitat (Guzmán 16562). 20: Fruit bodies (Guzmán 16551).

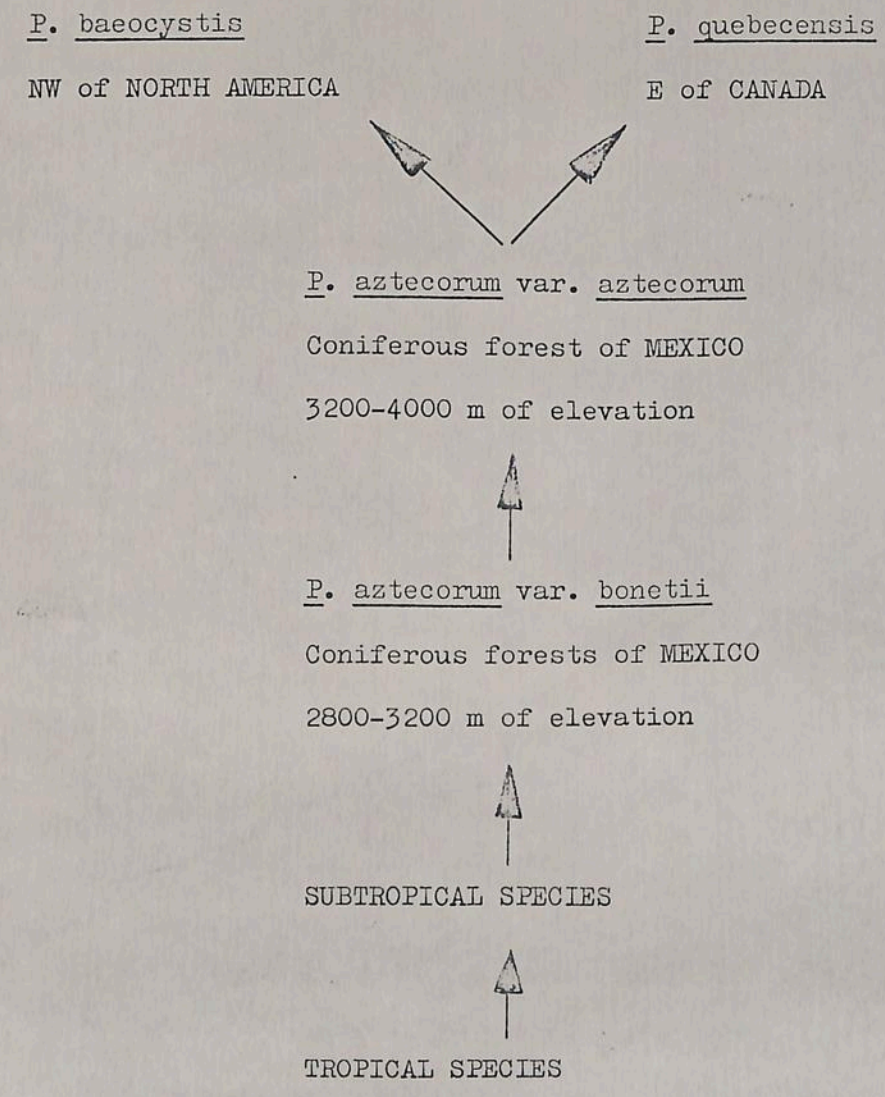
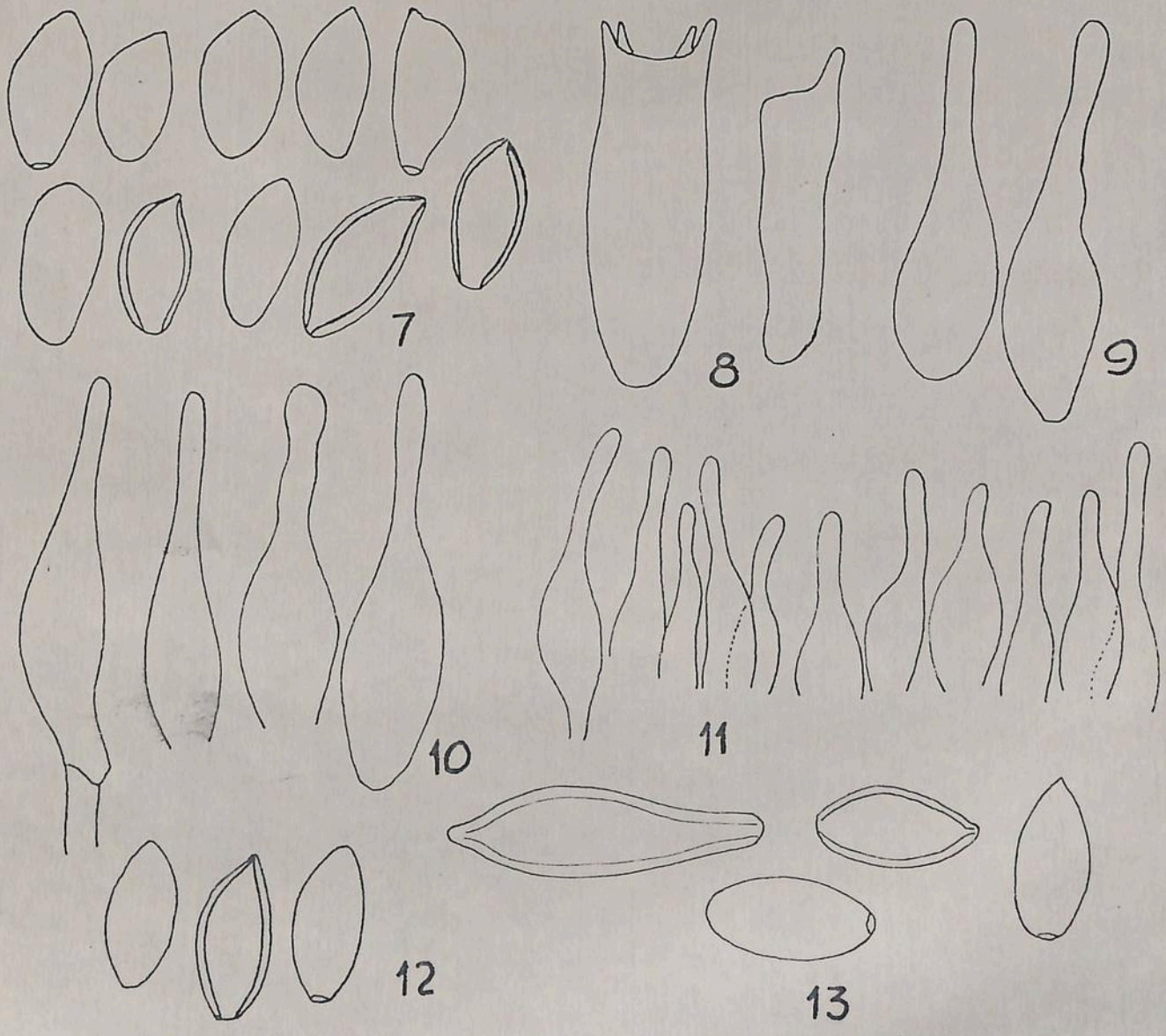


Fig. 21. Possible evolutionary series of Psilocybe aztecorum and its related species.

Guzmán, 1977



Department of Microbiology
Faculty of Science
Chulalongkorn University
Phya Thai Road
Bangkok 5, Thailand

June 28, 1977

Dr. Bernard Lowy
Louisiana State University
Baton Rouge, Louisiana, 70803

Dear Dr. Lowy

I would like to publish the paper on Effects of nutrition on amylase production in Monascus pupureus in Journal of Mycologia. I am enclosing the original manuscript along with this letter.

If you need any further information, please let me know. I am waiting for your consideration on the paper.

Sincerely yours,

Sumalee Pichyangkura

Sumalee Pichyangkura Ph.D.

Dr. SUMALEE PICHYANGKURA
DEPARTMENT OF MICROBIOLOGY
FACULTY OF SCIENCE
CHULALONGKORN UNIVERSITY
BANGKOK 5, THAILAND

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

8-VII-1977

Dr. Sumalee Pichyangkura
Department of Microbiology
Faculty of Science
Chulalongkorn University
Phya Thai Road
Bangkok 5, Thailand

Dear Dr. Pichyangkura:

I regret that your paper in its present form is not acceptable for Mycologia. I have made some corrections and suggestions for your consideration. It is my feeling in any case, that your paper would be more appropriate for a journal such as Mycopathologia et Mycologia Applicata. I hope that my preliminary editorial work may be helpful to you in revising the manuscript.

Sincerely yours,
B. Lowy
Bernard Lowy
Editorial Board, Mycologia

Dr. B. Lowy
Mycological Herbarium
Dept. of Botany
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

23 May 1977

Dear Bernie,

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

Amatoxin-containing mushrooms: Amanita ocreata and Amanita phalloides in California by J. F. Ammirati, H. D. Thiers, and P. A. Horgen (both "dangerous")

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received 27-V-1977.

Returned 30-V-77

p.7, lines 1-2: "...taxonomic relationship of A. ocreata to other Amanitas in this group can not be determined."

There may be a certain ambiguity here. A. ocreata and its taxonomic relationships to A. phalloides, A. bisporigera and A. verna are discussed and some comparisons are clearly pointed out (p. 6, for example). If A. bisporigera can be "easily distinguished" from A. ocreata (p. 6) and the latter from A. verna (p. 6), then matters are not entirely obscure on this level. Agreeing on a "concept of A. verna in North America or in Europe" in the absence of type material is somewhat more difficult. I believe the authors mean that the relationship can not be determined with certainty or with confidence. If this is their intent, adding the qualifying words (following "determined") would help.

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

30-V-1977

Dear Margaret,

It's my oversight in not letting you know that I had planned to leave Baton Rouge on 3-VI, but luckily the ms arrived on the 27th so I had a chance to read it.

I expect to be in Guatemala and Mexico until about 15-VII. There may still be a few details to take care of for the ethnomycology symposium to be given at the Congress in September. I was especially glad that Dr. Smith accepted my invitation to give a paper. Gastón Guzmán will also be on the program and I'll be seeing him in Mexico.

I hope to see you and Howard in Tampa.

With best regards,

Bernie

Dr. B. Lowy
Mycological Herbarium, Botany Dept.
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

9 April 1977

Dear Bernie,

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

The species of Exidiopsis (Tremellaceae) of the U.S.S.R.
by K. Wells and A. Raitviir

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Recvd. 14-IV-'77

SUMMARY

The species of Exidiopsis known to the authors from the U.S.S.R. are noted. Those species not previously studied in detail are described and illustrated. The genus Exidiopsis is emended to exclude the type, and closely related, species of Eichleriella. Exidiopsis candida, previously known only from tropical and subtropical regions, is shown to occur on Sakhalin Island. Exidiopsis effusa, on angiosperm wood, is reported from eastern Siberia and european U.S.S.R. Exidiopsis grisea, on gymnosperm wood, was identified from several sites in the Transcaucasia. A previously undescribed species, Exidiopsis gypsea, is shown to occur in Crimea Peninsula, Transcaucasia, and Estonian S.S.R. Exidiopsis calcea is reported from throughout the U.S.S.R. and apparently occurs on both angiosperm and gymnosperm wood. It is the most frequently collected and widely distributed species of Exidiopsis in the U.S.S.R. Additional collections of Exidiopsis griseobrunnea are noted from Siberia, and a new subspecies, E. griseobrunnea subsp. macrogyna, is described. Both subspecies of E. griseobrunnea are apparently only able to utilize species of Duschekia as a substrate. Additional collections of Exidiopsis laccata and Exidiopsis pallida, which have been previously reported from the U.S.S.R., were not noted.

This report represents a continuation of our joint studies of the resupinate species of the Tremellaceae of the U.S.S.R. Our objectives are to determine the distribution and to describe in detail the taxonomically important characters of the species of this region. Our previous reports described two new species of Exidiopsis (Raitviir and Wells, 1966) and characterized the species of Bourdotia and Basidiodendron (Wells and Raitviir, 1975) known from the U.S.S.R. We have restricted our studies to well preserved specimens that we have been able to examine microscopically.

Legends to Figures

Fig. 1. Exidiopsis candida. A-C. Apical segments of fertile hyphae. D. Basidiospores. All from TAA 61969.

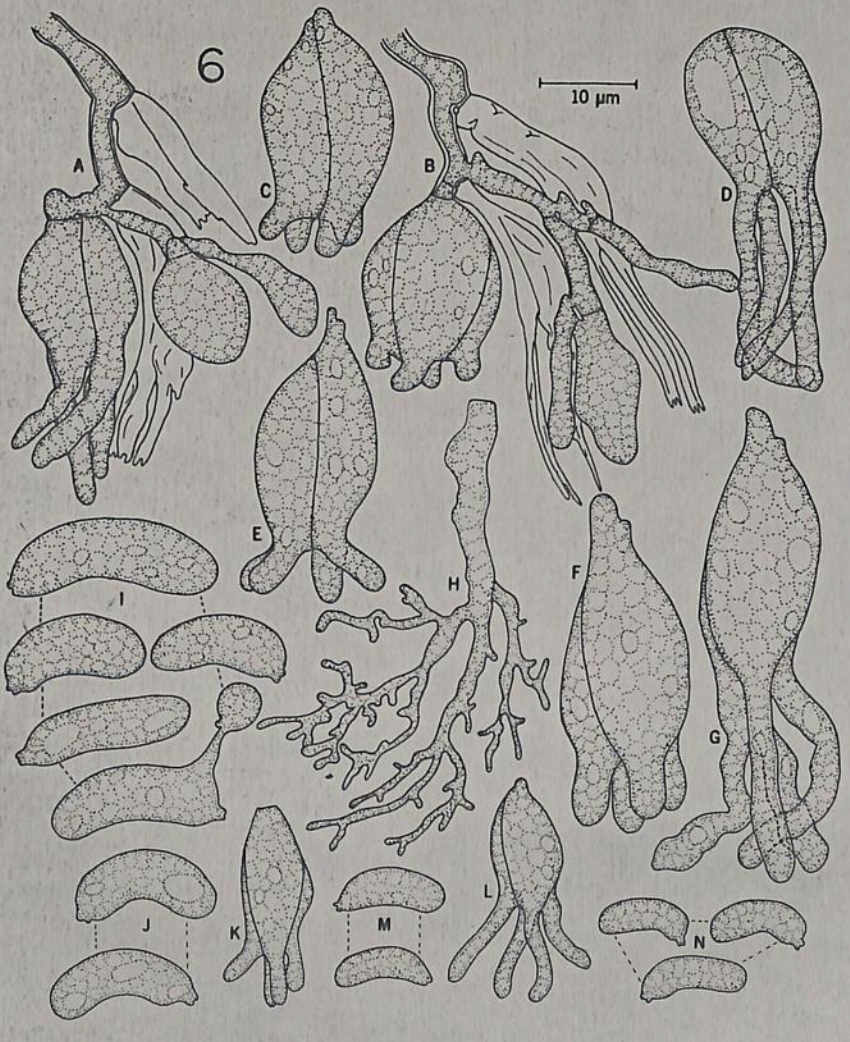
Fig. 2. Exidiopsis effusa. A,B. Apical segments of fertile hyphae (TAA 57). C-E. Basidia (TAA 57). F. Cystidiole (TAA 57). G-I. Dikaryophyses (G, H from TAA 57; I from TAA 15655). J. Basidiospores, one germinating by repetition (TAA 57).

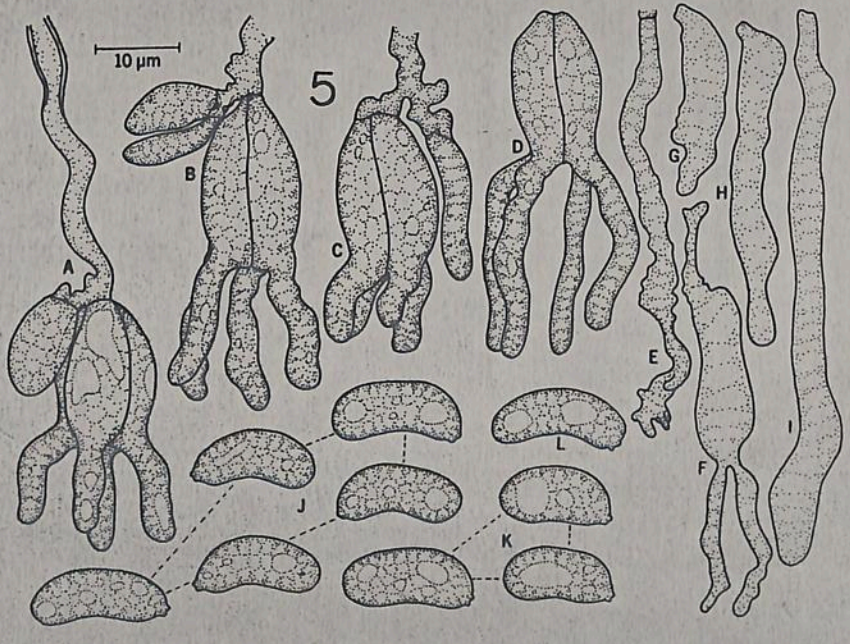
Fig. 3. Exidiopsis grisea. A-C. Dikaryophyses (A,B from TAA 16027; C from TAA 53187). D,E. Cystidioles (D from TAA 16027; E from TAA 53187). F-H. Basidia (TAA 16027). I. Portion of fertile hypha (TAA 16027). J,K. Basidiospores (J from TAA 16027; K from TAA 53187).

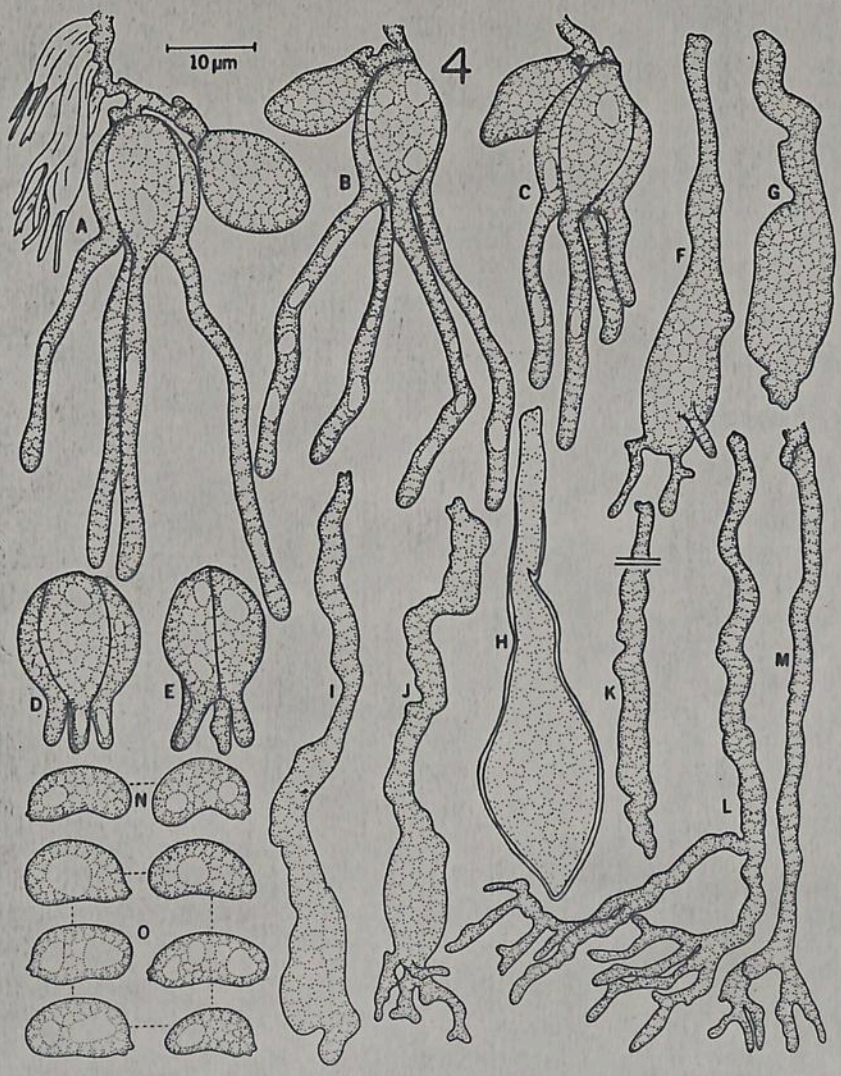
Fig. 4. Exidiopsis gypsea. A-C. Segments of fertile hyphae (TAA 16907). D,E. Basidia (TAA 16907). F-H. Cystidioles (F,G from TAA 16907; H from TAA 19870). I-M. Dikaryophyses (K,J,L from TAA 16907; K,M from 53237). The structures represented by Fig. 4, I and J might also be interpreted as cystidioles. N,O. Basidiospores (N from 53237; O from TAA 16907).

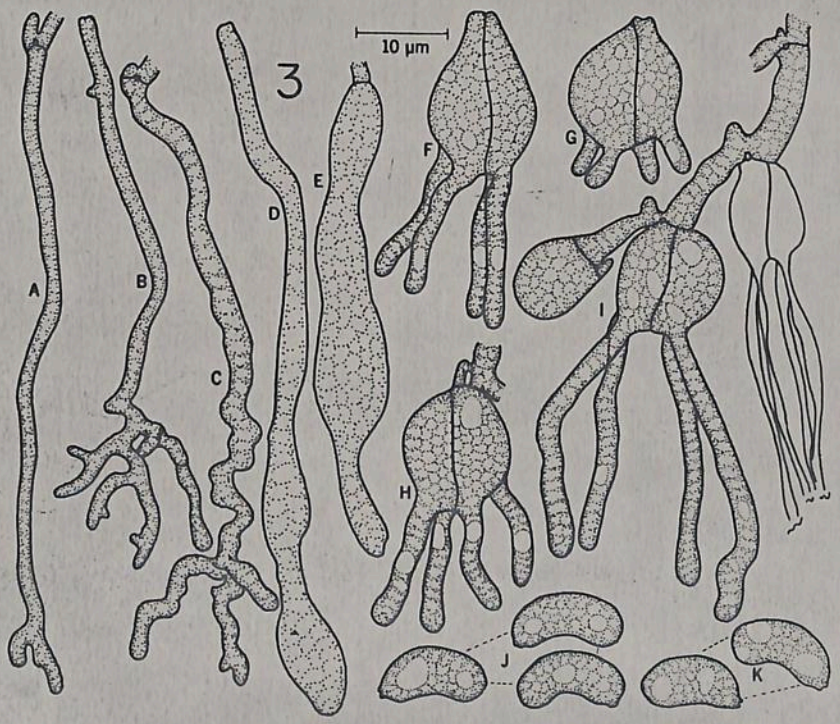
Fig. 5. Exidiopsis calcea. A-C. Segments of fertile hyphae (A from TAA 15314; B,C from TAA 6964). D. Basidium (TAA 69640). E,F. Dikaryophyses (TAA 16220). G-I. Cystidioles (G,H from TAA 16220; I from TAA 6964). J-L. Basidiospores. (J from TAA 12616; K from TAA 16220; L from TAA 6964).

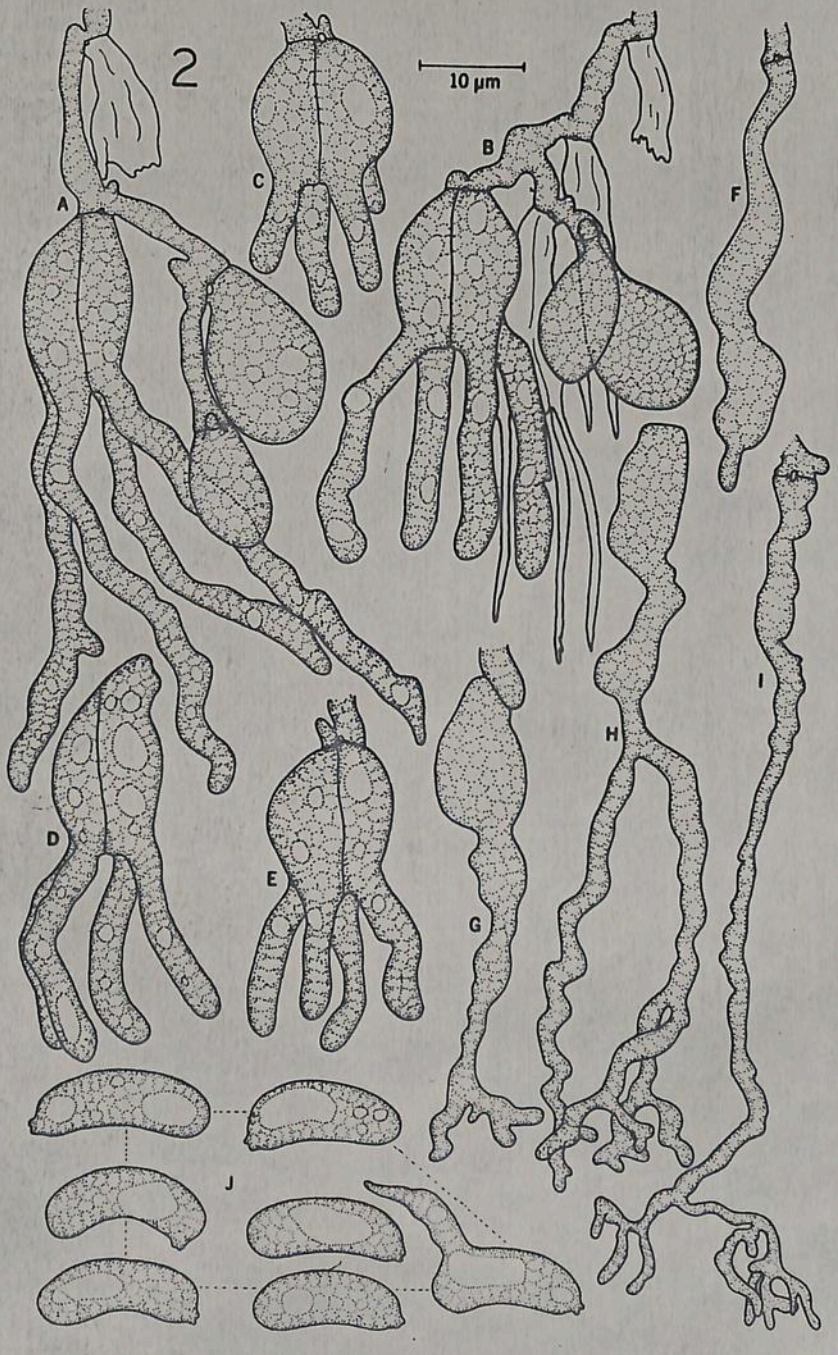
Fig. 6. A-J. Exidiopsis griseobrunnea var. macrogyna. A,B. Segments of fertile hyphae (TAA 56485). C-G. Basidia (TAA 56485). H. Dikaryophysis (TAA 56485). I,J. Basidiospores, one germinating by repetition (I from TAA 56485; J from TAA 56815). K-N. Exidiopsis griseobrunnea var. griseobrunnea. K,L. Basidia (TAA 56008). M,N. Basidiospores (TAA 56008).

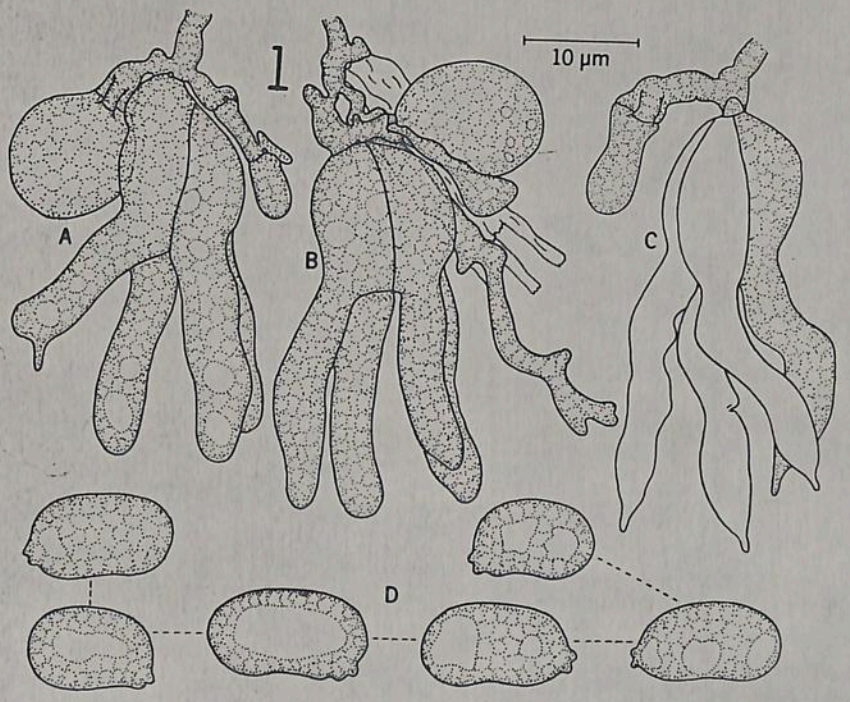












Dr. B. Lowy
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

Edit. Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

17 January 1977

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

Some aspects of bipolar heterothallism and other cultural characters of Poria carnegiea by J.P. Lindsey and R. L. Gilbertson

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Returned: 28-I-77

22-I-'77

28-I-1977

Comments on "Some aspects ....." by Lindsey and Gilbertson

p. 2, l.2: It would be preferable to cite the taxa given in parentheses as (Basidiomycotina, Aphyllophorales) or (Homobasidiomycetidae, Aphyllophorales) rather than "(Aphyllophorales, Basidiomycetae)." "Basidiomycetae," I think, is not a taxon with recognized status.

This exemplary paper should be published without delay.

RC

SOME ASPECTS OF BIPOLAR HETEROTHALLISM AND OTHER CULTURAL CHARACTERS OF PORIA CARNEGIEA[1]

J. Page Lindsey and R. L. Gilbertson

Department of Plant Pathology, University of Arizona

Tucson, Arizona 85721

Summary

Eighteen dikaryotic isolates of Poria carnegiea Baxter were used in a study of distribution and number of alleles for incompatibility in the population of the fungus. Thirty-six homokaryons representing both mating types from each dikaryon were obtained from single-oidium isolates and 29 alleles for incompatibility were found in the sample. The number of alleles in the population was estimated to be 90, giving an outbreeding capacity of 98.89 percent. Mosaic studies were carried out to determine the number of dikaryotic mycelia present in the decaying wood of a single fallen saguaro. Twelve homokaryons were retrieved from six dikaryons and ten alleles for incompatibility were found, indicating multiple infection points on this one plant. Nuclear staining showed cells of homokaryotic hyphae and oidia from both dikaryotic and homokaryotic isolates to be multinucleate. The optimum growth temperature for P. carnegiea on Nobles' agar medium was 32 C. The thermal death point after 24 hr constant exposure was 46-48 C.

---

[1] University of Arizona Agricultural Experiment Station Journal Article No. 2869 . This paper is based on a portion of a dissertation submitted by the senior author in partial fulfillment of the requirements for the Ph.D. degree at the University of Arizona.

MSA Career Brochure

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

29-II-1976

Dr. Fuller ——

This may not be just what you wanted but I have gone over the ms and made a few improvements (?) and suggestions.

An illustration for the brochure cover might follow the format of the June 1976 Newsletter, but show fruiting bodies of fungi instead of spores. Too bad there is no resident artist around such as they have at the Instituto de Botânica in São Paulo. But George Cimes might be willing to do it.

Good luck!

B. Lowy

no response from Dr. F.

The University of Georgia

BOTANY DEPARTMENT • Athens, Georgia 30602 • TEL. 404 542-3732

September 8, 1976

TO: Council Members of Mycological Society of America and Editorial Board for Mycologia. Other interested persons.

FROM: M. S. Fuller, Chairman. Committee on Careers Brochure

SUBJECT: Careers Brochure for Mycological Society of America

As most of you know we have been without a Careers in Mycology brochure for several years and Council has repeatedly urged that a new one be prepared. Ralph Emerson agreed to do this several years ago and I am enclosing the draft which he sent to K. A. Pirozynski and me. I am also taking the liberty of enclosing his covering letter which expounds the principles which guided him in his writing. My intent, after receiving your comments, is to get with Dr. Emerson on any changes and then go to a designer with the hopes of having a new brochure ready in January, 1977.

What do I want from you?

1. Your reaction and comments by no later than October 5.

2. Any pictures or illustrations which you believe would be appropriate to such a brochure. I like Emerson's suggestion that such illustrations be "artistic and spectacular" in illustrating the "sheer beauty of our subject!"

3. How many do you think we should print? Do you favor a well designed, but simple in layout, brochure?

4. Other comments.

Thank you for your time. I must have your answer by October 15.

Sincerely yours,

Mel Fuller

Melvin S. Fuller, Chairman
Committee on Careers Brochure

MSF/kb

Enclosure

UNIVERSITY OF CALIFORNIA, BERKELEY

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO

SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF BOTANY

BERKELEY, CALIFORNIA 94720

July 12, 1976

Dr. M. S. Fuller
Department of Botany
University of Georgia
Athens, GA 30601

Dr. K. A. Pirozynski
Division of Palaeontology
Museums of Canada
Ottawa, Ont., Canada

Dear Friends:

Well, I have a draft at last, and nearly a month ahead of the schedule I set up in June. Having thought about it a lot in these past 3 years (ouch!), the actual writing went more rapidly than I had expected. There are many things about it I would now like to discuss with you but since that is impossible, I thought I would outline for you some of the ideas and philosophy I had about it so that you may understand better why I presented it as I did.

1. Format. The more I have thought about it the more I have felt convinced that the current Bot. Soc. of America brochure is an ideal model for us to use. This is doubly appropriate in that a fellow mycologist, the late Robert M. Page was in charge of preparing it, the motif on the cover is nearly 50% mycological, and one of the six full-page pictures is a water mold! I like the color design for the cover, I approve the large, artistic illustrations with brief legends and acknowledgements at the back, and I find the text most attractively laid out. The direct comparison with our present brochure is very telling. Ours is almost as old-fashioned, unattractive, and stuffy as the botanical one was that Page replaced.

   By the way, you will note on the inside of the back cover-page of the new Bot. Soc. one an acknowledgement to Jim M'Guinness. He is still here in the Bay Area doing the same sort of work and says he would be glad to help again if we wish. His address is 1122 Golden Way, Los Altos, CA 94022.

2. In preparing the present draft I was guided by a number of general principles with which you may or may not agree.

   a) By and large a brochure such as this is going to be read by (i) young people of high school or early college age or (ii) lay adults who have little or no technical knowledge of the fungi or even of biology in general. It should, therefore, have a minimum of technical terminology and should read easily, directly and simply.

b) It is not the purpose of such a brochure to expound the science of mycology to the reader. Indeed Page's brochure has a minimum of botany as such, and I have aimed to include the bare minimum about fungi themselves and to avoid almost anything in the way of technical description. This is in direct contrast to the manuscript which Taber prepared and which is turgid with technicalities and reads more like his little booklet ("The Impact of Fungi on Man") than a career brochure.

c) Our brochure should not:
State the obvious and tell the reader what any good high school biology teacher will already know.

Present information or current science topics or research emphases that are today's special fads and will soon be dated.

Present information in the form of pedantic outlines or awkward small tables.

Lay special stress upon mushrooms and amateur mycologists.

Include lots of picky small drawings or photographs of "the scientist" in white lab coat.

d) Our brochure should aim to:
Be challenging and interesting from the start.

Be as honest as possible about the future and realities of mycology (You may well disapprove of my section on "wider employment opportunities").

Be as encouraging to women and minorities as to others (I could do with some help on wording here!).

Be as artistically and spectacularly illustrated as possible (It's not the information that's to be imparted but rather the impression and the wonder and the sheer beauty of our subject!).

e) I realize that book titles and subjects can become obsolete rather rapidly and that our brochure may be in print for a decade or even longer. Nevertheless, I believe that the titles of a few books of the sort I have listed might well be included.

Last but not least I shall understand fully if you throw out my endeavors as Taber's were. Perhaps we need something that is part way between? Certainly Page's is more gracious and subdued than mine. In that respect mine is likely to be less acceptable to the rank-and-file mycologist. If you wish me to redo or reread subsequent drafts, I await your request. On

the other hand, if you can take it from here, I shall be only too glad. I do hope the Society will be willing to put up the funds to do as attractive a job as the Bot. Soc. did. You will have to twist their corporate arm because you can't get it done on a shoe string.

By the way, if you use my introductory section on the "Did you know that...," all facts should be verified by you and there are undoubtedly other combinations of entries that would be equally or more appropriate and challenging.

I look forward to hearing from you in due course.

Very best to you both,

Ralph

Ralph Emerson

RE:lto
Enclosure

DRAFT

Are you thinking about being a MYCOLOGIST?

What is MYCOLOGY anyway?

Did you know that fungi provided clues to the discovery of the genetic code and can convert garbage to edible protein?

Did you know that fungi are used in making the lemon flavoring of soft drinks, the hormones of "the pill", and the alcohol in wine and beer?

Did you know that fungi destroy some 25 billion dollars worth of human food and animal feed each year, enough to feed all of India?

Did you know that fungi once provided the sacrament for ancient religions and now provide us with the makings of LSD?

Did you know that certain fungi cause athlete's foot and others cause fatal diseases for which no cure has yet been discovered?

Did you know that fungi and their products are being studied today by engineers, chemists, molecular biologists, ecologists, physicians, industrialists, investors, builders, physicists, pathologists and a host of other specialists of all kinds?

Did you know that fungi are the source of antibiotics like penicillin that have revolutionized the practice of medicine throughout the world?

Did you know that fungi number over a hundred thousand different kinds and new ones are being discovered by the hundreds every year?

Did you know that fungi are an essential part of the living world and fascinating to study for their own sake?

Did you know that fungi .....

So there you have it: very simply put, mycology is the study of fungi from any and every point of view. It started out long ago by people observing and naming mushrooms; today mycologists use electron microscopes and spectrophotometers; tomorrow, it's up to you! But wherever it leads mycology will be the scientific exploration of fungi: their physiology, genetics, development and reproduction, ecology, pharmacology, classification, biochemistry, pathology, economics and ~~every~~ many other ~~thing about them you can imagine.~~ related studies

Many people don't know what a fungus is. Relatively few fungi are large enough, like toadstools and puff balls, to be easily seen. Most of them, like other microbes, can only be studied with a microscope. They include molds, mildews and yeasts by the thousands. What is it that makes them all fungi? Most important, they have no chlorophyll so they can't make their own food ~~with sunlight the way~~ by photosynthesis all green plants do. ~~This means they must grow on something else.~~ Hence they are parasites, when they attack other living organisms, or saprophytes, when they cause decay of dead organisms or their products. It is this requirement for a source of food that accounts for their immense significance in the biosphere in general and their great practical importance to mankind in particular. As decomposers they are a vital link in the great cycles of matter: without them the trees of the forest would not rot when they fall. On the other hand, it is their need for food that brings the fungi into direct conflict with man. Since the dawn of civilization they have threatened his crops, destroyed his wooden ships and homes, and attacked his ~~skin and hair.~~ body Today they threaten cancer patients, grow in telegraph poles and the fuel tanks of jet liners, and are as much of a challenge to agriculture as ever. Meanwhile, however, with the help of mycologists, other scientists, and

engineers, man has turned the fungi to good account. Today they are employed in hundreds of essential industries and produce a bounty of vital materials.

So, this much is clear to you: mycology is the study of fungi in all their aspects and fungi have an important bearing on almost every major aspect of man's activities on this planet.

Who, then, employs MYCOLOGISTS?

From what has just been said you might suppose that mycologists are in great demand and practical jobs are open to them in many fields. This is not really true and we must consider carefully why. A current analysis of the ~~one thousand~~ thirteen hundred or so members of the Mycological Society of America reveals 70% in Teaching and Research positions in Educational Institutions (colleges and universities), 17% in Federal or State Government Laboratories (Agriculture, Health, etc.) and only 13%, ~~a little over one in ten~~, in all other positions representing pharmacy, brewing, hospitals, industry, and ~~so forth.~~ many more Why are the professional ~~experts on fungi~~ mycologists overwhelmingly employed only in education and government today? The answer appears to be because there is so much to know about fungi that most ~~professional~~ mycologists have chosen to devote their full time to basic studies, usually teaching and research, ~~on the fungi.~~ Very few of them have taken the time to prepare themselves, in addition, for high level jobs in such related practical work as administration, engineering, medicine, pharmacy, applied chemistry, agriculture or dozens of other areas of application and industrial development where job opportunities are so numerous. Putting it in another way, few specialists in medicine, environmental policy, engineering, chemistry, business management, etc. have sufficient knowledge or special interest in the fungi to be counted as professional mycologists. They may have learned

a bit about some narrow aspect of mycology in connection with their own chosen profession. Thus brewmasters and vintners know about yeasts and fermentations, dermatologists know about skin fungi, pharmaceutical chemists know about fungal antibiotics and toxins, plant pathologists may have extensive knowledge of the fungi that cause crop disease, and food technologists know about vegetable rots. None of them, however, is a bona fide mycologist, a specialist on fungi. ~~In summary~~ So, it is university and government departments that employ the great majority of professional mycologists today.

Caps — As a MYCOLOGIST how could I widen my employment opportunities?

~~That is a good question and~~ The solution, ~~as is so often the case,~~ appears to lie in the direction of more study to achieve a broader preparation. Most of the mycologists who have entered non-educational or non-governmental positions have had the initiative to become qualified to some extent in another profession. For example, they have studied enough industrial chemistry to qualify for a job in a drug house or fermentation industry where chemically trained mycologists are ~~welcome~~ needed. Or they have learned enough medicine or some aspect of medical technology so they could hold a post in a hospital or allergy clinic as a diagnostician. If you want to serve in the great wave of environmental ecology as a mycologist—and surely they are needed—become highly proficient in soil and water microbiology, or the statistics and computer analysis of ecological surveys, or the mycorrhizal aspects of forestry and crop production, or even biological applications of the law. Learn professional plant breeding and modern genetics and work in a plant disease control laboratory or chemical industry developing new and safer fungicides or other control methods against crop diseases.

There are ~~as~~ many other possible combinations ~~as you can think up.~~ of disciplines. The key to ~~this~~ sort of success is going to be to combine your interest and ability in mycology with special qualifications in an area of applied science. Increasingly the successful professional ~~is going~~ will to be the one who has expert knowledge in several related subjects. ~~And, finally, you will recognize that even~~ In ~~the~~ university teaching and research ~~that is done in a university,~~ it is the people who can bridge two or more basic areas of knowledge who will open up new vistas for themselves and future generations. ~~You~~ too can certainly widen your employment opportunities as a mycologist, as ~~well as your creativeness as a scientist, but it will involve wider studies and hard work.~~

Caps ← So, what education and training are required for MYCOLOGY?

What degrees do I need?

The first important point ~~to recognize~~ is that the mycologist must be a good biologist. Fungi, like animals and green plants, are living things, and anyone who is going to devote to them a lifetime of study must be as well educated and trained as he possibly can be in the natural science of biology. ~~Your high school biology teacher is well aware of this fact and will have told you about his own training and the background of a biologist.~~ ~~First,~~ This requires a knowledge of ~~it involves~~ the basic sciences—mathematics, physics, chemistry—just as much as you can understand and as far as you can go with them, especially organic chemistry and biochemistry. This work ~~will~~ should be accompanied or followed by intensive work on the fundamentals of life sciences, ~~biology itself.~~ ~~All along~~ Also the way, from your earliest years in school, you will ~~also~~ have paid serious attention to thinking and communicating clearly. Planning logically, writing and speaking so others can understand you are absolutely essential.

Perhaps by the time you are half way through college you will be ready for ~~to get into~~ a serious introduction to the fungi. If you are in a small, four-year college, you may ~~will~~ be fortunate to have a teacher, probably a botanist, qualified to give you a thorough beginning course in ~~the science~~ of mycology. For anything beyond a start in this specialty, just like others, you must turn to a major university. In fact, it is likely that much of your intensive training in the biology of fungi will have to occur in graduate school. Even here, after possible courses in medical mycology, plant pathology, and perhaps even applied mycology, ~~your~~ the major emphasis on fungi will be through individual guided reading, field work, laboratory study, research, and imaginative exploration and thinking. Here you will discover the excitement of seeking for new knowledge about fungi and here too you will become a practicing mycologist.

Because mycology is ~~barely~~ only superficially introduced to undergraduates, you will recognize that a 4-year college degree—A.B. or B.S.—will qualify you for few if any jobs really involving detailed knowledge of fungi. With a year or two of graduate work and an M.A. or M.S. degree, perhaps with a master's thesis ~~in~~ on one aspect of the fungi, a few non-academic and academic openings may be available to you, but they are likely to involve a good deal of routine work and relatively little ~~active~~ mycology. ~~In other words,~~ whether his interests are going to be basic or applied, the practicing mycologist will need an advanced degree and almost always this will be the Ph.D., usually in Biology ~~or~~ Botany or ~~sometimes in~~ Microbiology.

~~Do not forget what~~ As we emphasized in the foregoing section on employment opportunities. You will greatly increase the number and scope of ~~these~~ your opportunities, both within and outside academia, if you have prepared

yourself ~~as a professional or sub-professional~~ adequately in a second, related aspect of your career. As a Ph.D. with a doctoral thesis in fungal biology, there will be teaching jobs for you in various ~~collegiate and~~ university institutions but little to be found elsewhere. If you are also skilled in chemistry, environmental analyses, health sciences, or other related ~~aspect,~~ studies will you ~~should~~ have a wide selection of possibilities outside of ~~the~~ the field education ~~field as well~~. Advanced qualifications in chemistry or microbiology should prove particularly useful.

Who becomes a MYCOLOGIST? — See reverse

A hundred years ago Beatrix Potter (of Peter Rabbit fame!) wished to study mycology in Britain, but she was told there was no place for women in that profession. Instead she turned her attentions to writing and illustrating children's books that have become world famous. Her paintings of mushrooms are among the most sensitive and perceptively beautiful that have ever been done. Happily today we do not discriminate against women, minorities, or any other group. Mycology is open to everyone with the native ability and determination to pursue a career in science. Perhaps it is well to point out that a career as specialized and unusual as mycology ought only to be entered upon by those who enjoy the subject so deeply that they can look forward with genuine enthusiasm to a lifetime of association with mushrooms, molds, and mycologists! It can also be said that few in mycology have made great personal financial gain therefrom. One should not ordinarily count upon more than a good and reasonable living such as society accords to teachers, research biologists, and other similar professional groups. Mycology is open to all ~~and~~ but its rewards are more intellectual than financial.

How can I find out more about MYCOLOGY and MYCOLOGISTS?

Locate the mycologist nearest to your community. If your biology teacher doesn't know one, ask him to contact the local natural history museum or college. Or perhaps there is a local mushroom collecting club or mycological society that can get you started. If none of these approaches is successful, write to: Secretary, Mycological Society of America, c/o New York Botanical Garden, Bronx, NY 10458 and ask for information about society members in your area.

When you have found a mycologist, have him come out and talk to your class, or make an appointment to visit him. He will be glad to talk to you about "Men and Molds" or tell you about the work he is doing. Maybe he will be able to introduce you to the fascination of collecting fungi in the field. Or he may tell you about isolating microscopic fungi and growing them in the laboratory so you can do experiments with them. In your high school you will have facilities for making media and using a microscope, and there is no better way to find out more about mycology than to study fungi yourself.

Also, don't forget about the local library. At the very least there is always the encyclopedia. See what it has to say about fungi. Then go to the subject catalog or ask the librarian to direct you to books they may have on mushrooms or other fungi. Perhaps the librarian can arrange to purchase some of the books on the list we have provided just below. Or, your local book dealer may tell you where you can buy them yourself. Audubon book stores, natural history museums, or societies such as the Sierra Club often have recent books about mushrooms. When you start collecting your own library and your own specimens and cultures of fungi, you

will be well on the way to becoming a mycologist. We welcome you to a wonderful study with a long past and a bright future.

Here are a few titles of books on MYCOLOGY as a start.

Boedijn, K. B. 1969. Plants of the world, the lower plants, 312 p. E. P. Dutton, New York.

Carefoot, G. L., and E. R. Sprott. 1967. Famine on the wind, 231 p. Rand McNally, New York.

Christensen, C. M. 1965. The molds and man, 3rd edn., 292 p. University of Minnesota Press, Minneapolis.

Duddington, C. L. 1972. Beginner's guide to the fungi, 176 p. Drake Publishers, Inc., New York.

Ingold, C. T. 1973. The biology of fungi, 2nd edn., 176 p. Hutchinson Educational Ltd., London.

Kavaler, Lucy. 1965. Mushrooms, molds, and miracles, 318 p. John Day, New York.

Kleijn, H. 1965. Mushrooms and other fungi, their form and colour, 144 p. Doubleday & Co., Garden City.

Ramsbottom, John. 1953. Mushrooms and toadstools, 320 p. Collins, London.

Smith, George. 1969. An introduction to industrial mycology, 6th edn., 399 p. St. Martin's Press, New York.

Rolfe, R.T. & F.W. Rolfe. 1925. The romance of the fungus world. 308 p. Dover Publications, Inc. New York. (Reprint ed. 1974.)

Large, E.C. 1940. The advance of the fungi. 488 p. Dover Publications, Inc. New York. (Reprint ed. 1962.)

Gray, W.D. 1959. The relation of fungi to human affairs. 373 p. Henry Holt & Co. Inc. New York.

This section should be rewritten. The nice little story about Bea Potter is O.K. but presumably if she had not been discriminated against, she might never have found the time to paint her whimsical impressions! As for expecting a tyro to generate enough enthusiasm to desire spending a lifetime with club members and their interests — this seems a bit unrealistic — if not downright destructive to an effective observer.

Possibly a new section on A FEW STATISTICS would be in order. Perhaps some one-liners on such matters as: the number of working mycologists in U.S. — how many universities have a mycologist — summer courses in mycology at biological stations, universities — mycological journals — number of articles published last year — mycological herbaria, congresses, expeditions — current mycology, textbooks, monographs — mycology clubs — USA

Dr. B. Lowy Edit. Board
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002 27 September 1976

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

A note on Gomphus S.F. Gray by E.J.H. Corner (Brief Article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 2-X-'76 Returned: 2-X-'76

A note on Gomphus S.F.Gray

E.J.H.Corner

91 Hinton Way, Great Shelford, Cambridge CB2 5AH, England

In presenting the new subgenus Phaeoclavulinoides, Petersen (1976) observed that spore-ornamentation had been overlooked as a basis for infrageneric separation. This feature was, however, the first which I used in distinguishing the three subgenera of Gomphus (Corner, 1966). Exception was taken to this by Petersen (1971) who proposed another system which is also unsatisfactory (Corner, 1976). The new subgenus Phaeoclavulinoides is based, unfortunately, on the little known species G. guadelupensis (Pat.) Reid, but in Table 1 of his article Petersen has included G. retisporus and G. viridis both of which I had referred in 1966 to subgen. Chloroneuron (Murr.) Corner with G. viridis as the type. If, though I gravely doubt, G. guadelupensis and G. viridis are to be included in one subgenus, then it must be Chloroneuron with Phaeoclavulinoides as a synonym. This disposal overlooks or oversimplifies the great differences in fruit-body and habit between these two species. I would limit subgen. Phaeoclavulinoides, though the name is inept, to G. guadelupensis and G. grandis and treat it as a section of subgen. Gomphorellus Corner (1966).

Dr. B. Lowy Edit. Board
Mycologiaal Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002 11 September 1976

Dear Bernie,

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

Nova Scotian Inocybes IV by D.W. Grund and D.E. Stuntz

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

Received: 14-IX-'76. Mailed: 22-IX-'76

NOVA SCOTIAN INOCYBES IV

D. W. Grund

Department of Biology, Acadia University, Wolfville, N. S.

and

D. E. Stuntz

Department of Botany, University of Washington,
Seattle, Washington.

SUMMARY

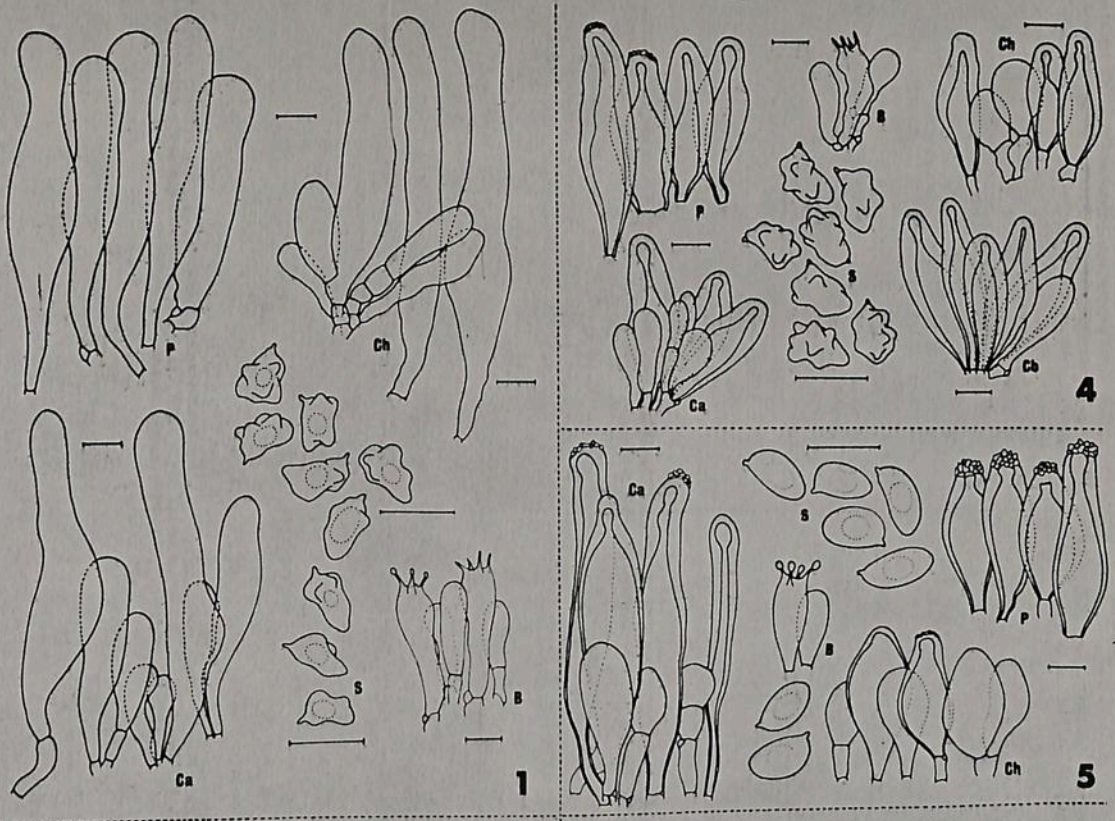
This fourth paper in the series considers nine taxa of *Inocybe* occurring in Western Nova Scotia. Two species and two varieties are described as new, and eight taxa are new records for the Maritime Provinces of Canada.

In our fourth paper of this series we add nine taxa to those previously reported (2, 3, 4). Two species and two varieties are described as new, and *I. obscurobadia* (Favre) Grund and Stuntz is raised from varietal to specific status. Only *I. nodulosa* Kauffm. was previously reported from the Maritime Provinces of Canada, and *I. acuta* Boud. and *I. obscurobadia* are reported for the first time from North America.

Color terms used in the descriptions are cited in the same manner as the first paper in the series (2). Herbaria in which collections are deposited are designated by the standard abbreviations published in the Index Herbariorum (10). An asterisk following a collection number indicates that camera lucida drawings were taken from that collection.

I. porcecoacta

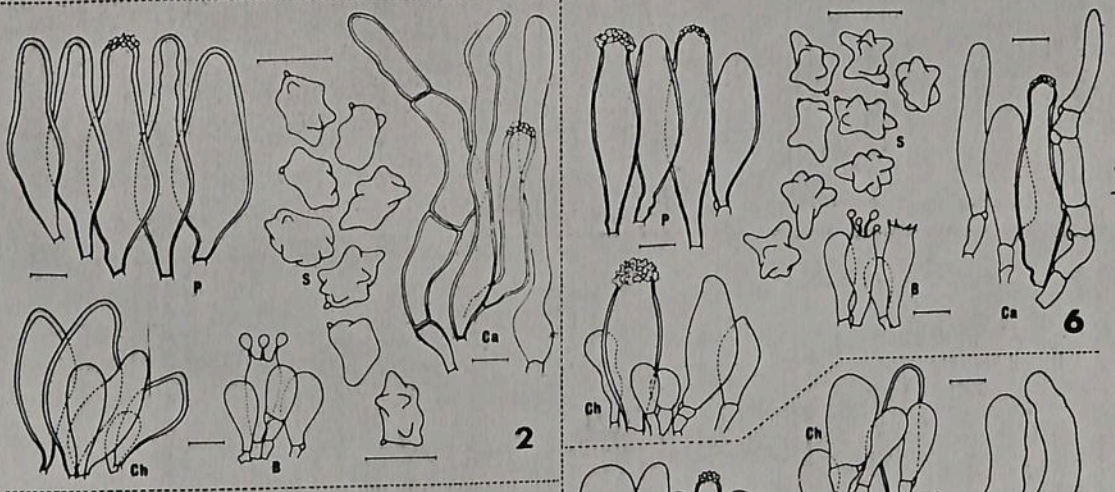
I. umbratica



I. Pallidocvema sp. nov.

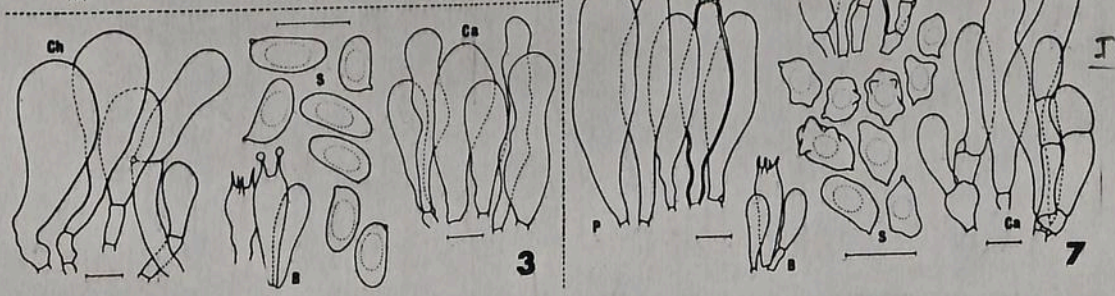
I. acuta

I. nodulosa



I. neobrunnescens var. leucothelota

I. mavitimoides



Var. nov.

Nova Scotian Inocybes IV by D.W.Grund and D.E.Stuntz

There is a small discrepancy, probably a typographical error, in the dimensions given for pleurocystidia of I. pallidocremea. On p. 10, l.4 fr. bot.: 33-65 X 12-21 um; p. 11, l.9 fr. bot.: 36-55 X 12-21 .... The largest of these cystidia in fig. 5 measures ± 50 um, so I have changed the numerals on p. 10 to make them conform with those on p. 11 which approach more closely the actual measurements shown in the figure.

5 October 1976

Dear Bernie,

This acknowledges receipt of your manuscript submitted for publication in MYCOLOGIA. Sorry - the Nov.-Dec. issue is now in galley proof; I'll keep you informed of the progress of your ms.

Margaret

M.E. Barr Bigelow
Editor, MYCOLOGIA

Dr. B. Lowy
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

2 August 1976

Dear Bernie,

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

A new Steccherinum (Aphyllophorales, Steccherinaceae) on quaking aspen
by J.P. Lindsey and R.L. Gilbertson (Brief article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?
5. Have the tables been prepared as clearly and concisely as possible?
6. Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M. E. Barr Bigelow
Editor

returned 13—VIII—'76

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

DEPARTMENT OF BOTANY

13-VIII-1976

Dear Margaret,

I am sorry about the delay in sending this to you. Although I returned from Guatemala over 10 days ago, I have been slow in getting back into my usual routine. Life in the tropics is too good to have to relegate it to only a short part of the year and some day I hope to be able to do something about it.

As well as I can tell, the only correction needed in this paper, apart from your final red pencil notations, is the deletion of the "Summary" if this is to be published as a brief article.

Just before leaving for Guatemala, I made an unscheduled brief appearance at the New Orleans meetings and regret that I did not see either you or Howard. I only talked briefly with a few of the people who are in charge of the II I.M.C.

Yours

Bernie

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

20 July 1976

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

Dear Bernie,

At the recent MSA meetings I discovered what I should have found out previously, namely, that Tom Sproston was completing his second term on the Editorial Board of Mycologia. Because two terms are all that one should ask for from any member, I did not place his name before the Council for another term. The name of Henry Aldrich was submitted and approved by the Council for a second term on the Board.

After some thought and with the advice of others, I have corresponded with Darrell Weber, who is agreeable to serve. I have forwarded his name to Jim Kimbrough to submit to the Council, and am informing you at this time.

Sincerely,

Margaret

M.E. Barr Bigelow

Professor Margaret E. Barr Bigelow
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

Dear Margaret:

I have completed my review of the article by B. Lowy on three new species of the Tremellales. As a supplement to the authors treatment of neotropical Tremellales (Lowy, B. 1971. Flora neotropica. Monograph No. 6. Tremellales. Hafner Publishing Co., Inc., N.Y. 153 p.), I would recommend publication of this article in MYCOLOGIA; however, I would also recommend that the author make some changes.

First, the article needs some sort of an introduction. I would suggest a couple statements. One to the effect that the new species were found among the collections made by K. P. Dumont in Panama. The other should refer to the author's monograph of neotropical Tremellales.

I believe the author should also describe how these new species differ from those described in his monograph. He essentially does this with Ductifera aurea and Tremella subrubiginosa; however, if he were to refer directly to the monograph as well as to the publications in which the closely related species being referred to were described, it would clarify the comments on relationships for the reader.

The following are some questions that might arise and should be answered in the manuscript:

would be so indicated if present.

a) In Exidia panamensis and Tremella subrubiginosa are their sterile structures in the hymenium, other than the chromatophyphae in E. panamensis?

were found other than indicated in fig. 2c. Generally understood that this is extremely variable

b) In E. panamensis, Ductifera aurea, and T. subrubiginosa are their clamp connections beneath the basidia?

c) Are the sterigmata of E. panamensis and D. aurea exceptionally long, short, etc? An introduction of the maximum length is sometimes helpful.

I have made a few, minor comments directly on the manuscript. My knowledge of Latin is such that I am unable to comment on the accuracy of the Latin descriptions.

Sincerely,

Kenneth Wells
Professor of Botany

KW/klk

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

11 May 1976

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

Dear Bernie,

This acknowledges receipt of your revised manuscript. It is scheduled for publication in the September-October 1976 issue of MYCOLOGIA. I assume that proof will not be ready till after you have returned in August.

I've further noted your travel schedule dates, so that I won't be sending mss. for review during that time. Hope that you have a pleasant and profitable summer, though I'm sorry that we won't see you at the meetings.

Thank you for your useful letter and for the names of likely members of the Editorial Board. As it stands, both Tom Sproston and Henry Aldrich were agreeable to a second term, so I am putting their names before the Council. I will keep your suggestions on file for future reference.

Thank you too for so obligingly and promptly reviewing a number of manuscripts during my first months in office. I do appreciate the necessity of having good reliable reviewers.

Sincerely,

Margaret

M.E. BarrBigelow

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

24 April 1976

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

Dear Bernie,

Enclosed is one copy of your manuscript "New Tremellales from Panama" and with it the reviewer's comments. Would you please take these suggestions into consideration in making any revisions? Please return the marked copy to me, and if you retype any pages, the original and one copy of these for my use in marking for the printer.

Is your trip to Guatemala still on for the summer? I'll note that so that manuscripts for review don't get sent to you. How long will you be gone?

Sincerely,

Margaret

M.E. Barr Bigelow

P.S. It did not seem necessary to return the copies of illustrations.

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

DEPARTMENT OF BOTANY

AMERICAN REVOLUTION BICENTENNIAL 1776-1976
LSU
A BICENTENNIAL UNIVERSITY

30-IV-1976

Dear Margaret,

I have taken Ken Wells's suggestion and added two brief sentences to the paper by way of introduction

Since I committed the embarrassing error (41 times) of writing mμ instead of μm and since you already had to go to the trouble of making the corrections, rather than retype the pages and undoing your work, I have tacked on the new paragraph at the beginning of the corrected ms. Likewise, the paragraph at the bottom of p. 4 is rewritten to eliminate the possible ambiguity and I have further identified Dumont with the NYBG.

The latin is OK, having been checked on that particular detail with a local expert. I am also returning Ken's comments, indicating briefly in the margin why I do not think it necessary to have mentioned these points.

Gilbertson & Martin's ms is also enclosed, together with my brief comments.

Regarding my travel schedule, I plan to leave Baton Rouge on or about May 30 and to return about August 5.

Best regards,

Bernie

~~B. Lowy~~

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

DEPARTMENT OF BOTANY

5-III-1976

Dear Margaret,

Enclosed is a ms for your consideration as a "Brief Article" in Mycologia.

I am invited to give a short course in mycology/ethnomycology at the University of San Carlos in Guatemala and it is possible that I shall have to leave Baton Rouge by the end of May, thereby regretfully missing the MSA meetings in New Orleans. The exact date for the beginning of the course has not been set and there seems to have been some interruption in communications since the earthquake. In any event, I'll be glad to receive any mss for review that you may care to send, but will have to ask that they reach me before May 15.

Yours,
Bernie

B. Lowy

Dr. B. Lowy
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

Editorial Board

MYCOLOGIA

Office of the Editor
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

19 April 1976

Dear Bernie,

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

Polyporus coronadensis, a new species from Arizona
by R.L. Gilbertson and K.J. Martin (Brief Article)

If so, please prepare two copies of your criticisms, one an unsigned carbon 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. Kindly return the paper with your comments promptly.

The following points should be considered:

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 value?**
5. **Have the tables been prepared as clearly and concisely as possible?**
6. **Are the illustrations adequate? Excessive? Properly grouped? Properly explained? Worthy of publication?**

Any other comments or criticisms will be appreciated.

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

Thank you for your assistance.

Sincerely,

Margaret

M.E. Barr Bigelow
Editor

I'll try to make this the last one, since you will be away -- what is planned date of return?

Received: 26-IV-'76 Returned: 30-IV-'76

POLYPORUS CORONADENSIS, A NEW SPECIES FROM ARIZONA[1]

R. L. Gilbertson and K. J. Martin[2]

Department of Plant Pathology, University of Arizona, Tucson 85721

Summary

Polyporus coronadensis is described as a new species from southern Arizona. It causes a uniform white rot in living and dead silverleaf oak. It differs from other species in the genus *Polyporus* Mich. ex Fr. em. Donk in the unusually large basidiocarps weighing up to 8.4 kg (18.5 lbs) and its apparent restriction to oak.

The genus *Polyporus* Mich. ex Fr. em. Donk has been studied by a number of workers (Bondarzew, 1953; Domański et al., 1967; Donk, 1969; Jahn, 1969; Pouzar, 1972; Ryvarden, 1969). It is a small genus in temperate regions with 12 species recognized in the USSR by Bondarzew (1953) and 12 species reported from Poland by Domański et al. (1967). North American species have been described by Lowe (1942) and Overholts (1953) in their studies of the genus *Polyporus* Mich. ex Fr. *sensu lato*. These are mostly wood-rotting fungi and the majority occur on dead wood, with *Polyporus squamosus* Huds. ex Fr. the only species commonly found causing a heartrot in living trees. The few species that have been tested give a positive reaction for polyphenol oxidases and are heterothallic and tetrapolar (Nobles, 1965).

---

[1]University of Arizona Agricultural Experiment Station Journal Article No. 2604.

[2]Present address: Arizona Commission of Agriculture and Horticulture, Phoenix, Arizona 85301.

Comments on Gilbertson & Martin's "Polyporus coronadensis..."

If this paper is to be published as a Brief Article, the summary should obviously be deleted.

There is a discrepancy in the basidiospore length as shown in fig. 2d. The description gives spore dimensions as 7-7.5 x 2.5-3 um. At least three spores in the figure are 10 um or longer as measured against the scale (9 mm = 10 um).

30-IV-'76

Comments on Gilbertson & Martin's "Polyporus coronadensis..."

If this paper is to be published as a Brief Article, the summary should obviously be deleted.

There is a discrepancy in the basidiospore length as shown in fig. 2d. The description gives spore dimensions as 7-7.5 x 2.5-3 µm. At least three spores in the figure are 10 µm or longer as measured against the scale (9 mm = 10 µm).

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

M. E. BARR BIGELOW
Editor-in-Chief
Department of Botany
University of Massachusetts
Amherst, Massachusetts 01002

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

31 March 1976

Dr. B. Lowy
Mycological Herbarium
Louisiana State Univ.
Baton Rouge, LA 70803

Dear Bernie,

As you are probably aware, their term of office expires in 1976 for two members on the Editorial Board of MYCOLOGIA, Dr. H.C. Aldrich and Dr. T. Sproston. I would appreciate any suggestions for reappointment or replacement of these members. The areas of specialization which these members now span should, I think, be taken into consideration in making any other recommendations. If you have suggestions, please forward them to me for consideration as soon as possible. I must make recommendations for nominations before 15 May, so that they may be included on the meeting agenda of the Council of the Mycological Society.

Sincerely yours,

Margaret

M.E. Barr Bigelow

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

DEPARTMENT OF BOTANY

6-IV-1976

Dr. M.E.Barr Bigelow
Department of Botany
Universiy of Massachusetts
Amherst, Mass. 01002

Dear Margaret:

Regarding the editorial staff, I would suggest first that if in your judgement, the members whose terms are ending have done a useful job, they should be invited to stay on for another (and last) term, unless circumstances otherwise dictate.

If either or both members decline to serve again or if you choose not to retain them, the following replacements and alternates might be considered. Of course there are many other good candidates, but I limit myself to these four whom I can highly recommend.

1. Luella Weresub - Highly competent in a broad field of fungi; taxonomy and nomenclature her forte.

2. Ron H. Peterson - Homobasidiomycetidae; good all-around man.

3. Don R. Reynolds - Ascomycetes and Imperfects. (Address: Los Angeles County Museum of Natural History, 900 Exposition Blvd, Los Angeles, Calif. 90007.)

4. Charles L. Kramer - Ascomycetes.

Sincerly,

B. Lowy

B. Lowy