Adams

Charles C. Adams
(New York State Museum, Albany)

Correspondence with and re: Charles C. Adams.

The following letter with out slight modification was sent to six different men who know of Adam s work or have had dealings with him. The brackets are the long-hand reply from Dr. Victor E. Shelford, man of few words who prefered to simply return the letter sent to him with his marginal comments.

Dear Dr. ____ .

At Rutgers University our supper ecological seminars held at our professors' homes have taken up the work of people in the field of ecology. These seminars are informal gatherings with about fifteen to twenty people present. We assemble pertinent biographical and autobiographical material on various ecologists.

I am writing you because I understand from Dr. Buell that you know Dr. Charles C. Adams or are familiar with his work. I wonder if you would be so good as to send me your written comments to these questions:

1) What paper or papers of Dr. C. C. Adams do you consider among his best contributions to ecological research? (Isle Royale papers: bird succession important)

2) What, in your opinion is his contribution to ecology?

3) What kind of teacher is Dr. Adams and what were his techniques? (good)

4) What facets of his personality are most interesting to you? (He always objects if he did not propose)

5) What would you say was his outlook on life? ("He is the material from which martyrs are made" S. A. Forbes)

6) Any points not covered in these questions which would be of interest? (He stresses history (geological) and stress older biologists.)

Bear in mind that the answers to these questions are not circulated beyond our seminar group, but are helpful in rounding out the total picture of the man under consideration.

I am enclosing a self-addressed, stamped envelope for your convenience in replying and look forward to receiving your reply preferably as early as is convenient for you. (We have our seminar next Thursday, April 15th and a prompt reply will be greatly appreciated)

Thank you.

Sincerely yours,

Stephen Collins

(my address ommitted to save space)

April 10, 1954

Dear Mr. Collins: [Re: Charles C. Adams]

Your letter was awaiting me on my return from a two weeks absence. Unfortunately, work has piled up so that I can only give you the briefest kind of an answer. Unfortunately, also, I do not have a complete bibliography of Adam's work.

By coincidence, I spent a couple of hours with him in Albany Thursday morning, and found him still vigorous and active, although somewhat slowed down by age. Briefly, I should say he has distinguished himself for the following things:

1) His work on geographical distribution in the BIOLOGICAL BULLETIN, Vol. 3, 1902. This was the forerunner of Transeau's work on forest centers, Waller's later work on crop centers.

2) His establishing of the Roosevelt Wildlife Preserve and the associated biological station.

3) His early understanding of the importance of human ecology, see ECOLOGY: 26, 1945.

4) His pioneer work on the dynamics of communities. I do not have his reference, but he is now elaborating the idea in a large manuscript.

Like many men of exceptional greatness, he has played in hard luck. His ideas on ecology were not received by Marks at Harvard or by Ward at Illinois, and in the last ten years I have observed that his attempts to make his fellow ecologists realize the breadth and importance of their subject have been dismissed as the annoying persistence of an old man. He was a founder, and steady supporter of the Ecological Society, and in my judgment, is one of the few who realize what its potentials in American science ought to be.

I am sorry not to have more leisure to give you more details, but trust that this much will be useful.

Sincerely yours,
(signed)
Paul B. Sears

April 10, 1954

Dear Mr. Collins: [Re: Charles C. Adams]

I have your letter with questions about Dr. Adams. There are two reasos why I am not going to answer in detail. In the first place I am exceptionally busy at the moment and cannot take the time to give the necessary thought to some of the philosophical aspects of your interest. Secondly, I only know Dr. Adams professionally and therefore cannot speak in terms of his teaching or general philosophy.

I think that his papers dealing with the southeast as a center of distribution have been outstanding among his contributions.

My recent associations with Dr. Adams have been almost exclusively in terms of the operation of the Ecological Society. In his later years he has been a promoter of causes and since he invariably wished to operate on a scale of financing which the Society could not truly afford with its limited budget he gave the Council of the Society constant trouble over a period of several years. Again, I'll not go into this because Dr. Buell was involved as much as I and can tell you about some of our peculiar difficulties in this respect. Please understand that I do not mean that he was trying to put anything over on anyone or to mismanage Society funds. He is always so enthusiastic about whatever he goes into that he is apt to drive ahead with his plans regardless of what other people's reactions may be later. Our problem usually was that he would present the Society with an accomplished fact when the budget had originally been made up without his plans incorporated in it.

You would perhaps be interested in just a minor sidelight of a kind of feud that went on between Dr. Adams and Dr. A. S. Pearse over a period of years. As I've indicated, Dr. Adams is inclined to want to have things done right now. As a result he frequently has been known to use telegrams when he could have written letters. It was his standard procedure to contact Dr. Pearse with telegrams sent collect. Whenever he did this, Dr. Pearse would immediately send him a telegram from 2 to 10 times as long as the one he had received and of course he always sent it back collect. Although they had the greatest respect for each other they carried on a needling of this kind over a period of many years. At various times I have spent days in the field with them together. Since both are most outspoken individuals and neither is at all subtle in his approach to anything I can assure you that it was an interesting experience in human relations as well as in ecology.

Sincerely,(signed)
Henry J. Oosting

Dear Mr. Collins: [Re: Charles C. Adams]

Your letter of April 7 is rec'd. I regret the rush of the answer since I could do a better job if time permitted. I have known Dr. Adams for some years, in fact have had good acquaintance with many of the "wheel horses" of early Amer. ecology. I'll return your question and avoid having to repeat them.

1) Principal papers & books:
   1902. SE U S as a center of geographical distribution of flora and fauna. Biol. Bull. 3: 115-31.
   1905 Postglacial dispersal of N A biota Biol. Bull. 9:53-71
   1913 Guide to the study of an. ecol. Macmillan Co., N Y
   1917 Relation of general ecology to human ecology. Ecol.16: 316-35.
2) His principal campaign has been for <u>integration</u> of ecology; plant-animal, general-human, environmental-ecosystem, autocoogy-synecology, ecosystem-economics & government. In other words, he has sought to hold the whole field into a logical whole by means of its binding general principles.
3) As a teacher his emphasis was on field work and concrete experiences He believed in summer field schools.

4) His enormous command of the literature--including the possession or control of thousands of classic books in ecology and all its marginal fields.

5) He knew literature so much better than those who should quote him. For instance, most of the ecological world credit "Prairie Peninsula" to Transeau. Dr. Adams had formally used this term much earlier.

6) His outlook on life has been critical yet charitable. He views the foibles of human nature with humor and though he attempts to be idealistic, he is aware of the practical limitations on human beings and their institutions.

7) Dr. Adams took great interest in museums as another means of concrete teaching method, second only to the direct field work. He is now at work on two manuscripts of practically book length. One is nearly ready for setting in type, the other (geographical) required another 3-5 years work. I greatly hope his energies will continue strong.

Yours, (signed)
Paul C. Lemon

Dear Mr. Collins: [Re. Charles C. Adams] April 12, 1954

I appreciate your letter asking for information about Dr. C. C. Adams--But am afraid you are going to draw a blank so far as material useful for your seminar group is concerned.

I did not know Dr. Adams- nor had I heard anything particularly about him- until became Secretary of the Ecological Society. During the years of my tenure I crossed swords with Dr. Adams many times. Most of the things I would have to say would not be complimentary to him. I think when I first knew him he was really beginning to reach his "dotage". His interest during these years have been in promotion of the field of Human Ecology. I have never discovered just what he has meant by it. I think he has been unfortunate in some of his associations during these years. He has become associated with Sociologists and I am afraid many of them have been "dreamers" without much solid training in Science or Ecology as most of us understand them. He seems to want to do big things in a promotional way. He wanted to tie up with United Nations programs and get the Society involved in that. He selected Dr. Shea-an ardent New Dealer -- to help him with the effort. Instead of going at it directly Dr. Shea wanted to get an audience with the President (then Truman) and work through the highest political levels. The effort came to naught. Dr. Adams had a Committe supposed to be an Endowment Committee to promote the interests of the Society. He dreamed of a National Headquarters and building, Library, etc. He succeeded in arousing the enmity of the members of his Comm. by his arbitary decisions and using their names when they had not agreed to what he was doing. One by one they all withdrew--Allee, Pearse, Miss Hayden and others -- and the Committee broke up with a lot of ill feeling. Dr. Adams would publish long mimeographed reports and insist on their distribution to the Society apart from the regular Bulletin. I refused to print some of his diatribes -and I guess if you would ask him you would find my name is mud. He was making some accusations and personal remarks about Allee and others of his Committee that I felt would do more harm than good.

Those are my personal impressions from my dealings with, and you must make due allowance for I am sure I never saw him at his best. Many of the older members of the Society tell about a time in the early days of the Society when he came to its aid and really saved it from dissolution as a result of internal strife and differences. I do not know this story --but all honor to him for it. I believe his real claim to fame arises from the fact that he got in on the ground floor in Ecolog --was associated with such men as Shelford, Cowles and others in the Chicago group in the early days. I do not know his early history --what institutions he taught in, what work he did. I find in some Bibliographies reference to papers in Human Ecology that came out in the '30's. I have a vague recollection of mention of a study of the growth of a New England or New York State village, that he made - or in conjunction with others. He has undoubtedly stimulated interest in the possibility of extending ecology to the study of man. In doing so he has arounsed controversy and dissention. Whether he has made a real and lasting contribution is something I would hesitate to Judge.

I hope you will be able to get information from some older person of his own generation that knew him as a younger man and knows what he did in those days. Please feel free to use my comments for what they are worth, but remember they come from a prejudiced and biased person. He was a thorn in my side during the days I was Secretary. The Society at a New York meeting passed a resolution committing the Society to offering its help in an advisory capacity to the United Nations Organizations. As Secretary I was instructed to sned copies of the resolution to the President of the United State --and a list of about 25 high officials in the United Nations, the State Department, etc. Dr. Adams asked me to delay doing it until Dr. Shea had paved the way. A year later at Columbus he asked me what replies I was getting. I told him I had no replies because I had never sent them -- that he asked me to wait on Shea before I did anything. He appeared infuriated at first but finally said that of course we couldn't go ahead until the proper channels had been laid open. Three weeks later Shea called me from Wahsingtonton (he was in the Agriculture Department) and in a long conversation informed me that he had been working through this and that person and in just a few days would be able to go in and lay the matter on Mr. Truman's desk. Then we could go ahead with our announcment. That was over six years ago --and I am still waiting for Shea to call. Nor have I heard from Dr. Adams since that time. Don't know how much he has been riding the other Secretaries since that time. Ask Buell.

Best wishes to the member s of your Seminar group. Tell them I am not a good ecologist. I just happed to be the sucker that was available at the time to become Secretary of the Society. I always felt I was sailing under false colors in the job --but I got to know a lot of fine people.

Sincerely, (signed)
Wm. A. Castle

April 14, 1954

Dear Mr. Collins: [Re: Charles C. Adams]

Your letter of April 7 was not received until today so this reply cannot possibly reach you in time for your seminar tomorrow. Nevertheless, I am trying to answer some of your questions, however imperfectly, as Dr. Charles C. Adams is one of my current enthusiasms.

1) Among the papers od Dr. C. C. Adams which I consider to be among his best contributions to ecological research I would list the following, from the extraordinarily rich bibliography of this great man:

(Dr. Taylor lists the following --see publications list for this seminar-- 1902, 1902, 1905,1908, 1913, 1929, 1940 (2) He lists als

1948 The responsibilities of governments for the conservation of renewable natural resources as a phase of human ecology. Proc. Inter-Amer. Conf. Cons. Renewable Nat. Res., Denver, Colorado. Sept. 7-20. p.608-62

1951. The application of biologic research methods to urban areal problems. Sci. Mo., 73 (1) : 39-40

(in preparation) Guide to the study of human ecology: the dynamics and processes of orientation and integration. In preparation, April 14, 1954.

2) A hard question to answer. In my opinion Dr. Adams has been one of the most open-minded leaders in the field of ecology. He has contributed a great deal of detailed field study and information. He has continually emphasize the vital, human applications of the science. Conservation has received much emphasis at his hands. His pointed emphasis on science and democracy as the two greatest discoveries of the human race is worthy of all praise and appreciation.

3) Never had Dr. Adams as a teacher. Judging from his students he must have been an inspiring and effective teacher.

4) Facets of his personality of most interest to me are his enthusiasm, which has not flagged in his so-called old age. His originality, his aggressiveness and his willingness to take risks, should not be forgotten. Then, too, his extraordinary generosity and his dedicated interest in promoting human welfare rather than making money are everlastingly to his credit.

5) On a recent trip which took us through Albany, New York, Dr. Adams offered us his generous hospitality. He has a quite large 8 room house in which he lives alone. In every room are books. Also in the back porch, glassed in, and in the garage. A huge library, the floor, shelves tables, davenports, etc. covered with books. Probably the most complete collection of books on human ecology to be found anywhere in the world within arms length are to be found in Dr. Adams residence. Fortunately he has the complete cooperation of the libraries of Albany, the N. Y. State Library and others. Apparently he knows where all the books are and is intimately familiar with them. To visit him is an enriching and stimulating experience.

6) In spite of his share of rough spots, obstacles, and frustrations, Dr. Adams outlook on life is optimistic and enthusiastic, with a fine background and undercurrent of humor running through it all.

[Re: Charles C. Adams]

7) Some of you young, eager, and influential ecologists and your friends might well cultivate Dr. Adams, as you have opportunity. Get him to come down to Rutgers and give a series of ecological talks to your seminar and to other interested groups, under the auspices of the University Bureau of Lectures, etc. so he could be well paid for his appearances.

Also, help him to find a publisher for his manuscript on "Human Ecology".

It would also be fine if you could put some steam behind the move so aggressively promoted by him with all too little support from the rest of the Ecological Society to secure an endowment, paid secretary, building and real resources for the Ecological Society of America, than which here is no organization in the country, in my opinion, more deserving of support and energetic promotion. Nor is there any organization which could do more to insure a better balanced outlook toward science, human progress, and democracy in America (and elsewhere)

Sincerely yours, (signed)
Walter P. Taylor
Past President, Ecological Society of America

(postscript) The greatest success to your seminar! You are doing one of the most original stunts I have heard of in connection with any seminar.

Dear Mr. Collins: [Re: Charles C. Adams] April 12, 1954

Your letter of March 12 is here, about your seminar meeting for April 16.

As it does not seem practical, at this time, to give you answers to the questions of your February 17 letter, I suggest that you borrow from Dr. Buell the series of Reports, which our Committee of the Ecological Society on Endowment Policy and Program, 1948-52. These reports will give you considerable information on the prospects for ecology. I have great confidence in its future and in its importance.

Also consult the symposium on ecology and human welfare in Ecol. Monog., Vol. 10, No. 3, where you will find additional matter of value. I think that these, with the reprints sent you will give you material for the Seminar.

Finally I will suggest that if you and Mr. Baird care to make an auto trip to Albany some time this spring or summer, I would be pleased to discuss the questions that you raise, and others, and show you the MS. of a book "Guide to Human Ecology," on which I am busy. It is for this reason that at present, my time is so limited.

I have in my home a very interesting collection of ecological literature, some of which would interest you very much, because the collection covers a broad field.

A few years ago two men from the Harvard School of Public Health flew to and from Albany and we had a busy, near all day session, on human ecology.

Since my retirement I do not have easy accessible secretarial help and that, with my other lack of time, prevents me from doing as much to help as I would be pleased to give you.

With best regards.

Very sincerely, (signed)
Charles C. Adams

Dr. Charles Christopher Adams

by
Stephen Collins
Department of Botany
Rutgers University
New Brunswick, N. J.

1873 Born July 23 at Clinton, Illinois
1895 B. S. , Illinois Wesleyan
1895-96 Ass. Biologist Illinois Wesleyan
1896-98 Asst. Entomologist, State Lab. Nat. Hist. Illinois
1899 M. S. Harvard
1900-03 Fellow, University of Chicago
1902 Summer asst., Chicago
1903-06 Curator, University Museum, Michigan
1906-07 Director, Cincinnati Society of Natural History, Mus. Curator
1908 Ph. D. from University of Chicago
1908-14 Associate animal ecologist, Illinois
1914-16 Assistant Professor, Forest Zoology, NYS Col. Forestry, Syracus
1916-26. Professor, Syracuse
1919-26 Director, Roosevelt Wild Life Forest Expt. Station, Syracuse
1926-43 Director, New York State Museum, Albany. (retired)

member of the following societies

A. A.
Society of Natural History
Ecological Society (presdient in 1923)
American Society of Mammalogists
Fisheries Society
History of Science Society
Association of Geographers (vice-president)
Association of Museums
British Ecological Society

The data above was taken from American Men of Science, 1944.

Publications of Dr. C. C. Adams

The following list of papers by Dr. Adams was prepared through consulting bibliographies of his papers, indexed lists of authors from scientific journals, and state bulletins, including university bulletins. This is not a complete list, nor has it been checked by Dr. Adams, but it does contain most of his important papers.

1892 Mollusks as catfish food. Nautilus 5: 127.

1901 Base leveling and its faunal significance. Am. Nat. 35:839-852

1902 Postglacial origin and migrations of the life of the northestern U. S. Jour. Geog. 1: 303-10; 352-57.

1902 Southeastern United States as a center of geographical distribution of flora and fauna. Biol. Bull. 3: 115-131.

1904 On the analogy between the departure from optimum vital conditions and departure from geographic life centers Science 19: 210-211.

1905 Postglacial dispersal of North american biota Biol. Bull. 9: 53-71.

1906 An ecological survey in orthern Michigan. Mich. Board Geol. Surv. Report 1905: 9-12.

1907 (Review) The crawfishes of the state of Pennsylvania by Arnold E. Ortmann. Science 25: 897-901.

1909. An ecological survey of Isle Royale, Lake Superior. Rept. of Board of Geol. Survey of Mich. for 1908 (1909):1-468.

1908 The ecological succession of birds. Auk. 15: 109-153.

1909 Isle Royale as a biotic environment. Mich. Geol. Surv., Ann. Rept. 1908 1-52.

1909 The ecological succession of birds. Ann. Rept. Mich. Geol. Surv. 1908: 121-154.

1910 The relation of field excursions to the activities of local museums. Proc. Amer. Association of Museums 4: 112-124.

1913 A Guide to the Study of Animal Ecology. N. Y. Macmillan 1-183.

1915 An ecological study of prairie and forest invertebrates. Bull. Ill. Lab. Nat. Hist. 11: 33-280.

1915 The variations and ecological distribution of the snails of the Genus Io. Mem. Nat. Acad. Sc. 12: 1-92.

1915 An outline of the relations of animals to their inland environments. Bull. Ill. State Lab. Nat. Hist. 11: 1-32.

1916 Diversified forestry. Jour. N. Y. S. Foreseters Assoc. 3:25-26.

1916 Science and progress in the protection of forest, fish and game animals. Calif. Fish and Game 2: 19-22.

1916 Game and game vermin in New York. Amer. Game Protective Association Bulletin 5: (2) 10.

1916 --Hankinson, T. L. Notes on Oneida Lake fish and fisheries. Tans. Amer. Fish. Soc. 45: 155-169.

1917 The new natural history--ecology. Jour. Am. Museum. 17: 491-94

1919 The relation of forest animals to the welfare of NYS. New York Forestry 6: 19-22.

1919 Migration as a factor in evolution: its ecological dynamics. American Naturalist 52: 465-490 ; 53: 55-78.

1919 The New York State Wild Life Memorial to Theodore Roosevelt. Natural History 19: 726-729.

1919 An ecological survey of the Palisades Interstate Park. Empire Forester, N. Y. State College of Forestry 5: 12-18.

1920 --and George P. Burns, T. L. Hankinson, Barrington Moore, and Normal Taylor. Plants and animals of Mount Marcy, N. Y. Ecology L: 71-94, 204-233, 274-288.

1920 The relation of natural history and ecology to public forest park. N. Y. S. College of Forestry, Syracuse Univ. Bull:10:11-1

1921 Roosevelt wild life state memorial 1: (1)

1921 Appropriateness and appreciation of the Roosevelt wild life memorial 1: (1)

1921 Suggestions for research on north american big game and fur-bearing animals 1: (1)

1921 Suggestions for management of forest wild life in the Allegany state park. 1: (1) (This reference and the three preceding is can be found together in the Roosevelt Wild Life Bulletin, volume one.

1921 Delights of the wild forest trails. NYS Service Mag. 5: 100-3.

1923 Notes on the relations of birds to Adirondack forest vegetation. Roosevelt Wildlife Bulletin 1: (3) 487-519.

1923 Maintenance of the fur supply. American Fur Buyer 12: (6) 54.

1925 The conservation of predatory mammals. Jour. of Mammalogy 6: 83-96.

1925 The relation of wildlife to the public in national and state parks. Roosevelt Wildlife Bull. 2: (4) 370-401.

1925 Ecological conditions in national forests and in national parks. Scientific Monthly 20: 561-593.

1925 --Cooper, William S. and C. C. Adams, Robert F. Griggs and Barrington Moore. Glacier Bay, Alaska proclaimed a national monument by the president, note. Ecology 6: 197.

1926 The fundamentals of cooperation among wild life agencies. Proc. Nat. Conf. Outdoor Recreation, 69th Cong. 1st Session Senate Document 117: 66-70.

1926 The economic and social importance of animals in forestry with special references to wild life. Roosevelt wildlife Bull. 3: 509-676.

1928 --and T. . Hankinson. The ecology and economics of Oneida lake fish. Roosevelt wildlife annals 1: (3-4) 235-548.

1929. The importance of preserving wilderness conditions. 22nd rept. of the director of the division of science and the state museum, N. Y. S. Mus. Bull. 279 : 37-46.

1935 Relation of general ecology to human ecology. Ecology 16:316-335.

1936 29th report of the director of the disiôn of science and the state museum, NYS Mus.. Bull. 306+ 1-98. (Note: while Dr. Adams was with the NYS Museum at Albany, he prepared these reports periodically until the time of his retirement)(unless specific articles are mentioned, these reports will not be listed here)

1937 The relation of natural resources to regional and county planning. 30th report of the Director of the division of science and the state museum of N. Y. NYS Mus. Bull 310: 121-41.

1938. A note for social-minded ecologists and geographers. Ecology 19: 500-502.

1940 Symposium: relation of ecology to human welfare, the human situation. Introductory note. Ecological Monographs 10: 309-310.

1940 Selected references on the relation of science to modern life. 102 annual rept. of NYS Museum: 79-96.

1942 School museums, field trips and travel as phases of objective education. 104 annual rept. of NYS Museum: 75-116.

Addenda

1908 Some of the advantages of an ecological organization of a natual history museum. Proc. Amer. Ass'n Museums 1: 170-78, 1907. Reprinted in N. Y. State Mus. Bull. 284: 53-60, 1929.

1940 A historical sketch of the Allegany school of natural history. reprinted from the Historic Annals of the Southwestern New York (Doty, Congdon and Thornton) 775-86. N. Y.

1941 The New York Stat Museum's historical survey and collection of the New York Shakers. Y. Y. State Mus. Bul. 323: 77-141.

63. J. M. Swan and V. du Vigneaud, *ibid.* 76, 3110 (1954).
64. C. R. Harington and R. C. G. Moggridge, *J. Chem. Soc.* 1940, 706 (1940).
65. P. G. Katsoyannis and V. du Vigneaud, *J. Am. Chem. Soc.* 76, 3113 (1954).
66. C. W. Roberts and V. du Vigneaud, *J. Biol. Chem.* 204, 871 (1953).
67. C. R. Harington and R. V. Pitt Rivers, *Biochem. J. London* 38, 417 (1944).
68. J. H. Burn, *Quart. J. Pharm. Pharmacol.* 4, 517 (1931).
69. J. H. Burn, D. J. Finney, L. G. Goodwin, *Biological Standardization* (Oxford Univ. Press, New York, ed. 2, 1950), p. 180.
70. J. M. Coon, *Arch. intern. pharmacodynamie* 62, 79 (1939).
71. S. P. Taylor, Jr., *Proc. Soc. Exptl. Biol. Med.* 85, 226 (1954).
72. V. du Vigneaud, H. C. Lawler, E. A. Popenoe, *J. Am. Chem. Soc.* 75, 4880 (1953).
73. F. Sanger, L. F. Smith, R. Kitai, *Biochem. J. London* 58, vi (1954).

Charles C. Adams, Ecologist

Charles Christopher Adams was a pioneer in and one of the creators of ecological perspective. Through the kindness of his daughter, Harriet Adams, I have at hand his bibliography of some 154 titles, including six unpublished works. This list makes clear not only the gradual development of his personal interests, but also his service in broadening our concepts of ecology and its role. Although it is not a matter of record, much that he accomplished was done in the face of inertia and even opposition on the part of established and conventional influences.

Adams was born in 1873. He began publishing notes on natural history before his graduation from Illinois Wesleyan University in 1895. Several papers on invertebrates appeared after he did his master's work at Harvard University in 1899, but it was his fellowship at the University of Chicago (1900–1903) that appears to have been decisive. Here his contacts with Cowles, Salisbury, and Transeau resulted in notable papers on base leveling and on Pleistocene climatic change in relation to faunal problems. Together with Transeau's paper on forest centers, these marked a resumption of serious American biogeographical study, initiated long before by Asa Gray, but with the added advantage of new information on geomorphology and Pleistocene history.

From 1903 to 1907, Adams was in museum work, first at the University of Michigan, then at the University of Cincinnati. He quickly developed the idea that the museum is a vital teaching center that is linked to the community and its natural history. This idea was to remain a guiding motive for the rest of his life; it expressed itself at this time in his ecological survey of central and northern Michigan. In 1904 he organized and directed the Isle Royale Ecological Survey. In 1908 he returned to Illinois as associate in animal ecology, where he prepared his *Guide to the Study of Animal Ecology* and a detailed report on the various communities of invertebrates in the prairies and forests of eastern Illinois.

In 1914 he became forest zoologist of the New York State College of Forestry at Syracuse. There he organized the Roosevelt Wildlife Experiment Station and directed its activities until he became director of the New York State Museum, a position he kept until his retirement in 1943. At these two posts, much of his most significant work was done, although it was often concealed behind the routine into which it was incorporated. He continued his idea of ecological surveys at the Palisades, Mount Marcy, Alleghany State Park, and in the Rochester region. He wrote much on wildlife and fisheries in relation to forestry, and after an initial paper on conservation in 1915, he gave that problem increasing emphasis.

Ecologists, notably in the Forest Service, had long appreciated the possibilities of their subject as an applied science. But it seems to me that Adams did more than any other individual in America to give this idea comprehensive form. In 1930 he turned his attention to the synthesis with human ecology, using the Shaker collection in the Albany Museum as a basis. In orderly fashion, he proceeded to enlarge the scope of his interest, through schools, water resources planning, and regional and urban planning.

He brought to the attention of ecologists the work of Mumford, Mackaye, Lindeman, Benedict, and Patrick Geddes, whose concepts of the living museum and of ecological planning and whose experience, in some respects, paralleled his own. Adams organized symposia on human ecology and insisted that ecology, with its tremendous possibilities for human good, ought to be underwritten on the same scale as geography, geology, and the physical sciences.

Keenly aware that ecology is a study of process, he saw the importance to it of records and archives, and of some central institute where these could be conserved and where workers could be housed with facilities fitting their value to society. He never ceased trying to rouse ecologists to appreciate their own importance, warning them that unless they could do so, their job would be taken over eventually by others.

One of the founders and a past president of the Ecological Society of America, he was also active in the Association of American Geographers. He maintained an extensive and lively correspondence, and he was unusually enterprising in hunting up those whose ideas interested him. Physically he was stout and vigorous. His tastes were simple, but his energy prodigious. His home was itself a museum and library combined, overflowing with books and pamphlets which he governed by his own mysterious kind of order.

At the time of his death in 1955, he was working on a *Guide to the Study of Human Ecology: The Dynamics and Processes of Orientation and Integration.* Like his earlier guide to animal ecology, this was to be an annotated bibliography, but so set up as to be immensely useful to any serious student.

His library, notes, and other material have been presented by his daughter to Western Michigan State College at Kalamazoo, where they will be kept available, as he wished that all important ecological materials might be, for future workers. There they will form a nucleus for the Charles C. Adams Center for Ecological Studies under the direction of Daniel Jackson.

PAUL B. SEARS

Yale University
New Haven, Connecticut

Aikman

J. M. Aikman
(Iowa State College)

IOWA STATE COLLEGE
of Agriculture and Mechanic Arts

Department of
Botany and Plant Pathology

April 15, 1957

Mr. George Van Vechten
Department of Botany
Rutgers University
New Brunswick, New Jersey

Dear Mr. Van Vechten:

I became interested in ecology and began the quantitative investigation of the vegetation and habitat factors of two contrasting plant communities with Dr. Claude J. Shirk at Nebraska Wesleyan University in the spring of 1916 while serving as an undergraduate lab. assistant in Botany. His intense interest in the subject and this early apprenticeship was no doubt an important factor in influencing me to try from that date to become an ecologist. I am beginning to think that my apprenticeship is about over and that I may at last be approaching my goal. This is not an attempt to be facetious but rather to emphasize the broad preparation and the long practice necessary.

The master's degree in ecology with Dr. Shirk and the Ph.D. degree with Dr. J.E. Weaver at the University of Nebraska were obtained during the time I was serving on a full-time basis as assistant and associate professor with Dr. Shirk.

Since 1927 my research, for the most part, has been conducted under projects of the Iowa Agricultural Experiment Station. These activities have been in addition to a full-time teaching program in the Science Division except for a 10 year period, 1937 to 1947, when I was on a half-time research basis as supervisor of a cooperative research project of our Experiment Station and the U.S. Soil Conservation Service. During this time my graduate students were employed as U.S. Cooperative agents by the above Service. I have had two leaves of absence from the college; one year as senior botanist with the U.S. Forest Service on the shelter-belt project, 1934-35 and two years as principal research advisor for the U.S. Foreign Agricultural Service in Ecuador 1952-53.

A cursory examination of the list of publications will be sufficient to acquaint you with the direction in which ecological research has developed here. Some of the breaks apparent in the list are filled by separates of student publications which I sent. Several of these are out of print.

You will note that these papers serve as reports of projects on which we have been working but at the same time an attempt was made to get at the solution of basic ecological problems. In practically every case we have been investigating the plant-environment relationships of the species and groups involved. We measured the growth response and attempted to correlate the two by analysis of the data. The microenvironments investigated differed in space, time or treatment. You will also note from the papers that we have had excellent cooperation with all the related departments of the college and, in several cases, of the U.S.D.A.

As to your questions, Drs. Buell and Small can probably answer them better than I. There is more ecology being taught and practiced today than ever before but much of it is included in other subject matter fields. This is true in the different phases of Botany and Zoology as well as in the applied fields. Some new fields have taken over large segments of it; for instance agricultural meterology in departments of agronomy. One of our greatest opportunities* would seem to be in attempting to organize ecological thought and action, wherever found, and direct it toward the solution of problems of broad scope which have at their center the problem of the interactions of plants and their environment and require for their solution the cooperation of workers in many disciplines both basic and applied.

Sincerely yours,

J.M. Aikman
Professor

JMA:ca

*There are many others.

allee

W. Clyde Allee
(University of Chicago)

WARDER CLYDE ALLEE--HIS LIFE AND WORK

1885-1955

Adapted from a biography by Karl Patterson Schmidt

Warder Clyde Allee was born on June 5, 1885, on a farm near Bloomingdale, Indiana. His father, John Wesley Allee, with names in his ancestry like Reed, Parkhurst, Warner, and Wesley, had been orphaned as a child, and grew up at the homes of various relatives in the Bloomingdale region. He was a Methodist, and joined the Society of Friends to marry Mary Emily Newlin, whose Quaker ancestry extended back to the seventeenth century in England and Ireland.

Clyde Allee attended a one-room country school, taught during several years by Mrs. Florence Rawlins Chapman, who was both teacher and friend of the family. He led his class in scholarship, in spite of occasional interruptions for farm work. At Bloomingdale Academy he was again at the head of his graduating class, and the winner of the oratorical contest. After his graduation, at the age of seventeen, he taught county school for a year and then the fifth and sixth grades in the Bloomingdale elementary school for another. Then, at nineteen, he was drawn to Earlham College.

Four years at Earlham provided a sound and broad undergraduate education rounded out by active participation in college football, congenial to his athletic frame. An interest in the general field of biology brought with him from the farm was strengthened and confirmed. Upon his graduation in 1908, under the influence of David Worth Dennis, Professor of Biology at Earlham, Clyde Allee undertook advanced studies at the University

of Chicago, where he received the degree of Ph. D., summa cum laude in 1912.

Family background, and the influence of Earlham College, with Quaker staff and a mainly Quaker student body, served to confirm the Quaker mold in which Clyde Allee was cast. Even more important to his confirmation as a member of the Society of Friends was his marriage to Marjorie Hill, in 1912. Her competence as a writer was of the greatest value to her husband in the preparation of his books and scientific papers, for which she served as critic, as unobtrusive collaborator, and on occasion as joint author. In later years she established herself in her own right as an authoress, with a notable series of novels for girls.

During the last two years of graduate work at the University of Chicago, under Victor E. Shelford, Clyde Allee resumed his teaching career, as Assistant in the Department of Zoology. In the ten years after receiving his degree, his teaching experience was expanded into a firm foundation for his subsequent thirty years' continuous tenure at Chicago, 1921-1950. In 1912-13 he was Instructor in Botany at the University of Illinois in Urbana; in 1913-1914 he was Instructor in Zoology at Williams College, in western Massachusetts; in 1914-1915 he was Assistant Professor of Zoology at the University of Oklahoma; and then for six years he served as Professor of Biology at Lake Forest College, north of Chicago, where he followed James G. Needham and Cornelius Betten.

One of the most significant of the influences that molded Clyde Allee's interests and his research career grew out of the departmental relations at the University of Chicago. Frank R.

Lillie, long head of the Department at the University, was one of the founders of the Marine Biological Laboratory at Woods Hole, Massachusetts. Under the Lillie influence, from his graduate years on, Clyde spent summer after summer at that great research center (Summer Instructor, 1914-1921, Director, Invertebrate Course, 1918-1921; Trustee from 1932 to his death).

Subtle influences from the older culture of the Hill home in southern Indiana were gently and harmoniously infused into the Allee household when the family came to rest at the University of Chicago. A son and two daughters were born. The book-lined walls at home were matched at Clyde's office by his vast array of biological books and pamphlets. Instead of contrasting, the two collections served to emphasize the unity of literature and science. Homes like that of the Allees form one of the most significant of the educational influences at every university; they reach into the classroom and to the campus to mold the hearts and the fundamental attitudes of mind of the generations of students who come into them.

Clyde Allee's teaching was so intimately related to his research that to give an account of his researches and of the books and papers that embody its results is to give some account also of his teaching. Of 181 research papers, reviews, and popular articles, 70 were in joint authorship with students or colleagues; he joined with colleagues in writing several of his books, ending with the massive Principles of Animal Ecology, with five authors. He clearly made a conscious effort to exemplify in man, at the university level, the principle of cooperation among animals.

The research papers begin modestly in 1911, while he was still a graduate student, with Seasonal Succession in Old Forest

Ponds, a plainly ecological topic; there is then a long run of reports on the reactions of the isopod Asellus to currents, with experiments modified as to chemical and physical and social factors. These papers exhibit the influence of Victor E. Shelford, under whom he had taken the doctoral degree in the field of ecology. They reflect also the focus upon ecological succession at the University of Chicago resulting from the pioneer studies of H.C. Cowles on the history of the plant formations in the nearby Indiana dunes region, at the southern end of Lake Michigan.

A contrasting area of profound ecological interest had opened up to him at the Atlantic seashore. As director of the invertebrate course at the Marine Biological Laboratory (1918-1921) he was thrown into intimate contact with the patterns of distribution of marine animals, and with the laboratory study of such problems. This made him in the best sense an experimental ecologist.

With these varied interests, Warder Clyde Allee was on the threshhold of his mature career as a productive scholar in 1922. Then, in 1923, came the first of the tragic family calamities in the Allee history. The accidental death of the ten year old son, in a street accident, on his way to school with his younger sister, struck a mind-rocking blow to both father and mother. The psychological blow fell hardest on the mother. For some years her husband's major preoccupation was to comfort and strengthen her.

A major diversion of both minds from their internal grief-fixation was presented by the opportunity to spend the winter of the year 1924 at the Barro Colorado Island Laboratory in the Panama Canal Zone. He was one of the first to climb to the tree

top canopy, and to engage in actual measurement of its environmental factors. Scientific papers about the Panama forest were written; more profoundly important was a little popular book about their experience, Jungle Island, written in the joint effort of Clyde and Marjorie Allee to master their consuming inward grief, and to turn outward and forward again.

A special phase of the Allee researches was inaugurated in 1923 by a little paper in the Condor: "Animal Aggregations. A request for Information." The experimental approach to the analysis of the causes that produced the phenomena of aggregation and to the study of the resulting effects upon the animals involved, was already familiar. The new researches grew directly out of the early series of experiments with Asellus, the little crustacean found in the local fresh waters. By 1926 a long series of research papers under the general title Animal Aggregations had begun to appear; it continued as the main field of his own and of directed student research long after the appearance of his book, Animal Aggregations, in 1931. In brief, the principal results of this research program were the repeated and conclusive demonstrations that there is an unconscious need for the presence of fellow individuals in many species among the lower animals, and in most of the higher; That there is, in effect, a deleterious effect of under-crowding as well as the more familiar one from over-crowding. The phenomenon of a better group-survival, as contrasted with individual survival, was tested against such artificial environmental factors as poisons, and against unfavorable natural factors such as oxygen or carbon dioxide deficiency. More important was the demonstration of the reality of an unconscious cooperation, which he referred to as proto-cooperation,

in a wide diversity of animal forms. The further insight that proto-cooperation supplies the natural foundation for both unconscious and conscious cooperation among the higher animals, in their various levels of social and community organization, led to the next segment of the Allee research program.

Early in 1930 the first indications of a most disturbing paralysis of the lower limbs began to appear. The paralysis increased, and was diagnosed as a spinal tumor. The problem was faced, and an operation was performed by the great neurosurgeon Dr. Percival Bailey. Recovery was rapid, and Clyde Allee could resume his teaching and research; but only to have the too-familiar symptoms recur, so that by 1933 a second excision of the tumor was required. Then after a second recovery, the tumor again slowly returned. Five years after the second operation, in March, 1938, a third operation was performed. The operation was again successful; the patient lived; but the damage to the thrice operated spinal cord was permanent. For the rest of his life, Clyde Allee was confined to a wheel chair except as he learned to swing himself by his powerful arms from chair to car seat, or when (as often) he was carried over the shoulder by his student-attendant. The physical problems that face a man with lower abdomen and legs completely paralyzed are grave. Some hours of care were required every day to prepare the body of this undaunted teacher and researcher for the day's lectures, student conferences, committee meetings, discussion of departmental problems with colleagues, editorial work, and the occasional doctoral examination. It was characteristic that Clyde Allee refused to accept any consideration of his disability. He was particularly valued as a committee chairman, for he neither

forced the proceedings nor let them drag. He carried a full teaching load, met every class, and continued to direct the research of devoted students.

Studies of social organization by observation and experiment grew naturally enough out of the aggregation studies. The first title in this new research program, in 1934, was The social order in the flocks of the common chicken and the pigeon. With a strong overlap of studies from the previous period, it is the words behavior and group behavior, social order and social organization, and leadership and dominance that give the clues to the nature of the individual studies in this field. These lent themselves to the kind of professor-student relationship that became especially necessary, under the new physical handicap of the wheel chair.

For the distinguished series of Norman Wait Harris Lectures at Northwestern University in 1937, he was forced to put together into simple form the results of a quarter-century of active research. The chapter headings of the resulting book, The Social Life of Animals, outline this research, and again contain the seeds of the thoughts that dominated much of his later writing: I. Science versus metaphysics; II. History and natural history; III. Beginnings of cooperation; IV. Aggregations of higher animals; V. Group behavior; VI. Group organization; VII. Some human implications; VIII. Social transitions.

The synthesis of information in a given field into handbooks and larger treatises becomes a necessary outgrowth of research. Animal Life and Social Growth (1938) was such a synthesis of Clyde's own work and of his own thinking. Another handbook, Ecological Animal Geography, had meanwhile been growing as a

joint product of the partnership between Clyde and Karl Schmidt.

A second synthetic work, in which cooperation of a group of authors was to become a major feature, was already in the offing. In 1939 a four-page statement in Science, Concerning Ecological Principles, by Allee and his colleague Thomas Park, was published. This had already been presented to the "Ecology Group." Ecological principles became the preoccupation of Clyde Allee and Tom Park, and others were soon drawn in--Alfred E. Emerson and Ralph Buchsbaum, from the same department, Professor Orlando Park, of Northwestern University, one of Dr. Allee's distinguished former students, and Karl Schmidt. Regular meetings of the authors began as early as May 1939, and the contract for a book was signed with the W.B. Saunders Company, of Philadelphia, in 1942. Clyde Allee had agreed to continue the leadership in the project and the chairmanship of the meetings. Ralph Buchbaum withdrew from the group on account of army duties in 1943. As work on the book progressed, the interruptions implicit in multiple authorship delayed first one part of it and then another.

The life of the indispensable leader and chairman was beset by further calamity, the most cruel blow of all -- the death of Marjorie Hill Allee in 1945. The depth of Clyde's love for her could be appreciated only by his most intimate circle, and his need of her was inexpressible.

Cared for now by his daughters, when yet scarcely adjusted to the loss of his wife, an extraordinary accident befell him in February, 1946. The home elevator, which by fixed family habit had always been in readiness for his arrival, had been left open. Dr. Allee wheeled himself directly into the shaft and fell vertically eight feet, landing squarely on his head on the concrete floor.

Clyde Allee was miraculously partly but irrationally conscious, and was a difficult and stubborn patient. When the ambulance arrived, he quite refused to lie on the stretcher. It was necessary to put him in his familiar wheel chair, and he insisted that he was going to the laboratory for the afternoon's experiments. He slowly improved, at first with speech painfully slowed, and with large segments of memory and other faculties blocked out; then with gradual further improvement, to the astonishment of friends and surgical staff alike, he recovered.

The shock of the accident to the other four co-authors of Principles of Animal Ecology was compounded by concern as to the fate of the work. Under this stimulus they took up the lagging work with renewed vigor and the book was finished in 1948 and published late in 1949. It had a favorable reception in the ecological world.

It was in the graduate school that his teaching was pre-eminent. At the research level he made life-long friends of his students, and displayed more than ordinary interest in their careers after they left the University. This relation was often carried beyond interest to effective aid.

Another long-term segment of the Allee career was his active interest in the professional scientific societies to which he belonged. He was fellow of the American Association for the Advancement of Science, Vice-President in 1942; member of the American Society of Zoologists, which he served as Secretary from 1918 to 1924, and as President in 1936; member of the American Society of Naturalists; a member of the American Entomological Society of America; and was member or fellow in many other organizations. He was a charter member of the Ecological

Society of America, of which he was President in 1929. One more long continued activity remains to be mentioned -- his nearly twenty years of editorship of Physiological Zoology.

Clyde Allee was a profoundly religious person, and lived his life within the framework of an organized religious group, the Society of Friends, commonly known as the Quakers. His services to the Friends included active participation in the 57th Street Meeting, in Chicago and chairmanship of the Chicago office of the American Friends Service Committee. Beyond this, he served as Trustee of Earlham College from 1925 to 1939; when his paralysis became confirmed, and made attendance of the annual meetings difficult, he gave up this congenial duty.

Within the Society of Friends he represented the extreme of liberalism with the minimum of preoccupation with theology. That Marjorie Hill had aided in that adjustment is evident; that it was complete and forthright appears in his religious confessions of faith in 1943, when he was fifty-eight, in the essay Where Angels Fear to Tread, in which he writes:

"Religion has much to learn from science in objectivity, in willingness and courage to follow evidence fearlessly, and even in judging what constitutes valid evidence. ...To me "God" is a possibly permissible personification of all the best that the human race has been able to think and do and of all the beauty we have created, together with all the natural beauty we can appreciate."

The directions of much of Clyde Allee's research and thought, especially in his later years, exhibit the impact of his scientific outlook upon his personal religious history. He became preoccupied with the problem of the evolution of human ethics

out of the crude struggle for existence, evisaged often in terms of individual combat. It is not surprising then that he was repeatedly asked to contribute to or take part in such meetings as the Fifth Conference on Science, Philosophy, and Religion (1945), and the Colloques Internationaux du Centre National de Recherche Scientifique (in Paris, in 1950).

As the age of 65 and retirement from his professorship approached, Dr. Allee had some hope of persuading the authorities that his case should have individual consideration, with extension of his tenure as long as he might be able to carry his full load of teaching and research. When this proved impracticable, retirement was accepted as a kind of challenge. He had been offered the Head Professorship of the Department of Biology at the University of Florida at Gainesville, and he at once accepted. There were personnel problems in the Florida situation within the staff of twenty-one; and there was a considerable imbalance in the representation of the biological subsciences. These factors had made the choice of a chairman from outside the University of Florida desirable. The transfer from Chicago to Florida was negotiated successfully; and Clyde Allee took on challenging duties for a five-year term. His friends were first dismayed and then astonished at his immediately successful coping with the new situation.

As the years flowed past, little change in Clyde Allee's life was to be discerned; there were research papers still, though with new names as junior authors; there was Quaker meeting to attend on First Day; and the summers were still spent at the stimulating but far from peaceful Woods Hole Laboratory.

Married students took over the management of the Allee household and cared for his transportation to and from house and office, as had been the arrangement in Chicago after the marriage of both daughters, and in fact before. The necessary transition from one couple to the next was always a hazard.

In 1953 Clyde astonished his friends by plans for remarriage. It seemed to all to be a minor miracle that a woman of extraordinary charm, an old friend of the family, Ann Silver, should have fallen in love with Clyde and evoked a downright youthful response from him. The marriage took place in the new home of the 57th Street Meeting in Chicago.

Ann Silver Allee bought a profoundly sincere and unselfish love into Clyde's life in Gainesville. She brought competence and continuity into the management of the household, and tact and charm into the university social circle. Next only to love, she brought deep understanding, and with these a deep-lying humor that effervesced in wit. There could be no change in the paralyzed limbs; but the long shadow of misfortune was lifted from Clyde Allee's mind as Ann evoked his reminiscences, and as he learned to laugh with her, with a spontaneitythat had been wanting for thirty years. There was a happy visit from both daughters, with their small flock of brisk grandchildren. There should have been more happy years than the scant two that were allotted to this extrordinary idyll.

On Monday evening, the fourteenth of March, Clyde had a chill after the evening meal. There was a flare-up of a kidney infection that had made his life precarious for seventeen years. This time it did not yield at all to treatment in the hospital; for a day he was irrational, and then sank into a coma from which

he did not awake. Clyde Allee's breathing stopped on the morning of March eighteenth, 1955. Only three months of his contract with the University of Florida remained unfilfilled, and only three months lacked of his reaching the Biblical span of three score and ten.

Toward the end of his career, a tract of some 200 acres of hardwood forest in the vicinity of the Allee farms was purchased with intent of establishing continuing ecological studies in this distinctive environment. This, he thought, would be an appropriate memorial. It would remind students in future generations of his own pioneer studies in Ecology, and provide the stimulus for new ones.

Condensed from an unpublished manuscript by Karl Patterson Schmidt.

Dr. Allee's personal library has been given, intact, to Earlham College. Karl Schmidt has most of his notes and papers, and his letters are in the Library of Congress.

From an application for a grant from the National Science Foundation for the period from July 1, 1955 to June 30, 1960:

"Although I am a paraplegic and have worked from a wheeled chair since 1941, it is my conservative judgment that I am capable of at least five more years of productive scholarly work. ...my own present health and performance show up well in comparison with the other members of the large Department of Biology at this University.

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Stanley I. Auerbach
(Oak Ridge National Laboratory, Tennessee)

Auerbach

Dr. Stanley I. Auerbach was born in Chicago, Illinois on May 21, 1921. He was raised and received most of his education in the schools and colleges of the Chicago area. He received his B.S. degree and M.S. degree in zoology, specializing in ecology, from the University of Illinois in 1946 and 1947, respectively. His advanced graduate studies in ecology were done at Northwestern University, Evanston, Illinois, again emphasizing animal ecology. He received his Ph.D. from this institution in 1949. Following a year of postdoctoral studies, he joined the staff of Roosevelt University in Chicago, Illinois, where he served as instructor and assistant professor of biology until 1954. That year he joined the staff of the Health Physics Division of the Oak Ridge National Laboratory as a research ecologist. Under his direction, the ecological research program of the Health Physics Division was developed into one of the largest units in the world devoted to research in radiation ecology. A major emphasis of this group has been the study and use of radioisotopes, especially fission product isotopes, as research tools for the study of ecological processes and for the study of ecosystems. Dr. Auerbach was Chief of the Radiation Ecology Section of the Health Physics Division for 10 years and is now Director of the Ecological Sciences Division at Oak Ridge National Laboratory.

He is a Fellow of the American Association for the Advancement of Science and is listed in Who's Who in America and is a member of the following professional societies: American Institute of Biological Sciences, American Society of Agronomy, American Society of Zoologists, British Ecological Society, Ecological Society of America, Entomological Society of America, Health Physics Society, Nature Conservancy, Society of Systematic Zoology, and Wilderness Society.

Among his publications in the field of radiation ecology are "The Soil Ecosystem and Radioactive Waste Disposal to the Ground," "Effects of Gamma Radiation on Collembola Population Growth," "Strontium-90 and Cesium-137 Uptake by Vegetation Under Natural Conditions," "Lethal Effects of Gamma Radiation Upon Segments of a Natural Microbial Population," "Biological and Environmental Behavior of Ruthenium and Rhodium," and "Cycling of Cesium-134 in White Oak Trees on Sites of Contrasting Soil Type and Moisture." In addition to these, he has supervised and directed research by members of his group which has resulted in approximately 350 additional reports and publications in the field of radiation ecology.

Auerbach served as Chairman of the Committee on Radioecology of the Ecological Society of America. He organized the first institute for training ecologists in the use of application of radioisotopes in ecological research. This institute was held for three years in Oak Ridge at the Oak Ridge Institute of Nuclear Studies. By sponsoring symposia and contributed paper sessions at

national scientific society meetings, the Committee has aided in fostering interest in the AEC's program in environmental science. Auerbach has served on ad hoc AEC ecology advisory committees which have been established periodically. He has been an associate editor of the journal ECOLOGY, is a member of the editorial board of RADIATION BOTANY and of the INTERNATIONAL JOURNAL OF ENVIRONMENTAL SCIENCE, is an adjunct Research Professor of Ecology at the University of Georgia, and is a lecturer in zoology at the University of Tennessee. He was one of a selected group chosen by the National Academy of Sciences to study the overall problem of U. S. Civil Defense as it related to radiation ecology. He has served as secretary of the Ecological Society of America (1964-1969); chairman, Division of Ecology (1967-68), American Society of Zoologists; and is a member of the Public Affairs Committee and Study Committee and President-Elect of the Ecological Society. Dr. Auerbach also is a program director in the new International Biological Program. He is responsible for directing the Analysis of Ecosystems Project for the eastern (Deciduous Forest Region) part of the U. S. In addition, he is a member of the U. S. National Committee for the IBP and is Co-Chairman of the Program Coordinating Committee for the IBP; member, National Academy of Sciences Advisory Committee on Research to the Secretary of Agriculture (1969-70); member, Board of Ecological Advisors of the Bureau of Reclamation; member, Board of Trustees of The Institute of Ecology; member, Special Committee on Biological Water Quality of the Ohio River Valley Water Sanitation Commission.

Civic Activities

In the community he has served as Treasurer of the Oak Ridge Nursery School Association for the past 11 years. He has been a member of the Board of Directors and Vice President of Oak Ridge Civic Music Association, member of the Board of Directors of Tennessee Citizens for Wilderness Planning, Treasurer and President of the Oak Ridge Chapter of Bnai Brith, and member of the Finance Committee of the Oak Ridge PTA Council (1968-69). The Auerbach family are members of ORCMA, Arts Council, Playhouse, Oak Ridge Civic Ballet Association and Community Art Center.

OAK RIDGE NATIONAL LABORATORY
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October 26, 1972

Mr. G. Ronnie Best
Botany Department
University of Georgia
Athens, Georgia 30601

Dear Ronnie:

I was extremely flattered to learn that you had selected me as one of the eminent contemporary ecologists. I will try to provide you answers to the questions that you have asked. In addition, I have enclosed copies of various biographical sketches that will provide some of the factual information. I am also including a number of reprints that you may find pertinent.

In responding to your questions, I think that I am going to follow the pattern of a "stream of consciousness" presentation rather than to present the answers to your questions in an organized form.

First of all, my interest in ecology arose from my own experiences as a small boy who spent his summers in Southwestern Michigan. There my curiosity about the natural world was aroused, and I became more and more interested in the out-of-doors and the questions about how and why organisms were there. This internal craving, however, was not brought to fruition until after I returned from the Army and World War II, and in the course of exploring different colleges and universities, I had a discussion with Professor Victor Shelford. In that rather brief discussion he quickly evoked in me a positive reaction toward ecology. Prior to that time in my previous undergraduate school, I had taken a number of biology courses, including field biology, but somehow none of my professors then aroused in me the motivation toward ecology that was latent. Under Professor Shelford I began to quickly appreciate the challenges and interests in ecology. I spent many hours discussing aspects of ecology with him, and began to appreciate the broad dimensions of the field, as well as the many kinds of questions, both basic and applied, that were as yet unresolved. Perhaps more importantly, I also gained from him the philosophy and point of view that ecology represents. I suppose this can best be translated in today's terminology in that I gained the feeling that as an ecologist there were many areas that one could contribute to, not only in the more narrow scientific areas, but in the broader issues of the day wherein an ecological perspective is needed.

Following my initial graduate training under Shelford, I went to Northwestern to work under Orlando Park. Park was a vastly different type of man than Shelford, and my several years association with him undoubtedly had a number

of influences on me, not the least of which was his being in a great degree responsible for setting me on a career path which has culminated in where I am now. Two or three major influences stand out in retrospect from my association with Park. One was an increased appreciation of the importance of field research and of the complexity and dynamics of ecosystems. Park, perhaps in his own somewhat oldfashioned way, led me more into the area of ecosystem analysis and concerns than did Shelford. A second influence from Park was one of the organization of multiple research activities. He, like other ecologists in universities, had to operate on an individual basis, but Park regularly organized his graduate students and class activities into highly integrated endeavors with different students being responsible for inputting different bits of data. As the teaching assistant in charge of a number of those courses, it was partially my responsibility to develop and maintain these organized team efforts whenever they were scheduled. Park also was strongly interested in the taxonomy of a certain family of beetles. I suppose he inculcated in me the appreciation for taxonomy and, at the same time, it made me aware of the attitudinal pitfalls that a focus on taxonomy can bring in ecological endeavors. Over the years I have discovered that while you must have a strong taxonomic foundation for your field research activities, nevertheless, one must be careful not to let that dominate the research program; especially if your objectives are somewhat broader and are concerned more with the tital ecological problems than with some of the specific and unique questions concerning individual species. While still a graduate student working under Park, I became more and more imbued with the idea that an ecologist need not merely serve as a scientist pursuing his individual researches in classical fashion. Rather that because of the very nature of his viewpoint, he could play a much more general or generalist role in an organization. At about that time there had been a number of discussions in the scientific literature, specifically in such journals as "Science", over the need for scientific generalists--individuals who could serve to bridge in different ways several areas of science. The term interdisciplinary was not yet in fashion, probably because there simply were no true interdisciplinary organizations. Within the scientific community, at least as far as certain segments of it were concerned, were individuals who would cross the different disciplinary areas. These were considered to be generalists, and to me it seemed logical that an ecologist was a person by nature of his background, training and viewpoint to fill such a role.

Following completion of my work at Northwestern, I found at that time (which was the early 1950's) that the job market was exceedingly tight, much as it is now. I started teaching at what was then a small college in Chicago. Although I found the teaching stimulating, it did not offer me what was as yet an ill-defined activity in the general area of research. About 1953 Park became involved with the Oak Ridge National Laboratory in a new program which was concerned with the environmental impact of radioactivity due to radioactive wastes. Park was instrumental in helping the unit in the Laboratory--the Health Physics Division--formulate this program, and assisted them in the recruiting of personnel. Park encouraged me to go to Oak Ridge and join the staff of the Health Physics Division as an ecologist.

At that time the tradition for ecologists trained in liberal arts departments was, as it mainly is now, to seek academic pastures rather than other kinds of institutions. Nevertheless, a number of my friends, due to the job difficulties, had taken positions in pharmaceutical houses and other organizations which offered them the opportunity to do biological work and earn a living. Once I got to Oak Ridge, however, I quickly became aware of the fact that the Laboratory offered two major opportunities in the area of my interests which were not available at any university. First it had unlimited field areas with a variety of habitats under close control. Whereas in the past I had thought a 40-acre forest belonging to a university provided an outstanding field research opportunity, here I was given the opportunity to work with 30,000 acres of forest. Secondly, I quickly discovered that the Laboratory's technological support and equipment facilities were without parallel in an academic situation. And shortly thereafter I was encouraged to consider developing a major ecological research program at the Laboratory. All of this took place about 17 years ago in 1955. Much of what has happened since then, of course, is history. We did start to develop a program. More and more we focused the program on ecological approaches. We oriented ourselves toward ecosystems, and particularly ecosystem processes with emphasis on mathematics and chemistry, while at the same time making sure that our directions not only were aimed at providing intellectual leadership in the scientific community, but also providing needed information and work directly in support of the AEC mission.

As the years started to pass by, it became soon evident to me that I had to make a choice between trying to pursue a research career or to pursue a management career. Herein many of the ideas that I thought about as a graduate student prevailed and I felt that I would, in the long run, be happier with trying to become a manager and a builder of a major ecological research program. By 1960, this shift in career plans had pretty well crystalized and I gave up all further thoughts of trying to be both a researcher and an administrator. I had discovered in the previous 6 or 7 years that in order to develop an effective interdisciplinary research team and to keep one that was focused on both a strong program in basic ecology, as well as a mission oriented program, took one's total efforts as a manager and as a motivator of, and a communicator, between individuals.

Having pursued thusly the career of an innovator of ecological programs rather than a more classical researcher, I cannot point to particular students who have trained under me. Even though we have had PhD's taking degrees at the Laboratory, most of these, particularly in the last few years, have done their research under other staff members. I can, however, point out a number of individuals whom I have influenced while they were here at the Laboratory, or even some of those who are still here and who have made some contributions to the science of ecology. Some names come to mind; and I must remark at this point that some of these individuals might not agree with the fact that I had some influence on them. But I do believe that in any kind of working relationship wherein individuals have a chance to communicate frequently and exchange ideas and thoughts, there is an exchange of attitudes and whether people recognize it or not, they do

undergo certain changes in thought patterns and viewpoints. Some of these may be as a result of reactions. Individuals that come to mind that I have played some role in career orientation and development are Manfred Engleman at Michigan State University. David E. Reichle, William A. Thomas, and Daniel J. Nelson of our organization, are individuals whom I think I have had some influence on their careers. There are also individuals who have spent time in our Division in the past, and who I think have both contributed to our thinking and in turn have been affected and learned from it, and their connection with us has played a role in their current careers in science. Among these I would include Professor D. A. Crossley and Professor B. C. Patten of the University of Georgia; and perhaps one who has been outstanding in bringing together the ideas that he received at our place is Dr. George M. Van Dyne of Colorado State University. The ones that I have just named I think are among the more outstanding ecologists who have been in part influenced by me at one time or another. There are a number of others who have gotten degrees here whose attainments have been more modest. They are teaching at a number of smaller schools, and they are making their own contribution to the science of ecology. Whether they will prove to be major contributors remains to be seen.

You ask where do I think ecology is today. It is in a very tough position. Back prior to 1970 and the awareness of Earth Day, ecology could live comfortably in the security of being an obscure science whose practioners tended to operate in their own little networks. They could carry on their research activities; they could comfortably bemoan the fact that they were little known and less heeded and that the world of big science did not include them. While this was considerable grist for individuals bitching about being not recognized for their importance, at the same time you must recognize that there was a kind of security in this position. Now, however, this security blanket has been stripped away. Without belaboring the obvious, ecology is now a household word. We are faced with the challenge of delivering to the public something tangible in what the science of ecology can do to help ameliorate our environmental problems. Or to put it another way, now that the public hears about all kinds of ecology and hears about it in a context of a broad public disapproval of science in general, it asks the question--how are ecologists going to help us with our problems? I still believe that the challenge and the opportunity that we have as ecologists is considerable. I think that we are still one of the few disciplinary groups that by training and experience tend to look at many facets of the scientific problems simultaneously. This is our great strength. It is this viewpoint that enables good ecologists to move into an area and see how many pieces of research can be threaded together to help resolve a problem. Our weakness, however, is still some of our long established traditional hangups of the need for the individual to be paramount in ecological research; and that the contributions that we make must be those done by individuals. Another hangup that we have is that we tend to get overly concerned about the exceptions to the rule; that is to say we worry about the taxonomy of species, we try to look at species' specific problems, we tend to focus heavily on the more narrow

aspects of environmental biology rather than on the broader aspects of holistic environmental studies which incorporate environmental biology. If we do not take the lead in trying to weave these various threads that comprise environmental sciences into some type of unified and meaningful whole, then I doubt that anybody else will, because few other disciplines are trained to do so. Those who are in environmental areas, such as meteorologists, geologists, etc., still tend to think in more narrow physical terms.

During the rest of the decade, ecologists will have opportunities to move in several directions, depending on their basic abilities as well as the general opportunities. I tend to feel that the opportunities in academic institutions are going to plateau. This is because of a broad public resistance to the growth of universities in general. Also this may be related to the fact that many people feel that the primary role of a university is to do research that is related to education and to developing the creative spirit in individuals. The role of the university as a major problem solver, particularly in interdisciplinary areas, is increasingly coming under scrutiny. The reason is that universities are basically communities of scholars, and scholars focus on particular disciplines. To make this focus more effective universities have been organized in departments representing these groups of scholars. It is therefore very difficult for individuals or units which span these departments to function very well. One can reason that interdisciplinary units should be able to function in a university, but practical experience over the last five years has demonstrated that it is much easier said than attained. Where are these interdisciplinary activities to be undertaken? Probably in institutions that are extra-academic, but which will draw on the academic community and will of course provide jobs for people trained, for example, in ecology. This means that the student entering into an ecological career may, or should, have the opportunity to know their career options. There will always be university positions for the students who have demonstrated that they have the unique capabilities that are most appropriate for a university context. At the same time I anticipate that there are going to be a large number of needs for ecologists in the private sector, either in big research laboratories or in various types of engineering and other firms which are concerned with analysis of the environment. And I feel that the student of ecology needs, either through external guidance or through strong self-analysis, to determine early for himself where his talents and motivations best lie. It is a great waste to motivate an individual toward a university career when that individual does not possess the highly individualistic creative abilities that we like to associate with scholars in the academic community. At the same time I think that it is equally unwise to disparage those areas of ecological endeavor which call for strong scientific minds and wherein both intellectual and physical rewards can be as great as in the academic institution.

Why am I saying all of this? Simply because I feel that during the rest of the decade we are going to be in a period of environmental analysis and synthesis on both a national and international scale. The focus is going to be heavily on trying to develop knowledge and information which will enable us to better adjust our technology and our support resources to the general environment.

This means that we will have considerable research on pollutants and their behavior in the biosphere. We will have research on the impacts of specific technologies on specific parts of the different ecosystems. This type of knowledge is going to have to be codified at various levels of organization and generality. It would not be too difficult for me to foresee a kind of ecological extension service similar to the agricultural extension service but on a much smaller scale, wherein all land-use operations are sort-of filtered through to the public via some kind of extension service activity. It is not unreasonable to foresee in the decades ahead that as our resources dwindle and as our food demands increase that we are going to have to plan the use of our environment much more closely and with a much more detailed data input which can be used to predict the consequences of our actions. Another matter that is going to preoccupy us heavily during the rest of the decade, both as citizens and as ecologists, is the matter of the increasing need for energy. This is going to pose all kinds of questions to us as ecologists--not only on the direct impact of generating stations, but the indirect impacts of the use of non-renewable resources, the disposal of their byproducts, and the balancing of their output and their effects on other needs in the total landscape system. I feel, therefore, that there are two broadly parallel trends that are going to be pursued in the training of ecologists and in the practice of their science. The first is a continued emphasis and perhaps increased emphasis on research dealing with specific phenomena, rather with the attempt to develop new broad hypotheses or to further strengthen those theories and principles we now have. The reason I say this is that the work that is already underway on evaluating the impact of technology has demonstrated that the public, and science in general itself, responds to information dealing with specific effects or responses. Secondly, however, we are also in the age wherein we have to do a great deal of synthesis. One merely can not be satisfied with enumerating specific consequences of some type of man's impact. These have to be brought together and codified in some configuration which enables us to make a generalization about the overall consequences. Here to ecology can play one of its traditional roles, namely, that of being an integrative science. Now while the picture I have just painted may seem to be an optimistic one, I do not want to leave the impression that there is going to be a largescale rush into ecological research with much demand for the production of ecologists. Rather, during the next three or four years, much more is going to be demanded of ecologists and much more is going to be thrown in their way of challenges than in the way of rewards. I think this simply represents the way our society, particularly at this point in time, will respond. There is a large group who are not sympathetic to having the brakes put on them by ecologists and other environmentalists. Therefore they will resist providing the support that would in turn tend to increase the number of ecologists who they fear then would further restrict economic growth. So, what we face is an uphill battle to demonstrate that the kind of knowledge and the kind of ways of doing things that we possess are of importance. To get society to both accept and support these is our burden. Signs of this already are apparent, but I feel that if we believe in what we are doing and believe in the need we will eventually gain support, if for no other reason then that circumstances will make it quite evident that there has got to be a continuing redressing of the balance between technology and the environment.

It is difficult to try and advise an undergraduate about what he should do in planning for a possible ecological career. I think before he makes that decision he has got to, either with the help of his advisors or through a personal market analysis, determine what the potentials are likely to be in the ecological field. During the past several years people have gone through undergraduate careers and into graduate school in a rather blind expectation that there will be an academic position waiting for them; or that somehow or other, some good fairy will take care of them. It is only the more astute few who undergo a critical self-evaluation as well as evaluating the fields and then make decisions about their life careers. I have seen numbers of biology majors who have gone into graduate schools and who have been allowed to proceed and have come out discovering that there is no market for their training and abilities. This is a terrible social waste. On the assumption, however, that the student has undergone this critical self-analysis and has determined that in the environmental area there are markets such as there are in engineering and economics, then a lot depends on his own intrinsic abilities. My own prejudices lie toward an undergraduate not focusing narrowly in biological areas but rather broadly in a number of the basic disciplines. I think that he needs core courses in biology and especially in those areas of biology that will enable him to do later work in ecology. At the same time I feel he needs good grounding in mathematics and chemistry. Increasingly it seems to me that if he is interested in being an ecologist, he has got to have at his command the fundamentals of some of physical sciences, since you cannot work within the environment without a sound appreciation of the laws of physics, chemistry and mathematics. Usually by his junior or senior year he should have developed his perception and his inclinations to enable him to start to focus more narrowly on a particular field of interest. If he has oriented strongly toward some area of biology, there is still ample time for him to get the additional training necessary.

I think the same kind of advice generally applies to a new graduate student, although perhaps not as sharply as to an undergraduate. The new graduate student must not only rely on his faculty to drive him, but by this time must have determined what his potential is, and what are the likely market places for these potentials. He has got to force himself into a critical self-analysis and determine his own strengths and weaknesses and build his graduate training program accordingly. This is not an easy thing to do, and I recognize it, as all of us have gone through the same agonies. Perhaps the most difficult thing that a new graduate student faces is how to select a combination of advisors, courses and researches that can best meet his future career needs. I have no easy answer to this. It depends again, as I said, on his own critical self-analysis, his willingness even to make drastic changes in schools and advisors, if necessary, based on his perception of the changing scene. Certainly there is no formula for it, and one could only hope that our educational institutions provide the flexibility of choice and opportunity for individuals as they perceive their own self-needs. I realize the latter is a rather pompous sounding statement, and I don't mean it to be such. I guess what I am trying to say is that ecology is still a relatively small science, numbers-wise. The market place is still rather small, and

so the feedback signals from the market place are fragmentary. Consequently, it is not easy to plan a career.

I can not take any pride for any original contributions to the science of ecology. I frequently regret this, but on the other hand, I also do not apologize for it. Once I saw my career opportunities, I attempted to work in them and take advantage of them in a way that I felt would contribute to the strengthening of ecology, although not in a traditional fashion. I feel, therefore, that what slight contributions I have made have been toward giving ecology an increased recognition and respectability among the other science and engineering groups, which both compete with and yet at the same time depend and need what ecology can provide. If I could name a second contribution that I hope I have made in some small way, it would be that ecology by its nature and practice is not only a point of view, but that it is an interdisciplinary science and it is one which can connect many different approaches and disciplines in the study of landscape systems. I felt that this is a challenge and an opportunity, as I indicated somewhat earlier in my commentary, and I hope that before my professional career comes to an end that I will have been successful in fully demonstrating the effectiveness of an ecological research organization working as an interdisciplinary team. And by effectiveness I mean in terms of their contribution to the needs of society, as well as their contributions to the advancement of certain areas of ecology. I suppose another area wherein I take some small satisfaction in having made a contribution, is to the organizing of professional ecology in this country. Not that we haven't had a professional organization during the past 15 years, but during the last eight or nine years there has been a need to strengthen the professional organization of ecology in order to bring into the national scene the input of ecologists as thinkers, planners, and advisors to decision makers. I would like to think that I have played some small role in providing ecological input into the national government at various levels and at various places.

As I said at the start of this commentary, I was talking in a stream of consciousness fashion. I realize that I have rambled quite a bit, and not being used to dictating this much material, I may have been somewhat incoherent at times. I apologize for this and wish I had the time to go over it and make it a more completely polished document. I do think, however, Ronnie, that I have given you at least the germ of a number of my thoughts on the questions you have asked.

Sincerely,

S. I. Auerbach, Director
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SIA/mr

Enclosures

USE II

(A) STANLEY I. AUERBACH, Director, Ecological Sciences Division, Oak Ridge National Laboratory, Oak Ridge, Tennessee

(B) Born May 21, 1921, Chicago, Illinois

(C) Ph.D., 1949, Northwestern University (Animal Ecology)

(D) Positions

(1) Research Associate, Northwestern University, 1949-1950.

(2) Instructor, Roosevelt University, 1950-1953.

(3) Assistant Professor, Roosevelt University, 1953-54.

(4) Associate Scientist, Oak Ridge National Laboratory, 1954-56.

(5) Scientist, Group Leader, Ecology, Oak Ridge National Laboratory, 1956-59.

(6) AEC Site Committee, Savannah River Plant, 1957.

(7) AEC Advisory Committee, Camp Mercury, Nevada, 1958.

(8) Chairman, Aquatic Biology Subcommittee, Clinch River Study Committee, 1958-62.

(9) Section Chief, Radiation Ecology Section, Oak Ridge National Laboratory, 1959-70.

(10) Director, Ecological Sciences Division, Oak Ridge National Laboratory, 1970-present.

(11) Lecturer in Zoology, University of Tennessee, 1959-present.

(12) Subcommittee on Radioactive Waste Disposal, American Standards Association, 1960-65.

(13) Chairman, Committee on Radioecology, Ecological Society of America, 1960-64.

(14) Elected Fellow of American Association for Advancement of Science, 1960.

(15) Organizing Committee, First and Second National Symposia on Radioecology, 1961, 1967.

(16) AIBS-AEC Advisory Committee in Radiation Botany, 1961.

(17) AIBS Visiting Lecturer in Biology, 1961-63.

(18) Co-organizer, ORAU (ORINS) AEC-NSF Summer Institutes in Radioecology, Oak Ridge, Tennessee, 1962-65.

(19) Member, NAS-NRC, "Project Harbor" Study Task Force on Civil Defense, 1962.

(20) Member, Honorary Editorial Advisory Board, Radiation Botany, 1963-present.

(21) Associate Editor, Ecology, 1964-66.

(22) Secretary, Ecological Society of America, 1964-1970.

(23) Visiting Research Professor of Radiation Ecology, University of Georgia, 1964-present.

(24) Study Committee, Ecological Society of America, 1965-present.

(25) Chairman-Elect, Division of Ecology, American Society of Zoologists, 1966-67.

(26) Public Affairs Committee, Ecological Society of America, 1967-present.

(27) Chairman, Division of Ecology, American Society of Zoologists, 1967-present.

(28) Board of Directors, Tennessee Citizens for Wilderness Planning, 1967-present.

(29) President, Ecological Society of America, 1971-72.

(30) Director, Eastern Deciduous Forest Biome project of the Analysis of Ecosystems program of the IBP, 1968-present.

(31) Member, Executive Committee, U. S. National Committee for the IBP, 1969-present.

(32) Vice Chairman, Executive Committee, U. S. National Committee for the IBP, 1971-present.

(33) Member, U. S. National Committee for the International Union of Biological Sciences, 1971-present.

(34) Member, Committee on Power Plant Siting of the National Academy of Engineering, 1970-present.

(35) Member, Board of Trustees, The Institute of Ecology, 1971-present.

(36) Member, National Academy of Sciences Advisory Committee on Research to the Secretary of Agriculture, 1969-70.

(37) Member, Board of Ecological Advisors of the Bureau of Reclamation, 1971-present.

(38) Member, Special Committee on Biological Water Quality of the Ohio River Valley Water Sanitation Commission, 1971-present.

(E) Publications:

(1) Auerbach, S. I. 1956. Lethal effects of gamma radiation upon segments of a natural microbial population. Jour. Bact. 72: 505-510.

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(3) _____. 1958. Some effects of gamma radiation on Trogoderma Sternale Jayna. Ann. Ent. Soc. America 51: 48-51.

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(5) _____. 1958. The soil ecosystem and radioactive waste disposal to the ground. Ecol. 39: 522-529.

(6) _____, and P. K. Lee. 1959. Determination and evaluation of the radiation field above White Oak Lake bed. ORNL-2755.

(7) _____, and D. A. Crossley, Jr. 1960. A sampling device for soil microarthropods. Acarologia 3: 279-87.

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(14) _____, R. I. Van Hook, Jr., and D. E. Reichle. 1970. Energy and nutrient dynamics of predator and prey arthropod populations in a grassland ecosystem. ORNL-4509 (Thesis).

(15) _____, C. E. Baker, and P. B. Dunaway. 1970. Relationship of cesium-137 and iron-59 elimination rates to metabolic rates of small rodents. ORNL-4568 (Thesis).

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Becking

Rudolf W. Becking
(Humboldt State College)

Biographical Sketch of Dr. Rudolf W. Becking.

Taken in Part from American Men of Science.

Born: Blora, Java, Indonesia, October 10, 1922.
Married: 1952; children 2.
M.F. Landbouwhogeschool, Netherlands, 1952.
Ph.D. University of Washington (Seattle), 1954.
Forest Officer, Dutch Forest Service, 1954-1956.
Assistant Professor, University of New Hampshire, 1956-1957.
Pennsylvania State University, 1957-1958.
Associate Professor, Alabama Polytechnic Institute, 1958-1959.
Division of Natural Resources, Humboldt State College, Arcata, California, 1959-
Mem. Sta. Int. Geobot. Mediterranean and Alpine, France, 1947-
Royal Dutch Indonesian Army, 1942-1945.
Society of Foresters, Society of Photogrammetry, Ecological Society, Dutch Society of Forestry, International Society of Phytosociology International Society of Tropical Ecology. Phytosociology.

Publications of Dr. Rudolf W. Becking.

1954. Site Indicators and Forest Types of the Douglar Fir Region of Western Washington and Oregon. Ph.D. Thesis, University of Washington, 159 pp.

1956. The Forest Associations Sensu Braun-Blanquet of the Douglas Fir Region (Pacific Northwest) and their Relation with the Site Quality of the Douglas Fir. Bull. Ecol. Soc. Amer. 37(4):95.

1957. The Zurich-Montpellier School of Phytosociology. Botanical Review. 23(7):411-488.

1959. Forestry Applications of Aerial Color Photography. Photogrammetric Engineering. 25(4):559-565.

1960. Committee on Photo Interpretation for 1959- Annual Reports of Subcommittees - Subcommittee on Forestry Applications. Photogrammetric Engineering 26(5):792-797.

1961. Commission VII on Photo Interpretation: Working Group on Forestry Applications. Annual Report 1960. Photogrammetric Engineering 27(4):645-647.

Humboldt State College
Arcata, California
Division of Natural Resources.
March 16, 1962.

Mr. Donald W. Davidson
Dept. Botany
Rutgers. The State University
New Brunswick, New Jersey.

Dear Mr. Davidson:

Thank you for your letter of March 10 which I haste to reply in view of the short time period you have for your report.

I am sorry to state that I am not inclined to write my own biography. My personal and professional life is of little importance to the science of vegetation as a whole. For my education etc. I understand that this has been incorporated in Who is Who. I do not know if this is true.

For my views of the problem or possible "controversy" between phytosociology and ecology, I have published extensively in 1957 on this point. I also tried to answer some of the most frequent questions which may arise in your mind in the same paper. Why phytosociology is not more applied in this country is chiefly that an intimate contact between student and teacher has to be established first before you can begin to understand this method. Currently, in the past five years I have never taught a course in phytosociology because according to American educational standards I am not fully qualified to teach a course in ecology. The second great bottleneck for this method is that very few students are familiar with the flora and vegetation as a total, even on a local basis. Practically, none of the ecologists I have encountered the past years, and that are men of great scientific stature have such a botanical knowledge of trees, shrubs, herbs, mosses and lichens. I myself, have to take considerable time and effort before being able to work on phytosociological studies after coming to a new region. My advice to any student who is seriously interested in becoming familiar with phytosociology is to forget all the theories, hypotheses pro or con but start from scratch and learn first the plants, the objects of your studies, see and observe and work hard before jumping to conclusions. Do not use statistics as a criterion or decisive power to judge your results but rather use statistics as a subordinate tool to common sense in a diligent manner. Keep always flexible and change your opinion or scientific point of view whenever other facts support this. Do not believe you ever found the truth or the best or most objective method but rather be humble and be glad that you contribute something to understand nature and its problems. As an ecologist never rely upon one or two sciences alone. Study until you have a good working knowledge of plant taxonomy, ecology, microclimatology, plant geography, soils and forestry before trying to attempt to practice phytosociology. You may object that this hardly can be mastered by anyone. I may suggest that you check in what is done in Western Europe and Russia and you can readily see that the excellent contributions in this field have been made by persons who have such a knowledge in a number of sciences. Good luck with your symposium.

Very sincerely yours, Rudolf W. Beeking

R. W. Beeking

Billings

W. Dwight Billings
(Duke University)

DUKE UNIVERSITY
Durham
North Carolina

Department of Botany

November 9, 1956

Mr. John W. Andresen
Department of Forestry
Rutgers University
New Brunswick, New Jersey

Dear Mr. Andresen:

I hope that the enclosed material will reach you in time. I am sorry to be so slow but interruptions kept me from finishing earlier.

It was a little difficult for me to put down my views on modern ecology in a short space. Perhaps you will be able to gather that if I have any "philosophy" about ecology it is that all organisms are part of the whole system and are interdependent - from my biographical sketch you can probably deduce that I believe this is true of ecologists too. Everyone is influenced and helped by the ideas of others.

With best wishes for a successful seminar and discussion.

Sincerely yours,
(Signed)
W. D. Billings
Associate Professor

WDB:ff

Enclosure

The first seven years of my life were spent in Washington, D. C., where I was born. All of my early recollections are happy ones of taking walks through the Maryland woods with my father and brothers on Sundays, or going to the Zoo in summer or the National Museum and Smithsonian on winter Sunday afternoons.

I started to kindergarten and grade school in Washington but during that year we moved to Indianapolis, Indiana. Both of my parents, William Pence Billings and Mabel Burke Billings were natives of southern Indiana where their families had lived since the early days of the State. The Billings family itself was originally from New England and my middle name, and the one generally used, Dwight, was a New England family name.

In Indianapolis, we lived a normal and happy life with the usual music lessons, baseball, swimming, camping, and summer vacation trips. With my two brothers, I attended the local grade schools and Shortridge High School. Probably our main preoccupation in those days was basketball and we all played - for at least seven months out of the year. During the summer most of the time was spent playing tennis or swimming. When I look back on it, this seems like a strange way to get into field biology but we all liked to hike and were active in the Boy Scouts for two or three years anyway, and so we developed an interest in the woods, lakes, and streams. One of our favorite places was Bacon's Swamp where we spent a great deal of time after school making rafts or ice-skating in winter. Little did I realize then that this was one of the farthest south of the bogs initiated by the Wisconsin ice or that Potzger would find that it was once surrounded by forests of spruce and fir. It would have been a surprise to know that - but I probably would have believed it because my father had shown me fossil shells in the Ohio River cliffs near my Grandfather's house in Madison and I knew that things must have been different in the past.

My attendance at Shortridge High School undoubtedly was the thing that led me into biology as others were before me - Stanley Cain and Rex Daubenmire among others. Two remarkable teachers - Miss Rousseau McClellan and Miss Elizabeth Rawls - had the enthusiasm for teaching that succeeded in attracting many students to their classes. I'm sure that many of these did go on in biology or medicine. One day in my last year at Shortridge, Miss McClellan introduced a guest speaker to our class in advanced zoology class - Stanley Cain. He was then teaching at Butler and he told us of his ecological work in the Great Smoky Mountains. It was the first time I had ever heard of these mountains and I'm sure it was the first time I had ever heard of ecology. This lecture so enthused us that three of us decided to go camping in the Smokies after school was out in June. We went in Dick Van Fleet's old car and slept out on the ground in what is now downtown Gatlinburg. The vegetation was overwhelming in its luxuriance and variety as compared to that of central Indiana. I decided then that this was something I had to know more about and a place I had to see again. Arriving home, I immediately applied for admission to Butler so I could major in botany.- and talked my father into taking my brother and me back down to the Smokies for another look.

So that fall I entered Butler and found that Stanley Cain was away on leave. But I went directly to the head of the department, Dr. Ray C. Friesner, and was one of the fortunate ones to come under his influence. Dr. Firesner, through his enthusiasm, kindness, and drive made the Botany Department at Butler; it was the best department in the College and we not only were taught to high Friesner standards but enjoyed working in the labs and spending the weekends on field trips. Dr. George Fischer (now of Washington State) was my instructor in General Botany and Rex Daubenmire was the laboratory assistant. The next year Stanley Cain came back and I was allowed to be his laboratory assistant although only a sophomore. This is where he really convinced me that I wanted

to be an ecologist. The following summer I was his field assistant in southern Indiana. But before I could take an ecology course from him, he moved to Indiana University. I was lucky in his replacement though because it was John E. Potzger.

During my senior year at Butler I decided to go on to graduate school in plant ecology. I knew very little about where to go or who I wanted to work with. I picked three schools more or less out of a hat, not knowing whether they had ecologists. These were Michigan, Illinois, and Duke. The latter offered me an assistantship which I quickly accepted and I looked in their catalog to see if ecology was a part of the curriculum. It was, and taught by someone named Oosting of whom I had never heard. I pictured him as probably a rather old Germanic type with a beard. I couldn't have been more surprised when I met him in Durham a few months later to become his first graduate student in ecology. This choice of a school and the sheer accident of meeting and working with H. J. Oosting was probably the luckiest (and blindest) choice I ever made professionally. In my opinion, it kept me in ecology and, of course, has influenced my life right up to the present time. It is difficult to measure or even estimate what I have learned from Oosting - not so much in facts or methods but in attitudes and enthusiasm toward my work in ecology.

The first day at Duke I met another person who has influenced me probably more than he realizes. He was also a newly arrived graduate student from Dartmouth, John Reed. His stimulating analyses and arguments have kept me alert from the graduate days bull sessions at Duke to the late-at-night discussions of recent years around a Coleman lantern at 10,000 feet in the Wyoming mountains.

I was at Duke a little over 2 1/2 years and did both my master's and doctor's degrees under Oosting in forest ecology. My interest in physiological ecology and soil relationships of vegetation was greatly influenced by such men as Kramer, Korstian, and Coile but I still kept my interest in plant taxonomy and greatly enjoyed my work with Blomquist.

I received my Ph.D. in June 1936 and Stanley Cain, who was then at Tennessee taking Jack Sharp's place while he was away on leave. Few years have been as enjoyable as that one. Bill Drew, just out of Harvard, was there on a similar appointment, and many weekends were spent in the nearby Smokies working on bryophyte ecology with Bill and Stan Cain. When we weren't sticking thermometers in tree bark up in the Greenbrier wilderness, we would be helping Hesler collect fungi or collecting plants with Jennison. I was very idsappointed when Jack Sharp came back and Tennessee had to be left behind.

There was a chance at a job at the University of Nevada in Reno where P. A. Lehenbauer was looking for someone to teach ecology and taxonomy. I was somewhat surprised to hear that there was a University of Nevada and I certainly knew nothing of the flora or vegetation of the desert. Luckily, I got the job (in those late depression years there weren't many such opportunities) and took off for the West.

My job at Nevada consisted of teaching three new courses each semester and taking care of a rapidly expanding herbarium. This took almost all of my time the first year and there was little time to do more than learn the principal plants of the desert and nearby Sierra.

The second year though, I began to try to learn something of the distribution of the plant communities of the region and their causal climatic and edaphic conditions. There was almost no ecological equipment but little by little this was built up. One of the first purchases was soil moisture cans. One of the freshmen students that year offered to paint the numbers on the cans - he was John Cantlon. He did such a good job and rust is so slow in the desert

air when I left Nevada four years ago we were still using those cans.

John was interested in going out in the field with me, so together we learned the desert flora, vegetation, climate, and soils. The Sierra wasn't neglected either. Probably no region was as fertile and varied and ready for ecological research as the Reno region at that time. And again it was sheer luck to have students of the quality of John Cantlon and his associates.

Before anything could be finished, though, the war came along and we were busy with other things. John became a Navy flyer and I was busy teaching meteorology and navigation for the Air Force.

After the war, we started again and this was a most productive period; good ecology classes and research began to add up. After more than twelve years in the West, I felt that I was beginning to know the country and its ecological problems. I certainly did not want or plan to leave it for any other region. It was home (and after three years in the East, it still is).

Then, in 1952 I received a very attractive offer to return to Duke in ecology as an associate of Oosting. Almost simultaneously the Army asked me to direct a study of cold desert microenvironments for them in thewestern United States. Although I was then Chairman of the Biology Department at Nevada, I accepted both propositions. So my first year and a half with Duke were spent in the desert with two very capable assistants, Marshall Humphreys and John Darling, and hundreds of instruments, studying the relationships between vegetation types and desert microclimates. The field work and report were finished on schedule and I began my teaching on the Duke campus.

Here, my first graduate student was Larry Bliss who had worked in Alaska with John Cantlon on the Boston University Air Force project. We set up his Ph.D. problem as a comparison of artic and alpine microenvironments and their effects on the growth rates of native plants. The arctic phase of the work was carried out at Umiat, Alaska, and we picked the Medicine Bow Range in Wyoming as the site of the alpine work since I would be teaching at the Wyoming Science Camp in that summer of 1954. We spent that summer and the following in Wyoming and Bliss finished his Ph.D. degree in June 1956 - my first student to finish. In addition, we intensified work on some research on alpine snowbank areas on which I had made preliminary observations in the Sierra. During this last summer we shifted our attention to starting work on the comparative ecology of Rocky Mountain species of Oxytropis in the area.

The last five years have been somewhat complicated by the botanical editing of ECOLOGY which I assumed from Don Lawrence in the fall of 1951. It has been difficult to handle this and still carry on active field research. Somehow it was done and now that the chore is almost over I can look back on it as an educational experience. An editor learns a lot about his science and about people. But I wouldn't take it for another five years - it would be impossible to finish the field work in other parts of the world which I still hope to do. My principal work in the future will still be in alpine and desert areas. However, I have become interested in recent years in the vegetation of Hawaii and its relationships to tradewind rainfall. Although, I have spent only parts of two summers in Hawaii trying to learn something of its Malaysian flora, I would like to do some real ecological work in the Pacific some day.

Some ideas on modern plant ecology

Others more fluent that I have commented in recent years on the current status of plant ecology. For some reason, ecologists have always spent a great deal of time and energy arguing pro and con the various "philosophies" or "schools" of the science. Some of this has done great good in realigning the course of plant ecology and establishing principles. But much of this discussion has been based on pure speculation and dogma without the well-controlled experimental work upon which theory should be based.

Ecology now is in a state of rapid change. The past with its emphasis on community description and floristics is a matter of record - its main contribution, in my opinion, has been the principle of dynamic succession, both autogenic and through climatic shift. Now the emphasis should be on the working of the community. We need to know more about the energy flow and efficiency in ecosystems. Tansley's introduction of the concept and its development by Lindemann, Clarke, the Odums,and others is the really big advance of the last 20 years. Even now its importance isn't realized fully by ecologists, especially those working on terrestrial communities. But the aquatic ecologists have made great gains in recent years in knowledge of productivity of the ecosystems of natural waters. The selection of the Odum's work on the Eniwetok reef for the Mercer award this year is significant.

The complexity of even relatively simple ecosystems is tremendous. In the past ecologists have been too prone to assign simple one-factor causes for many broad ecological changes. We now realize that in many cases there is no simple answer.

It is rather amazing that ecologists have been so slow in realizing the importance of the work of Turesson and Clausen, Keck, and Hiesey, and others on the nature of species. The physiological complexity of morphological species invalidates much of the indicator concept based on recognition of morphological types. The study of comparative physiological ecology of related ecotypes based upon a solid experimental basis in field and laboratory will do much to correct this and to lay the foundations for a better understanding of species relationships and functions in different ecosystems.

The main advantage of the ecologist is his ability to see the whole environment - community picture. He cannot afford to be a specialist. He should be an ecologist - not a physiological ecologist, radiation ecologist, forest ecologist - and no more. Perhaps the idea of a complete ecologist is an ideal not possible of perfect realization. But without this ideal the greatest function of the ecologist is lost, that of synthesis of scattered fragments of biological knowledge into an ecological understanding of a working community.

The backgrounds of most present and past ecologists have been too narrow. The future ecologist should have not only an excellent background in biological science (with more emphasis on genetics, physiology, and the new taxonomy), but also in mathematics, physical chemistry, nuclear physics, geology, and pedology. But overall he must have the broad ecological viewpoint of understanding organisms in their natural environments and avoid becoming a narrow narrow specialist. The specialist will always provide much of the basic information concerning different parts of the ecosystem. But it will continue to be the role of the ecologist to put these data together and to attempt to understand their relationships in the whole functioning ecosystem.

Botanical Publications
W. D. Billings

1936. A bud and twig key to the southeastern arborescent oaks. Jour. Forestry 34:475-476.

1937. Winter key to the trees of eastern Tennessee. Jour. Sou. Appalachian Bot. Club 2:29-44. (With S. A. Cain and W. B. Drew).

1938. The structure and development of old field shortleaf pine stands and certain associated physical properties of the soil. Ecological Monog.8:437-499.

1938. Bark factors affecting the distribution of corticolons bryophyte communities. Amer. Midl. Nat. 20:302-330. (With W. B. Drew).

1939. Edapho-vegetational relations in Ravenel's Woods, a virgin hemlock forest near Highlands, North Carolina. Amer. Midl. Nat. 22:330-350. (With H. J. Oosting).

1941. Grasses and clovers for Nevada farm and range. Univ. Nevada Agr. Ext. Bull. 89, 68 pp.

1941. Quantitative correlations between vegetational changes and soil development. Ecology 22:448-456.

1942. Factors affecting vegetational zonation on coastal dunes. Ecology 23:131-142. (With H. J. Oosting).

1943. The red fir forest of the Sierra Nevada. Ecological Monog. 13:259-274. (With H. H. Oosting).

1945. Nevada trees. Univ. Nevada Agr. Ext. Bull. 94, 102 pp.

1945. The plant associations of the Carson Desert region, western Nevada. Butler Univ. Bot. Stud. 7:89-123.

1949. The shadscale vegetation zone of western Nevada and eastern California in relation to climate and soils. Amer. Midl. Nat. 42:87-109.

1950. Vegetation and plant growth as affected by chemically altered rocks in the western Great Basin. Ecology 31:62-74.

1951. A comparison of virgin spruce-fir forest in the northern and southern Appalachian system. Ecology 32:84-103. (With H. J. Oosting).

1951. Reflection of visible and infrared radiation from leaves of different ecological groups. Amer. Jour. Bot. 38:327-331. (With R. J. Morris).

1951. Vegetational zonation in the Great Basin of western North America. C. R. du Colloque sur les bases ecologiques de la regeneration de la vegetation des zones orides. U.I.S.B., Paris, pp. 101-122.

1952. The environmental complex in relation to plant growth and distribution. Quart. Rev. Biol. 27:251-265.

1954. Nevada trees. 2nd ed. Bull. 94, Univ. Nev. Agr. Ext., 125 pp.

1954. Temperature inversions in the pinyon-juniper zone of a Nevada mountain range. Butler Univ. Bot. Stud. 11:112-118.

1954. Environmental studies in the cold deserts and semideserts of the western Great Basin of North America. 112 pp. plus 80 tables and 125 figures. Rept. Contract DA 44-109-qm-1261, Quartermaster Research and Development Command, U. S. Army. (With M. H. K. Humphreys and J. B. Darling).

1957. Physiological ecology. Ann. Rev. Plant Physiol. 8: (in press).

1957. Factors involved in the persistence of montane balds. Ecology 38(1): (in press). (With A. F. Mark).

Bliss

Lawrence C. Bliss
(University of Washington)

[illegible], 1961

Dr. Ralph E. Good
Department of Botany
Rutgers, The State University
New Brunswick, New Jersey

Dear Ralph:

Upon reading your letter requesting an autobiography relating to my academic life, I could not help but think how rapidly time flies. It was only a few years ago while I was a graduate student at Duke University that Dr. Oosting responded to a similar request. Three years ago I wrote a letter for a seminar on Dr. Billings who was out of the country at the time. I must say that I feel the kind of experience you folks get from reviewing the lives of contemporary ecologists is a valuable one. That is, valuable until now. Seriously, I do appreciate being asked to participate in a direct-indirect way in your seminar. This letter introduction is made much easier with the presentation by you.

One hardly knows where to begin, but I guess the logical place is with my reasons for going to college and for choosing the biological sciences. I was raised on a farm in northeastern Ohio near Akron where I learned to love the out-of-doors. From the time I was a small boy, I wanted to be a medical doctor and following graduation from a township high school (Rootstown) in 1947, I enrolled at Kent State University, Kent, Ohio in the premedical curriculum. In my sophomore year I was required to take general botany and at the end of the quarter, I changed my major to botany with minors in zoology and geography-geology. As an undergraduate I was influenced most by the 2 botanists (about par for a biology department of 12) and the Shelford trained ecologist. It was not until my senior year that I decided upon plant ecology for graduate study. I chose to stay at Kent State where I worked under Dr. Dexter, the ecologist. My Master's thesis was a study of forest development in a previously grazed stand of beech-maple.

Upon completion of my M.A. degree in 1953and having married my good wife Gwyneth who also graduated from Kent, I wanted to go to Duke University to work under Dr. Oosting. He and the department had offered me a research fellowship in tobacco under Drs. Wolf and Kramer in addition to an exciting opportunity to spend the summer of 1953 in northern Alaska working on a study of microenvironments and plant growth. This arctic experience was one of the finest that I have had, though at the time it seemed hellish to be off in the corner of the hemisphere doing field work in such a new and strange area. The research work was conducted by Boston University for the Air Force. The ecology group was headed by Dr. Mohn E. Cantlon and Dr. F. H. Bormann. It was agreed that I could use the data collected for my dissertation. Thanks largely to the guidance of Dr. Cantlon, the summer was a success.

Upon arrival at Duke in the fall of 1953, I switched to working under Dr. Billings for he also arrived that year to share the teaching and graduate student load in ecology. While Dr. Billings was my major professor, I feel that I really am the product of both men because of my course work and their guidance of my research. I am proud of the fact that I was the first Ph.D. of Dr. Billings as he was the first one of Dr. Oostings.

I had wanted to return to the Arctic the following summer, but with major cuts in defense research programs this seemed out of the question, so with the help of Dr. Billings it was decided that a comparative study of microenvironments and plant development in northern Alaska and the Central Rocky Mountains would be valuable. Dr. Billings taught at the University of Wyoming Summer Science Camp in the Snowy Range of the Medicine Bow Mountains while I worked in the alpine above Science Camp. My good wife helped me with the alpine field work as well as providing much of the financial support for she taught while in Durham. We spent 3 fine summers in Wyoming, the first two on thesis research and the third on a geecological study of Oxytropis.

Upon graduation from Duke in 1956, we accepted a position at Bowling Green State University, Bowling Green, Ohio, where I taught for one year. In the fall of 1957 we moved to the University of Illinois with the retirement of Dr. Vestal.

My major interest in alpine research has continued to the present via work on Mt. Washington, New Hampshire. In 1959, I finished a three year study of microenvironments and plant productivity. This work will continue for the next two years with the major emphasis on field measurements of photosynthesis and respiration; work which will be used by Elmer Nadley for his dissertation.

Regarding a philosophy of ecology and biology in general, I think it can be summarized by stating I feel that there are many areas in ecology as well as biology worthy of intensive work and all should be pursued at the expense of riding on band wagons. I do not mean that any one person can pursue many aspects of a discipline at one time, but one should at least keep an open mind with regard to the value and necessity of concerted work in many areas. While science tends to have major bursts forward such as at the present time in cell biology, this would not be possible without a tremendous background of information, nor will all the problems be solved at this level. I would hope that in ecology people will continue to delve into the many and varied areas that have been investigated to this time, ie. community studies, soil and other environmental factors, geecology, autoecology, growth and productivity to mention a few.

As an undergraduate, and through my M.S. degree, I was well stuped in the Clements-Shelford approach. This was highly modified at Duke with the realization of the important contributions of men like Gleason, Mason, and Clausen. Certainly, an understanding of plant communities from a descriptive view point is still of prime importance. It is only after an understanding of communities and their interrelationships that we are able to ask more specific questions regarding how and why these species are grouped the way they are. My own work has centered upon microenvironmental factor interaction upon plant growth and development. The latter has led me in recent years more in the direction of productivity, energy flow in the producer level of the ecosystem and now into field measurements of photosynthesis and respiration as well as mineral cycling in the decomposition of forest leaves.

One frequently has long range plans with regard to hoped for future work. I guess I would have to admit mine centers around tundra studies, more specifically, continued comparative studies of productivity of tundra communities in relation to similar or quite different tundra environments. This work will include harvesting of roots and shoots, caloric values, chlorophyll and relative turgidity studies as well as measurement of environmental factors. Though it would appear that I tend to live on high peaks and barren waste this is not the entire picture for I find interesting problems here in Illinois in our few forests in spite of the multitude of corn and soybean fields.

Regarding an academic life, I feel that a balance between teaching and research is highly desirable. I must say I love both aspects equally well though would not want an entire dose of either. I feel that teaching is and should be an important part of our University responsibility. Personally, I have enjoyed the 3 years work in our general botany course. This area of work is greatly reduced now from the lecturing standpoint. I still, however, have a part in the laboratory end of the course - a rather major part what with the near completion of a completely revised edition of our general botany laboratory manual.

One last point and I'll close. I feel that field work is an important part of any ecology course. As an outgrowth of this philosophy and the enthusiasm of our graduate students we now have an annual spring vacation field trip to some portion of the continent. I say continent, for this year we plan to go to Texas and Mexico. I feel that the students' first hand contact with principal vegetation types and their flora is an important part of their education. We also try to see at least one major ecological center on our trip so the students can see staff and students on other campuses.

Sincerely,

Signed

Lawrence C. Bliss
Associate Professor of Botany

Publications

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1956. A comparison of plant development and microenvironments of arctic and alpine tundras. Ecol. Monog. 26: 303-337.

1957. Drought resistance in tobacco. Tobacco Science 1: 120-123 (with F. A. Wolf and P. J. Kramer).

1957. Succession on river alluvium in northern Alaska. Amer. Midl. Nat. 58: 452-469 (with J. E. Cantlon).

1958. Seed germination in arctic and alpine species. Arctic 11: 180-188.

1959. An alpine snowbank environment and its effects on vegetation, plant development, and productivity. Ecology 40: 388-397 (with W. D. Billings).

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1961. Net primary production of tundra ecosystems. Chapter in "Die Stoffproduktion der Pflanzendecke" Stuttgart, Germany.

1962. Adaptations of arctic and alpine plants to environmental conditions. Arctic (in press).

1962. Laboratory manual for general botany. 2nd Ed. Holt, Reinhart and Winston Co. New York. (with W.N. Stewart and H.J. Fuller).

Dr. L. C. Bliss

Bormann TRANSCRIBED from a tape by Dr. F. H. Bormann:
F. Herbert Bormann
(Yale University)

- Frank Day 1972

Answering your letter .... stacked up, and I work from day to day trying to get the things to say as they come. I have chosen this method to answer you by tape, rather than writing, in the hopes that it will be somewhat easier, possibly even clearer. I should hasten to add that I don't think that I have ever dictated a decent letter in my life, so what comes here may not be all that red-hot.

First of all, let me say that I am somewhat flattered by your letter, not really regarding myself as one of the eminent ecologists, and perhaps your asking me is evidence of the poor state of the field. But be that as it may, your first question asked me how did I become interested in the field of ecology. Actually, I think that is a very long story and perhaps one that even I do not well understand. I can say that as a youth I was very much interested in nature and spent a great deal of time in New Jersey, where I grew up, in the woods and in the fields. Since I had a very strong aesthetic appreciation for nature and as an initial entrance into the general study of nature. But in addition to that, I spent some time largely on my own, because there was not any decent instruction in the schools, just learning something about taxonomy and plants. I just liked the idea of knowing what they were, where they grew, and things like that.

Now, coupled with that, I guess, was a very strong interest in politics, political science, and history. I came from a very political family, somewhat radical, I guess, and we spent many long hours discussing and arguing about politics and history. You know that there are many aspects to history that are very similar to the field of ecology (of course I didn't realize it then), in that I felt that to understand history, one had to understand the simultaneous occurance of a wide array of events and how these were playing one on the other. I think for some reason that I have always kind of thought in "wholes," rather than in pieces.

Well, when I went off to college, and I started at the University of Idaho, I had the very good fortune there to take a course under Rex(ford) Daubenmire in General Botany, and I found it very intriguing, and of course with Daubenmire teaching it, it was basically an ecology course. The war broke out at that time and I joined the Navy, where I spent about two and a half years. Toward the end of that period, I got into Officers Training School and went to Princeton, and there, because I had a previous semester of college, I had a chance to major in the (??) ~~detailed~~ (??) program, most of the persons in it didn't have the opportunity, and I majored in political science, and had some excellent teachers. But I found that to my dismay that I really didn't .... there was something about political science that troubled me, actually, that there were so many pieces and so much data and there was no theoretical thread that seemed to hold things together and everything was very empirical, and it didn't appeal, I guess, as much as it had formerly to my mind which I must admit has a somewhat orderly bend to it. [TYPIST'S NOTE: Reading that paragraph, there is some doubt about this last statement]

After the war, I went to Rutgers, where I studied in the School of Agriculture, mainly because it was the only place I could afford, and there I was studying plant pathology, which I kind of enjoyed, but not that much. Toward the end of my period at Rutgers, I had the good fortune of taking Professor Buell's course in Ecology; I believe it was the first year that he taught it there at Rutgers, having recently arrived from North Carolina State. I must say that that course, a general course in Ecology, gave me a very great deal of pleasure, because, among other things, it allowed me to pull together a great many things I had been learning in Botany, Physiology, [and] Soils, Pathology, and pull them together into a working unit, so that one could get [?] the stresses on interactions and analysis of situations, and of course all this is hung on the two great principles of ecology, succession and climax, and I

found this whole idea extremely appealing and it was at that point that I seriously became interested in ecology, and from that point forward I went into it professionally. I went to Duke, where I studied with [?] Oosting for four years, and I also had an opportunity during that period to go to the Minnesota Biological Station at Lake Otaska [?] with Professor Buell and the most marvelous trips with him across the counrty. We stopped all along the way and visited ecologists like Paul Sears, Charles Ohmstead, and Bill Lawrence, and all of these things -- visits -- gave me an opportunity to reinforce my interest in ecology, but also to get to see from an ecologists' point of view many areas of this vast and beautiful country of ours, and I find that this, almost as much as anything, the experience of seeing these great swatches of nature, and getting some idea on how they fitted together and how they worked, to me was as exciting as anything; as a matter of fact, it was sort of that kind of goal that I had in history and political science and still have, and yet I had never really been able to get that degree of a grasp on the situation.

It might be kind of fun to tell you a little bit about my evolution in ecology after Duke. I went to Emory University, where I taught for four years and had the good fortune to be a colleague of Robert Platt, who I enjoy and admire, and to come into contact with Eugene Odum, and the group at the University of Georgia at that time. My research interests at Emory were largely, I guess, between sort of physiological ecology, and I was particularly interested in the ecology of pine. I had done a doctoral dissertation at Duke on old field succession and the role of loblolly pine and sweetgum, and I continued some of that work, looking at the changing photosynthetic response of pine with ontogeny and questions of that sort. The main point was, I was somewhat experimentally oriented; I wanted to use material I could grow and control and put in growth chambers or in exchange systems and study their responses and try to relate that to what I saw in the field.

Well, after four years I took a new job at Dartmouth College, and at the same time I developed a research interest that I had come across at the Biological Station at Minnesota, where drinking beer one evening with Clark Christensen, plant pathologist, and he told me about some of the [2] root grafts between individuals, and that idea sort of stuck in my mind and I began to realize that the intonations of that potentially were very great because of the fact that much of our thinking in the ecology of plants deals with the concept of the plant as an individual, yet here was evidence of plants not being individuals, but literally being linked together, and that started the course of about eight years of research in which I published I guess maybe eight or ten papers on various aspects of ~~wood~~ root grafting, culminating with an ecological monograph about 1960 or so. All along in that period, my contacts in ecology were broadening and I became one of the advisors in the Brookhaven program, along with several others, who established the idea of a radiation field. In time, George Woodwell at the University of Maine subsequently came to Brookhaven, so Woodwell and I came to be very fast friends. We spent a great deal of time talking and thinking and all. As a result of that experience, my whole view of ecology had largened, moving away from the notion of physiological ecology, which is, although important, is very limited in scope, and I was getting more interested in the idea of looking at larger systems, and of course the Brookhaven radiation course which I had a small hand in helping George Woodwell plan, gave me some notion of dealing with systems.

At that point, in about 1960 or so, a thought came to me that perhaps is maybe my best contribution to ecology. While I was at Emory University, I regularly made field trips to Coweeta, I thought this was an extraordinarily fine instructional area; a place where one could deal with ecology and its operation, and in quantitative terms, particularly hydrologic terms. It so happens that about 1960, I had a joint student with John Tidrow [2] from Rutgers, and I guess Murray Buell was on this man's committee as well; a very

.... a fellow from Germany, and I must say, to my way of thinking, a rather pig-headed fellow, because I am somewhat that way myself--you put two pig-heads together, and you've got problems. At any rate, he was studying Alpine vegetation atop Mount Washington, and he had a tendency to think that anything published in European literature was automatically good. He was very much enthralled with one of the ideas of Dahl, the Norwegian ecologist, on flushing, the notion of water passing through the soil at one point would remove nutrients and when that water would appear it would have enriched and fertilized another area. I didn't think that this was particularly so, and this fellow and I would argue. One day we were climbing this mountain and carrying on this argument and he said "Now, if you measure the chemistry of that stream, you would know I am right." Well, that little phrase stuck in my mind, and sometime later, I don't know exactly when it was, that notion coupled itself with the many years of experience I had had in visiting and talking about Coweeta, and it suddenly occurred to me that the small watershed would be a really ideal situation in which you could measure nutrient budgets, because it had definite boundaries, and if it had an impermeable underlayment, all the liquid water that would leave it would leave the stream and it would be possible to measure the input as well. And so, in a way, then there was this burst of the small watershed technique as a means of measuring ecosystem function. Now that whole idea would have lain dormant and nothing would have happened, except for a very fortunate circumstance. At Dartmouth, we hired a young graduate student, as a matter of fact, he came before he got his degree. His name, as you know, was Gene Likens, or is Gene Likens [*]. Gene and I got along splendidly well, we exchanged ideas about limnology and plant ecology, as I used to think of it then, and I brought up this notion of this watershed idea, and he saw its potential immediately, but more than that, Gene had this marvelous knack of

knowing how to quantify things, and he had this real feeling for technical problems--how to get into them, how to solve them, how to organize the data, and as a matter of fact, a great many things like that. In addition to that, he brought to the whole project a whole new array of perceptions and views, and together, then we put together the ~~Haverbrook~~ Hubbard Brook [?] ecosystem study, based on this model that we jointly put together, and argued through and corrected and refined for our purposes. The major point about ~~Haverbrook~~ Hubbard Brook, which, now, including the Forest Service Publications, has something like 187 publications to it; of those, I guess maybe 60 are major contributions that have come through Gene and my program.

As a result of that, my whole concept of ecology changed, and not only did I no longer see this plant ecology, I began to see the field of ecology no longer as a branch of biology, itself, I saw it as an ecosystem ecology, I guess I should call it, as a branch of science, leaning heavily on certain aspects of biology, but as well involved with geology, soils, meterology, and a whole host of other factors. It's a level of organization above biology. The recognition of ecological problems, or pollution problems, I should say and all the other array of ecological problems, my understanding of ecology has gone even a step beyond that; and I now consider human manipulation, as a matter of fact, social an d political processes, as all being intimately involved in the structure and function of ecosystems, so in a certain way, then, I through the field of ecology I have come to realize some of the earliest interests I had in political science and history, and I now find this kind of analysis how even decision-making affects ecosystems and how they work for man. I find this whole area extremely interesting; so in a certain sense, I have come full circle. But I should add one last thing--it is my feeling that the field of ecosystem ecology cannot really prosper to the fullest degree in a department of Biology. Somehow or other, other biologists do not understand

the scope of this field and they tend to diminish it, they want to think of ecology, or ecosystem ecology, as just another aspect of biology, equivalent to cytology or physiology, or something like that and the great tendency is not to let it grow. It is for this very reason, in 1966, that I left Dartmouth College. I felt after several years of struggle there that the time was right for the field of ecology to branch out--to start having meaningful communications with other areas, and to start investigating some of our ecosystem problems from a much broader and wholer point of view than was possible inside biology departments. I could never realize that, and it is my general feeling, looking around the country, that other ecologists in biology departments wishing to note a genuine multidisciplinary kind of ecosystem study cannot do it because of the boundaries established by the field of biology which ecosystem ecology, by its very nature, transcends. So, in some senses, I think in a way we need the development of new departments of ecosystem science--departments that are multidisciplinary, and in a certain sense by a historical quirk of fate, that is something we almost have in the School of Forestry at Yale, where we have a 20-member school of people from all disciplines, ranging from basic sciences through sociology and economics to management, and in a way, we have a certain latitude to approach ecosystem problams in a more coherent way, although I must say that we really don't realize that full potential.

I guess in a way that sitting here talking I have kind of partially been answering your questions 2 and 3 : What do I consider my major contributions to the field of ecology, and what are some of your philosophies with respect to ecology in the future. I am not quite sure how to deal with the question about ~~rules in~~ views on the future of ecology. I suppose one could speak about that in terms of basic research, and my own feeling out of our work at [2] Hubbard Brook is that we are just beginning to see some of the extremely beautiful relationships or interactions that go on to govern and maintain

ecosystems through time and ..., but you know I don't really want to dwell on that, I think that's probably the diet that students get barely through their seminars and all that, rather I would like to speak a little bit about what I consider to be an extremely important contribution or potential contribution of ecology to society as a whole. I think in a way as a western culture in particular has come through a period in which reductionism, resulting largely from the methodologies and thinking in the physical sciences has reached an absolute peak of development where it seeps into all of our thinking, not only in the sciences, but everywhere else. We are forever attempting through these questions to the barest minimum to the essential truths. But I think what the field of ecology tells us is that there are no barest essentials; that things really work as systems, and it is only by really understanding systems that we can ever hope to make wiser decisions regarding the regulation of the surface of the earth, and, for that matter, the whole social system and how to improve it. And so I would say one of the really important things I would like to see is a kind of revolution in education, particularly at the primary school level, and maybe in the secondary school level. I would like to see education at these levels fashioned in such a way that man in his relation to nature became the whole central theme of education, and that all the separate disciplines were somehow like a constellation of stars surrounding this central theme. The notion being that insofar as possible and as quickly as possible we start to teach children how to think wholly, how to understand interactions -- the fact that an import in one level in their lives will have ramifications in the whole array of other levels. I think that specialization could come later, maybe in the high schools or or even in the universities, but I think it is too late to attempt to

introduce ~~wholeistic~~ holistic thought, after people have been trained in reductionism. It just doesn't seem to work. Yet what the nation and, in fact, the world, is crying for today are people with broader views, more sensitive to the complexities of life and in a certain sense I think the field of ecology could make great contributions to a revitalization of the educational system at the lower levels. And I think that the byproduct of this would perhaps be as great as anything that we might do.

Secondly, in a more practical way, I think that the professional field of ecology has a great deal to say in certain areas of planning. It doesn't have ... what it has to say is somewhat limited by what the field is and certainly others can make contributions in this area as well. But one project I would like to see fulfilled someday and I hope soon is the planning of a totally new city--a city that initial plan based on ecological principles--principles that would allow us to lay out the city with regard to hydrology, productivity, aesthetics, nutrient cycle, and sewage waste disposal, and all those sorts of things ... see this city coupled ... the planning of this city to be coupled in such a way to build into it the things we know about socializing and creating a humane environment. I would like to see this city be structured on the principles of energy conservation insofar as possible, and I think that ecologists could make major contributions all along the line. Now you might ask, well, this is being done, [?] is doing it and others are doing it. Well, I don't believe that's so. I think what's being done is being done in a much narrower field with many more [?] and with little regard to the ultimate increasing growth of [?] useage and things like that. I'm thinking of this city to be a model that could be ~~coupled~~ copied, based on the principles of [?] conservations and durability. I think nothing much will happen to change the landscape of this and other countries until such a model of that kind comes into existence, where

people can actually look at it and see how it functions, read the record books as to its performance, and I believe that it could have an impact on the reorganization of society and tend to [?] the spread of ecological ethics, which I think .....?

Braun

E. Lucy Braun
(University of Cincinnati)

Letter from E. Lucy Braun

Early in the spring of 1904, I attended a lecture on Isle Royale given by C. C. Adams. Then I was only a sophomore in high school; observation of plants and animals in the woods and fields had been encouraged from early childhood by my parents. Adams had told the story of Isle Royale in a new and interesting manner -- of course I did not then know it was ecological.

Botany in college was interesting, but not exciting, and I had no great desire to pursue the lines of study presented. Geology under N. M. Fenneman was equally interesting, some of it, particularly physiography, even more so. Dr. Fenneman was then working on his manuscript of "The Physiography of Eastern United States". As one of his first group of graduate students in a seminar on this topic, I came upon Harshberger's "Ecological study of the flora of mountainous North Carolina" in my search of the literature (for every section studied we were to look up vegetational as well as geological features). It rang a bell; here was something really interesting; the Isle Royale lecture came back to my mind. Suddenly I realized that ECOLOGY was the subject I was really interested in. I read paper after paper, among them Cowles' "Physiographic ecology of Chicago and vicinity." At once I began a similar outline for the Cincinnati region, and that summer (1912) went to the University of Chicago to study under Cowles, whose dynamic viewpoint has made a lasting impression. He encouraged my study of the Cincinnati area, and heartily endorsed my idea of "pre-erosion climax" in contrast to "erosion climax" -- the former a physiographic climax, the latter the climatic or regional climax (see p. 210 of 1916 paper). In this paper, the term "mixed mesophytic" was used for the first time.

I was fortunate in being located in an area where the dynamic aspects of vegetation were prominent, where succession is an obvious process. Its reality became firmly fixed in my mind, and I deplore not only the present lack of emphasis on this dynamic phase of ecology, but worse, the failure to recognize it which is evident in many recent publications. I was fortunate in being near to areas where the influence of underlying rock could be studied and evaluated (Mineral springs region of Adams County, Ohio, 1928). I was fortunate, also, in being located where the influence of past events was pronounced, where changes of environment must be considered which are not due to topographic change nor to reactions of occupying vegetation. Within reach of a day's field trip are the relatively young areas within the limits of Wisconsin glaciation, the strongly dissected and also the flat areas of Illinoian glaciation, and unglaciated southern Ohio. Migration and cliseres began to assume real importance in my mind. Gleason's historic paper of 1923 had much to do with my paper on glacial and postglacial migrations (1928).

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Throughout the first two decades of my ecological study, my concept of a plant association remained more or less unchanged. It was essentially that presented by Nichols (1923). A well-marked forest community assignable to an early successional stage and a forest community of climax status seemed to be about equally important -- both were "associations". In many instances, the former occupied greater area and were prominent and frequent features of the vegetation. The recognition of concrete and abstract concepts I believe necessitates the recognition of the individualistic concept at the same time that it recognizes similarities and permits classification.

Gradually, however, my geographic area of observation expanded. More and more time was spent in the geologically and physiographically old unglaciated Appalachian Upland. Something was wrong with previous concepts. They were the result of local intensive studies; extensive observation necessitated a change of viewpoint. Field contact with Dr. Clements, first in 1928, again in 1931, made some of his concepts more realistic. His concept of the association -- not the association as an organism, but the association as a regional climax -- appeared sound. Observation over extensive areas emphasized the tremendous amount of difference between a developmental community (however prominent it may be locally) and a climax community, recurring (with individual variations) over and over again in a great geographic area. One term could not be applicable to both. Restriction of "association" to the climax unit seemed logical, and adoption of "associes" to designate a developmental stage seemed mandatory. This does not mean that consocies, facies, or locies need be recognized. But it does mean that recognition of developmental communities and recognition of evidence of change, of succession, is absolutely essential. It also means that one must distinguish between regional climax communities and physiographic climax communities. The former are relatively few (if the individual variants are grouped), the latter almost innumerable.

Studies in the Cumberland Mountains where the mixed mesophytic forest association is best developed and displays considerable variation resulted in the presentation of the idea of the "association-segregate" (1935). This is a genetical approach, and I believe quite different from Clements' division into faciations and lociations. The association-segregate concept has been applied by Richards in the complex tropical rain-forest. This concept emphasized origin and relationship and lead to a desire for still more extended field work which would go into all parts of the deciduous forest, which would permit examination of forest development and of climax communities of the whole deciduous forest formation.

Meanwhile, vegetational studies necessitated taxonomic studies. Interesting and unexpected plants were found frequently. Communities, colonies of species, and individual disjunct species began to assume patterns of distribution -- patterns which suggested past events, past climates and past physiographic history (1937). Could this be more than a coincidence? Facts and possible explanations were discussed with Dr. Fenneman who encouraged the historic interpretation. The remarkable correlations between distribution and physiography suggested a cause and effect relationship. It seemed possible to explain many features of present forest (and species) distribution on a physiographic basis. "Development of the deciduous forest of eastern North America" (1947), later elaborated as " A chronologic account of forest development correlated with physiographic history" in "Deciduous Forests of Eastern North America" (1950) resulted from this trend of thought.

The cumulative results of early and later field studies and the mass of observations accumulated in some 65,000 miles of travel, together with the many facts derived from the diverse and widely scattered literature of ecology have been brought together in my book, "Deciduous Forests of Eastern North America".

The dynamic aspects of vegetation, involving the process of succession -- biotic, topographic, and climatic -- and the causes thereof, and involving the explanation of present-day features of distribution appear to me as the most vital of all phases of Ecology.

E. Lucy Braun

LIST OF PUBLICATIONS

E. Lucy Braun

The physiographic ecology of the Cincinnati region. Ohio Biol. Survey, Bull. 7, 1916. 97 pp.

A northern occurrence of Dentaria multifida Muhl. Jour. Cin. Soc. Nat. Hist. 22: 16-17, 1916.

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Notes on Kentucky plants I. castanea 1: 41-45, 1936.

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New plants from Kentucky. Rhodora 42: 47-51, 1940.

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An ecological transect of Black Mountain, Kentucky. Ecol. Monog. 10: 193-241, 1940.
Notes on Kentucky plants III. Castanea 6: 10-12, 1941.
Notes on Kentucky plants IV. Castanea 6: 28-30, 1941.
Notes on Kentucky plants V. Castanea 6: 137-140, 1941.
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The red Azalea of the Cumberlands. Rhodora 43: 31-35, 1941.
A key to the deciduous trees of Ohio, native and planted, in winter condition. Edwards Bros. Ann Arbor, 1937. 31 pp.
A new station for Pachystima Canbyi. Castanea 6: 52, 1941.
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Chapters in "Naturalist's Guide to the Americas." Williams and Wilkins. Baltimore, 1926.
National Parks and National Monuments. pp. 20-27.
Ohio. pp. 354-372 (except 357, birds).
and Editor for Botany, of the Naturalist's Guide, 1926.

Braun-Blanquet

J. Braun-Blanquet
(Station Internationale Geobotanique
et Climatique, Montpellier, France)

Station Internationale Geobotanique Mediterraneenne et Alpine (S.I.G.M.A.) - Chemin du Pioch de Boutonnet, Montpelier

Le 22 Octobre 1956

Mademoiselle,

In answer to your letter of September 26 I give you below a list of my 'projects' which I consider most expressive of my work.

My work is grouped essentially around four themes:

1. Theory, classification and cartography of vegetational groupings, their floristic composition, their ecology, their genesis and their geographic distribution.

See: Prinzipien einer systematik der Pflanzengesellschaften auf floristischer Grundlage, 1921.
Vocabulaire de Sociologie Vegetale, 1922.
Pflanzensoziologie 1928 (translated into English and Spanish).
Pflanzensociologie, 2e ed. 1951, Vienne 631 pages.
Prodrome desGroupements Vegetaux, 7 volumes.
Sur l'importance pratique d'une carte detaillee des Associations vegetales de la France, 1944.
Instructions pour l'etablissement de la carte des Groupements Vegetaux, 1947.

2. Vegetation des hautes montagnes.

See: Die Vegetationsverhältnisse der Schneestufe in den Rätisch-Lepontischen Alpen, 1913.
Übersicht der Pflanzengesellschaften Rätiens. Vegetatio 1949.
La vegetation alpine des Pyrenees orientales, 1948.
La vegetation alpine et nivale des Alpes francaises, 1954.

3. Mediterranean historical phytosociology and phytogeography.

See: Les Cevennes meridionales (Massif de l'Aigoual), 1915.
L'origine et le developpement des flores dans le Massif Central de la France, 1923.
Etudes sur la vegetation et la flore marocaines, 1924.
Les Groupements vegetaux de la France Mediterraneenne, 1952.

4. Phytosociology and Pedology.

See: Le tapis vegetal de la region de Montpellier et ses rapports avec le sol, 1947.
Vegetation und Böden der Feld- und Zwergstrauchgesellschaften (Vaccinio-Piceetalia), 1954.

I am taking the liberty of sending you a few recent publications (projects) which also bear the titles of the series of publications of the Station which I have (created) (started) founded.

The fundamental and guiding idea of all my work is to arrive at a system, a rational classification, enabling the presentation of the entire universal phenomena which rule the community life of organisms, plants and animals.

As a rational basis for such work I consider the vegetational groupings as well circumscribed and floristically defined.

I notice with regret that in the USA one does not seem yet to have understood the entire implications of such a system which allows for the reunion in a general synthesis all that which bears upon the community life of plants and animals. Phytosociology is a science of synthesis--much more so than ecology!

Recevez, Mademoiselle, avec mes bonnes salutations l'expression de ma parfaite estime.

J Braun-Blanquet

Bray, J.R.

J. Roger Bray
(DSNZ, Palmerston, New Zealand)

BIOGRAPHICAL SCETCH OI J. ROGER BRAY

Born: Belleville, Illinois, June 20, 1929.

Married: December, 1961 to Gwendolyn Struik (Ph D. with Curtis in 1960)

Degrees: B. A. University of Illinois 1950, Ph D. with Curtis , University oi Wisconsin, 1955.

Visiting lecturer at Minnesota 1955-1957.

Assist. Professor Toronto 1957-1961

British Columbia 1961-1962

Principle Scientific Oiiicer at Grasslands Division, New Zealand, 1962-

Societies:
Botanical Society
Ecological Society oi America
Society for General Systems Research
British Ecological Society

Major fields oi interest:
Sampling and classifying vegetation
Productivity
Ecological theory
Climatology

PUBLICATIONS OF J. ROGER BRAY

1956. A study of the mutual occurence of plant species. Ecology 37:21-28.

Gap phase replacement in a maple-basswood forest. Ecology 37:598-600.

1957. --and J. T. Curtis. An ordination of the upland forest communities oi southern Wisconsin. Ecol. Monographs 27:325-349.

Climax forest herbs in prairie. Amer. Mid. Nat. 58:434-440.

1958. The distribution oi savannah species in relation to light intensity. Can. J. Bot. 36:671-681.

1959. --, D.B. Lawrence and L.C. Pearson. Primary productivity in some Minnesota terrestrial communities for 1957. Oikos 10:38-49.

1960. The composition of savanna vegetation in Wisconsin. Ecology 41:721-732.

The chlorophyll content of some native and managed plant communities in central Minnesota. Can. J. Bot. 38:313-335.

1961. An estimate for a minimum quantum yield of photosynthesis based on ecological data. Plant Physiology 36:371-373.

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1962. The primary productivity of vegetation in central Minnesota, U. S. A., and its relationship to chlorophyll content and albedo. In "Die Stoffproduction der Pflanzendecke".

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1963. - and L. A. Dudkiewicz. The composition, biomass, and productivity of two Populus forests. Bull. Torr. Bot. Club. 90:299-308.

Root production and the estimation of net productivity. Can. J. Bot. 41:65-72.

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1965 Forest growth and glacier chronology in northwest north America in relation to solar activity. Nature 205: 440-443.

Contemporary Ecologists Seminar

J. Roger Bray

1. Attended public schools of Urbana, Illinois 1-8 grade. Attended University of Illinois High School 1943-1947. Champaign County Scholarship to University of Illinois where attended 1947 to 1950, with B.A. High Honors in Botany. Graduate work at University of Wisconsin 1950-1955, Ph.D. on "The Savanna Vegetation of Wisconsin and an Application of the Concepts Order and Complexity to the Field of Ecology".

2. Worked as gardener and in hybrid corn fields, summers in high school and for U.S. Forest Service, summer 1947. Teaching assistant and research assistant, University of Wisconsin 1950-1955. Visiting Lecturer University of Minnesota 1955-1957. Assistant Professor, University of Toronto 1957-1962. Consulting Ecologist, Kelowna, B.C. and Research Associate, University of Saskatchewan 1963. Principal Scientific Officer, D.S.I.R., Palmerston North to present.

I was mainly stimulated in the direction of plant ecology by Dr. A.G. Vestal, University of Illinois, who was a most learned and entertaining lecturer with wide field experience and a file card knowledge of the world ecologic literature. Dr. Vestal's approach to vegetation was skeptical as regards the then existing vegetation theories and exacting in regard in individual pieces of vegetation. He saw the relational nature of vegetation and its continuous variability but his exhaustive knowledge of individual species and their aberrations kept him from generalizing. He anticipated the findings of the Russian and Wisconsin schools but did not publish on ecologic theory probably from a reluctance to further burden the literature.

My graduate research at Wisconsin was with Dr. J.T. Curtis. Dr. Curtis' early research was on orchid physiology, but he gradually shifted to plant ecology in the late 1940's. He was an outstanding lecturer and gave the most polished and logically organized talks I have ever heard. His research activities are probably well known by your seminar.

My research has been mainly in three areas:

1. Sampling and classification of vegetation.
2. Productivity and consumption of vegetation, and
3. Climatology.

1. My first paper tried to establish area levels on which to study interspecific association in plants. It suggested it was improper to use an association index when two species did not occur jointly within a sample area because they did not occur jointly in the same community area. This suggestion was later supported by Grieg-Smith but has not penetrated very far into ecologic practice. I am currently working on non-area methods to assess interspecific association, so far with no success. The difficulty is in getting an accurate density estimate, which is very difficult for non-random plant populations.

The paper in Ecology 43:328 suggested a way to estimate dispersion without an additional sampling procedure, if a standard non-area sample with a constant 4 or more plants per point was utilized. I think this technique is more sensitive (i.e. more exacting than the Clark-Evans or Hopkins methods in its delineation of non-randomness).

I worked out a multi dimensional ordination technique while I was a grad student and later Dr. Curtis and I applied the techniques to the upland forests of S. Wisconsin (Ecol. Monographs 27). The technique and its various modifications has been used in North America, mainly by Wisconsin graduates, and in England.

The paper on estimating the informativeness of vegetation gradients has been virtually ignored. It's an empiric technique, but I think the approach is correct and hope someone with more mathematical sophistication may someday make something of it.

The paper on ecologic theory was the result of a minor in philosophy as an undergrad plus much discussion (mainly with non-ecologists) at Wisconsin and a lot of reading. I've gotten many requests for this paper, some from the most unlikely places (M.D.'s in charge of loony bins). It has been referred to occasionally, but not much used. I hope it may someday form a bit of a bulwark against the silly simplistic notions of the reductionist minded molecular chemists.

I thought the paper on gap phase replacement would challenge a very sacred cow (the reproduction of sugar maple in its own shade because it is the most 'shade-tolerant') but the cow still seems to stand. The forest studied had such a thick canopy that sugar maple seedlings were in very low density except in the gaps caused by death of mature trees. Seedlings of species which were less shade tolerant but had a larger seed with more food reserves than sugar maple could hang on in the non-gaps for longer than sugar maple.

The paper in Ecology 41 on savanna vegetation showed a strong tendency for mesic prairie species to be more shade tolerant and xeric species to least shade tolerant. This made a linear gradient between prairie and forest vegetation possible. The savanna of Wisconsin had florae with mixtures of prairie and forest species; no species was limited to savanna (therefore savanna had no 'faithful' species and could not be described by the classification techniques of Braun-Blanquet). I spent a good deal of field time locating suitable stands; when found, they are a very beautiful vegetation type, especially the bur oak groves with long lower limbs sweeping out and sometimes touching the ground.

The paper on forest herbs in prairie was a bit of detective work following up some early observations of Gleason. From what I know now about the influence of man on vegetation and the similarity of some forest and prairie soils, its conclusions are probably correct and a much larger area of the Middle-West was once forested with mesic communities, as suggested in Gleason's classic work on Mid-West vegetation.

2. The papers on production ecology summarize work at Cedar Creek, Minnesota and in Ontario. The root sampling device worked out with D.B. Lawrence and L.C. Pearson enabled us to get root production estimates, something many production studies lack. The work on chlorophyll content and net production is still controversial and much research is in progress in Europe and Japan on this now. The difficulty is that plant physiologists have trouble thinking in terms of ecosystems and total land areas; they still refer back to work on individual leaves or one-celled algae.

The summary work on light reflectivity of vegetation will appear in Ecology shortly. It shows that the darker (less reflective) stands have a higher chlorophyll content up to a certain point beyond which increased chlorophyll doesn't influence reflection. We measured forests from high ladders and helicopters and we all survived.

The short papers on energy balance in cattail marsh and on a minimum quatum yield of photosynthesis are attempts to work with total energy balances for a community as a whole.

The work on primary consumption uses a very simple technique to gather data which is badly needed in energy balance studies. It showed leaf canopies of trees weren't terribly much consumed (at least compared to plants in my vegetable garden which can get over half eaten in no time at all).

The long review article on litter production is an extension of a literature survey I made while starting a litter sampling project at Toronto.

3. The paper on CO_2 changes in Tellus was the result of my interest in climatic theories. I tried to apply statistical analysis to the proposed recent increase in atmospheric CO_2. After publications I got letters thanking me for showing it hadn't increased and thanking me for showing it had increased.

The work on glacial chronology of Yoho and President glaciers used some of the techniques worked out by Lawrence and by Mathews plus some innovations of our own. The chronologies of tree growth and ice growth seem to support the solar radiation climatic hypothesis. This work has been summarized in Nature and will hopefully get a reaction out of someone. It has given me a crude outline for climates of the past several millinia which I'm now using in analyzing C-14 variation.

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Stanley A. Cain
(University of Tennessee)

26 Sept. 55
Rio do Janeiro

Mr. Ernest S. Hamilton
Department of Botany
Rutgers University

Dear Mr. Hamilton,

If you will write to the Secretary of the Conservation Department, Mrs. Elizabeth Dakin, School of Natural Resources, University of Michigan, Ann Arbor, she can send you a list of my publications. Also asker to find a reprint of a brief sketch of me published in the Tennessee Acad. Sci. at the time I was President.) I just noticed my new word "asker" in the above).

I do not have the time to answer your request at any length. What philosophy I may have about ecological and general scientific matters should be apparent in my publications. You would notice that recent years have a trend toward application of ecological thought and methods to human ecology, especially resource utilization ahd human population trends. You would also notice that I have been more interested in methodology than I have in turning out descriptions of vegetation — this applies in pollen analysis as it does in my interest in Phytosociological matters. But as for giving you a "critical autobiography" as you request, I wouldn't do it if I could. My god, you don't want a person to take off his shoes and show his feet of clay — that's for someone else.

I will add my present work. I'm a member of the UN Technical Assistance Mission to Brazil. My first assignment here was to help plan a study of a fishing millage now being affected by industrialization. Since July first I have been working with two Brazilians on a Manual of Vegetation Analysis— a "how to do it" book.

Please tell Murray hello, I think he is a swell fellow and a damned good ecologist.

Yours,

Stanley A. Cain

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Volume 20, No. 2, April, 1945

DR. STANLEY A. CAIN, THIRTY-FIFTH PRESIDENT OF THE TENNESSEE ACADEMY OF SCIENCE

Stanley A. Cain, born a Hoosier in Jefferson County, Indiana, June 19, 1902, attended Butler University and secured the B.S. degree in 1924. Later he earned both the M. S. and Ph.D. degrees from the University of Chicago where he held an assistantship. He has taught in many schools, including Butler University, Indiana University, and for the last ten years at the University of Tennessee where he is now Professor of Botany. He was a John Simon Guggenheim Fellow in 1940-41, and a collaborator of the U. S. Soil Conservation Service, Physiographic and Climatic Research Division for a time.

He has been very active in many professional organizations, including the Ecological Society of America (he was treasurer, 1938-44), Sigma Xi (he is president of the U. T. Sigma Xi Club), Phi Kappa Phi (he is now secretary of the U. T. Phi Kappa Phi Chapter), the Torrey Botanical Club, the Indiana Academy of Science (fellow; editor of Proceedings, 1930-35), the Ohio Academy of Science (fellow), Tennessee Academy of Science (Botany editor, 1944-). He has been on many editorial boards, including Ecology, 1932-34; Ecological Monographs, 1938 1940; American Journal of Botany, 1942- ; Bulletin of the Torrey Botanical Club, 1942- ; Botanical Review, 1945- . Dr. Cain has written many technical papers mostly in the fields of ecology and plant geography, and one book, "Foundations of Plant Geography," published in 1944 by Harper and Brothers. This book has been well received and commended for its critical examination of the bases underlying Plant Geography.

Dr. Cain was co-founder of BUTLER UNIVERSITY BOTANICAL STUDIES and originator of the Cold Spring Harbor Conferece on Plant and Animal Communities, 1939. He was an Invitation Lecturer, Seventy-fifth Anniversary Celebration, Torrey Botanical Club, 1942. In 1940, he was president of the Knoxville Science Club, and in 1943-44 of the U. T. Faculty Club. He married Louise Gilbert Marston on July 29, 1940. They have one son, Stephen Gilbert, botn October 30, 1941.

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1928

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