

# LIMNOLOGY AND OCEANOGRAPHY BULLETIN

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limnology and oceanography

## AN APPRECIATION OF ALFRED C. REDFIELD AND HIS SCIENTIFIC WORK

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*[A Brief History of this Biography, Jonathan J. Cole, Institute of  
Ecosystem Studies, 65 Sharon Turnpike, Millbrook, New York 12545  
USA; [colej@ecostudies.org](mailto:colej@ecostudies.org)*

Why is ASLO publishing a biography of A. C. Redfield in this special issue of the Bulletin? I am sure that most ASLO members would agree that Redfield was sufficiently interesting as a person, and well beyond sufficiently significant as a scientist, to warrant a biography. If you do not yet feel that way, after you read P.J. le B Williams's interpretation of Redfield's life and I am sure you will. But, that is only part of the story. Several years ago the ASLO Board realized that all of the society's awards that were named after someone were named only after limnologists (G.E. Hutchinson, Ruth Patrick, Raymond Lindemann). Of course both limnologists and oceanographers receive these awards and do so in about equal numbers, but that no oceanographer had been memorialized with a named award seemed a salinity imbalance in need of fixing. The Board decided to take the one award that was not yet attached to a name (the Lifetime Achievement Award) and name it after an oceanographer whose career exemplified a lifetime of scientific achievement, A.C. Redfield. Since that time the Board has also added a new award, named after John Martin, but that is another story.

In the mean time and quite serendipitously, Dan Conley, who was then on the Board, had been given an old photocopy of a biography of Redfield which was prepared by his daughter (Elizabeth Marsh). Could ASLO, Dan suggested, publish this biography and make it part of the Redfield Award? This seemed like a good idea but there were some issues. Who owned the copyright of this unpublished manuscript? What would Redfield's descendents think of this plan? How were we going to turn this family document into something ASLO could publish? All Dan knew was that he got the photocopy from Scott Nixon. Scott, it turned out, got it from John Farrington. John Farrington, it turned out was still in contact with the Redfield family. P.J. le B. Williams, whose term was just ending on the ASLO Board, volunteered to turn the unpublished manuscript into a shortened text that ASLO could publish and the result is published here. So by a circuitous route and a lot of people to thank, ASLO got permission to use Elizabeth Marsh's manuscript as the basis of Peter Williams short biography of A.C. Redfield. ]

## PREAMBLE

Redfield came from a family with both academic and engineering roots. The former was to have a more powerful effect on his career. His career would appear to be more opportunistic than directed, driven largely by circumstances: he moved from biology to physics, back to biology and into oceanography, chemistry, then geology.

Without doubt, his major contribution was his seminal papers rationalizing the proportion in which the major biological elements occur and react in the oceans. It is interesting, when following his career, to try to understand what forces drove him and what opportunities availed themselves.

The main part of this biography is drawn from the transcripts of an interview in 1973 of Redfield (then aged 83) by his daughter – Elizabeth “Libby” Marsh – formerly Chair of Environmental Studies at Stockton College in New Jersey. The interview occurred some 10 years before his death. In preparing the biography, I have used his words where possible, which are given in italics and within quotes; where appropriate I have used his daughter's questions, again in quotes. In this way I have tried to allow Redfield and those close to him to play a major part in telling his story. There are also direct citations from the transcripts in italics, but not in quotes. Whose words these are is unclear, but I suspect they were added by Elizabeth when the transcript was compiled. I have supplemented the transcript with other sources: notably Redfield's own writings and also an interview in August 1974 by Elsa Keil Sichel and Dr. John Dale Roslansky, who were recording oral histories for the Woods Hole Library's Historical Collection. These extra sources have added depth and enabled me to confirm details such as dates. I have added some clarifying comments to the abstracted text in square brackets.

As I have discovered, a biography is more about what you elect to leave out, than what you chose to put in. To keep the biography to an acceptable length, 90% or more of the material available to me has been omitted. Those who knew him well may feel that as a consequence there are biases and perhaps serious omissions – for these I apologize. Further, I have made no

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attempt to consider his contribution to administration at Harvard and WHOI, which would have been considerable – this would need to come from someone within those institutions. I have deposited all the material I obtained in the ASLO office so that others can access it.

I am greatly indebted to Marisa Hudspeth at the Woods Hole Oceanographic Institution for confirming the dates of Redfield's career points at the Institution and for sifting out a valuable collection of bibliographic information on Redfield. I also am pleased to thank Jen Walton (MBL) for obtaining from Elizabeth Marsh the photograph of Redfield relaxing, Everett Fee for his meticulous proof reading of the text, and John Marra, Karl Banse, Dave Kirchman, John Farrington and Nick Fisher for their valuable comments on the earlier drafts, and Michael Pilson for his encyclopedic knowledge of oceanographic history. The feedback from these colleagues greatly improved the accuracy and the detail of the text. Redfield's son, also Alfred, is thanked for providing family details and correcting errors. I am indebted to David Roberts, in our School of Ocean Sciences, who improved the quality of the photograph of Redfield as a child, which came from a cutting from the *Woods Hole Weekly* newspaper. Finally, I thank the ASLO Board for allowing me to volunteer for this fascinating and challenging task. It would be nice to think that others may enjoy reading the short account of this remarkable man's life as much as I enjoyed researching and writing it.

## PERSONAL BIOGRAPHY

1890	Born November 15 <sup>th</sup>
1913	Married Elizabeth Sewell Pratt – June 19 <sup>th</sup>
1913	Entered graduate school, graduating in 1918
1920	Elizabeth died – Aug 4 <sup>th</sup>
1921	Worked at the Cavendish Laboratory (UK)
1921–22	Field expedition in Peru
1922	Married Martha Putnam – June 7 <sup>th</sup>
1922	Moved to Milton, south of Boston
1923	Birth of Elizabeth Redfield – July 18 <sup>th</sup>
1926	Birth of Martha Washburn Redfield – March 7 <sup>th</sup>
1929	Birth of Alfred Guillou Redfield – March 11 <sup>th</sup>
1930	Joined the staff of WHOI
1930–31	Sabbatical along with family in Munich
1931	Moved to Cambridge, Mass.; purchased summer home in Woods Hole
1941	Family moved to Woods Hole on a year-round basis
1942	Associate Director at WHOI (until 1956)
1956	Formally retired to start full time work at Barnstable
1957	Elected Senior Oceanographer Emeritus
1973	Interview with Elizabeth Marsh
1974	Interview with Elsa Keil Sichel and Dr. John Dale Roslansky
1983	March 17 <sup>th</sup> Alfred Redfield died
1983	June 1 <sup>st</sup> Martha died

*Alfred [Allie] Clarence Redfield was born in Philadelphia in 1890, a late, youngest child. His brother, J. Howard, was an engineer, a student of the Basque language, and a gifted mathematician. One sister, Heloise, was an artist; the other, Mary, became mother of Donald R. Griffin, the animal ethologist. Howard's daughter is Priscilla Redfield Roe; Allie's children are Elizabeth R. Marsh, Martha W.R. Koch, and Alfred Guillou Redfield.*

We have more details of the paternal side of his family than his mother's: *Alfred was a great-grandson of William C. Redfield, first president of the American Association for the Advancement of Science, a self-taught man, an engineer whose scientific work was in meteorology and paleontology. Alfred's grandfather on the other side was Asa Whitney, the railroad wheel manufacturer, who was an early partner of Nathaniel Baldwin, making locomotives. Alfred's grandfather, John Howard Redfield, was a serious amateur botanist; his employment was with the Whitney firm. Robert, Allie's father also worked in the Whitney family business until it failed. Robert's forced early retirement gave him time with his youngest son; they spent summers in Barnstable and wintered outside Philadelphia, in Wayne. Robert's real gift was as a photographer, an artist of the naturalist school; his photographs are now at the Library Company in Philadelphia, and at Yale.*

Of his mother's side of the family the interview gives us no more information than: *Alfred's mother was Mary Guillou, daughter of Rene Guillou, a son of a Huguenot family that had migrated to Philadelphia from Haiti.*

The Redfield family had a long standing association with the Cape, Woods Hole and Barnstable in particular and enjoyed summers there: *"During those summers everyone migrated to Woods Hole, to the expansive house overlooking the ball field off Millfield Street. The household in those summers included parents, the three children, cousin Priscilla Redfield, and grandmother Mary Guillou Redfield from Wayne, an occasional extra child of a friend and, during the war, Anne Henry, from England."*

Redfield's scientific career began at Harvard in 1910. *During the college years, when he was 21, Alfred married Elizabeth, his first wife. He wrote his daughter, also Elizabeth, at the time of her marriage, "We were very happy..." and from old photographs this is evident. Elizabeth was lively and beautiful, but she had had rheumatic fever and could not have children. A few years into the marriage, she had a nervous breakdown and then died during the 1918 flu epidemic. Allie never spoke of her to his children."*

I have a problem settling upon the date of his marriage to Elizabeth. The above abstract, which appears to be added later as a note to the interview, would suggest 1911 or 1912 as the year; in his brief autobiography in the Harvard Class of 1914, the date of their marriage is given very specifically as June 19, 1913, and the date of Elizabeth's death as 4<sup>th</sup> Aug 1920. I'm inclined to accept these dates as the most reliable ones.

He passed through graduate school (1913-1918) and it was during this period he established his initial contact with the Marine Biological Laboratory at Woods Hole. Following his Ph.D., he was hired as an instructor in Physiology at Harvard Medical School Boston for one year. He then took a job at Toronto, in Macleod's group, leaving after about a year because

of his first wife's illness. He returned to Harvard Medical School, remaining there until 1931.

*In 1922, while teaching at Harvard Medical School, he met and married Martha Putnam. Martha was an artist and had served with courage, as a therapist, in the First World War. She was the daughter of Charles P. Putnam of the Boston medical family descended from James Jackson, a founder of Harvard Medical School. It would appear to have been something of a lightning romance, as a mere six weeks apparently elapsed between their meeting and their marriage.*

Whilst at the Medical School, he made two major excursions abroad: to the Cavendish Laboratory (UK) and to Peru. In 1931 he became Associate Professor of Physiology at the main Harvard campus in Cambridge. After a sabbatical year at Munich, where he learned to ski (very few people did, in those years), he was appointed Director of the Biological Laboratories, then in a striking new building and a year later was elected Chairman of the newly formed Biology Department. His links with Woods Hole grew with time. In the early part of World War II, the family took what was to be a permanent change of location: *When World War II began, in 1941 [she is referring to the US entry], Redfield left Harvard and moved to Woods Hole to live permanently, first in the house on Millfield Place, and after 1950 to the house he and Martha built on Maury Lane, on land he had purchased from the Faye Estate and later sold to the Oceanographic Institution.*

The war years involved him in applied science which evidently he enjoyed. In the post-war period, largely driven by necessity, he continued on a number of applied problems.

He retired in 1956. One thing is clear, there was no sign of retirement from science: his last publication "*The Tides of the Waters of New England and New York*" was published in 1980, three years before his death.

What was he like as a man? Sadly I never met him; however from the transcript of the interviews one can gain a pretty good impression. An appreciation of him by Bill Von Arx (reproduced in part in his obituary by the National Academy of Sciences, and given in full below) certainly seems to chime with the impression one gets from his writings:

*"Alfred Clarence Redfield was an inherently civilized man: urbane, courtly, gracious, but at the same time forthright, redoubtable and demanding. He was neither modest nor immodest. A man of pure reason, he was thoroughly convinced of his own worth, yet always open to rational improvement and adventure. He enjoyed his mark as a plain-mannered patrician; one who could move as easily in the company of artisans and tradesmen as among scholars.*

*He employed his life and talents skillfully. His sense of practical reality served him well as an administrator, but it was his potent curiosity, about nature in general and about coastal waters, estuaries and wetlands in particular, that made him a truly influential naturalist. He turned his life-long intrigue with ocean margins and winds into a scientific currency that had public engineering as well as scholarly values.*

*Well educated at home and abroad, qualified in all the traditions of scholarship, his academic learning still needed constant exposure to nature to be productive in the ways he deemed worthy. Accordingly, he spent much time in-the-field, keeping nature close at hand to guide his thinking and writing. No passive observer, he labored as necessary to*



Redfield as a child.

*expose the inner features of nature, which he explained to colleagues and students as he went along. His working methods were honest expressions of his character. He was logical and not above hard work in all he did as a gentleman farmer, sailor, citizen, forester, architect, historian, lecturer, writer, pipe-smoking editor and friend. A few months past age 92, his book on tides newly in print, Alfred Redfield died, rich in wisdom, honor and affectionate esteem.*

## SCIENTIFIC BIOGRAPHY

In describing his scientific biography, I shall broadly follow the structure in the transcript of Elizabeth Marsh's interview.

### Education

Haverford College, 1909-1910  
S. B., Harvard University, 1910-1913,  
degree awarded *magna cum laude* in 1914  
Ph.D., Harvard University, 1913-1918

### Harvard University

Instructor of Physiology, Harvard University, 1918-1919  
Assistant Professor of Physiology, Harvard University,  
1921-1930  
Associate Professor, Harvard University, 1930-1931  
Professor, Harvard University, 1931-1956  
Director of Biological Laboratory, Harvard University, 1934  
Chairman, Department of Biology, Harvard University,  
1935-1938  
Professor, (Emeritus), Harvard University, 1956-death

### Woods Hole Oceanographic Institution

Senior Biologist, 1930-1938; 1942-1953  
Research Associate, 1938-1940  
Associate in Marine Biology, 1940-1942  
Associate Director, 1942-1956  
Senior Oceanographer, 1953-1956  
Senior Oceanographer Emeritus, January 1957-death  
Member of the Corporation, Woods Hole Oceanographic  
Institution, 1936-1974  
Member Executive Committee, 1943-1955  
Deputy Clerk of the Corporation, 1951-1952; 1954-1955  
Associate, 1959-death  
Honorary Member of the Corporation, 1974-death

### Other Offices Held

Managing Editor, Biological Bulletin, 1930-1942  
Lecturer, Commission Relief Belgian  
Educational Foundation, 1931  
Consultant, Bureau of Ships, Department of Navy, 1942-1943  
U. S. Weather Bureau, 1954-1955  
Beach Erosion Board, 1955  
Creole Petroleum Corp., 1955

### Periods at Other Institutions

Assistant Professor of Physiology, University of Toronto,  
1919-1920  
Cambridge University (Cavendish Laboratory and Christ's  
College), 1920-1921  
Visiting Professor, Hopkins Marine Station, Stanford  
University, 1930  
University of Munich, 1930-1931  
Walker Ames Professor, University of Washington, summer 1948  
University of Minnesota, 1956

### Memberships

American Physiological Society; Secretary, 1929-1930  
Ecological Society of America, Vice President 1945;  
President, 1946  
Natural Resources Council of America, Chairman,  
1946-1948; Honorary Member, 1948-death  
American Zoological Society  
American Geophysical Society  
Geochemical Society  
American Geophysical Union, President, Section on  
Oceanography, 1950-1953  
American Society of Limnology and Oceanography,  
President, 1955-1956  
Marine Biological Association of United Kingdom,  
Honorary Member, 1956  
Chicago Academy of Sciences, Honorary Member, 1946

### Trustee

Boston Society of Natural History (Honorary)  
Marine Biological Laboratory, 1930-1953; Trustee Emeritus,  
1960-death  
Bermuda Biological Station for Research, 1944-death;  
President, 1962-1965  
Woods Hole Oceanographic Institution, 1936-1964;  
Honorary Trustee, 1964-death  
Member of the Corporation, Marine Biological Association  
of India

### Awards and Distinctions

Ph.D., (Honorary), University of Oslo, 1956  
Agassiz Medal, 1956  
Elected to the National Academy of Sciences, 1958  
D.Sc., (Honorary), Lehigh University, 1965  
D.Sc., (Honorary), Memorial University  
of Newfoundland, 1967  
D.Sc., (Honorary), University of Alaska, 1971  
Walker Prize in Natural History, Museum of Science,  
Boston, 1973  
Elected to American Academy of Arts and Sciences  
[I understand this is an elected honor]

## SCIENCE IN HIS EARLY YEARS

The Redfield family, starting with the patriarch William C. Redfield, had and continued to play an active role in academia and science in particular; so it is not surprising that Redfield's scientific interest started young. Three members of the Redfield dynasty were elected to the National Academy of Sciences: Alfred himself, his son (Alfred Gillou Redfield) and his nephew Donald Redfield Griffin.

In the interview, Elizabeth enquires of him: *"When did you develop social and scientific interests?"*

He replies: *"My earliest scientific recollection was, one day I was coming home from school. I must have been 10 or 12 years old. And suddenly I realized that I could make a collection of butterflies. I don't recall what the stimulus was. But I remember very vividly...I can picture the exact place in the field—I think it was where this brilliant idea came to me—and I went to work, it was in the spring of the year, and I collected butterflies.*

*And then I was very fortunate, because my grandfather (John Howard Redfield) had been connected with the Academy of Natural Sciences Museum in Philadelphia. He was a very competent botanist, though not a professional in the modern sense. And as a result of this connection, my father was able to take me to the Museum and introduce me to the Curator of Entomology and he very kindly allowed me to come into his offices, which were behind the scenes. Well, I suppose I was in my early teens.*

*Then as the years went by I became interested in birds. I don't know just why I became interested in birds. .... Skinner, who was curator of entomology, introduced me to Mr. Stone, who was the Curator of Birds. And he introduced me to the Delaware Valley Ornithological Club, which is a serious bird club, Whitmer Stone was the leading bird man in Philadelphia. He was the editor of The Auk and this club was where all the really serious bird students around Philadelphia met in his office at the museum and talked about birds."*

Redfield's formal school education was at Haverford School in Philadelphia. The school was originally a Quaker College although: *"Alfred attended Haverford School as a scholarship boy - the Redfields were Episcopalians but Allie maintained a life-long aversion toward any formalized religion. After a year or two at Haverford College, young Redfield was sent up to Harvard where he later became a professor. This account of his life as a scientist starts in 1910, with his college education at Harvard."* A short statement by him of his religious views in the 25th Anniversary Report of the Harvard College Class of 1914 is delightfully dry: *"My religious activities are limited to an attempt to get along so as to keep myself and others out of trouble. I support charities which appeal to my special interests in education, conservation, natural history and civil liberties, and those which seem to be preventive rather than curative."* In the same report he sums up his political views: *"Politically I am a faded pink - voting republican against my will in state elections and having voted for Wilson, Al Smith and F.D.R. I think the New Deal deserves great credit for attempting to face modern problems in a constructive way, and although I don't like it, I feel confident that an increasingly controlled economy is an inevitable necessity. Having been an experimentalist for twenty-five years, I expect many experiments to fail."*

## UNDERGRADUATE YEARS AT HARVARD (1910-1913)

*"I went four years, the high school years, to Haverford School and I went for one year to Haverford College and then transferred to Harvard. I had to enter as a freshman, all over again. When I made this move then I had letters of introduction from Whitmer Stone to Professor Parker at Harvard and to some of the people in the - Nuttall Club, the local bird club in Boston. So it all handed along in a very nice way from the beginning, because of this chance connection of my grandfather with the museum.*

When asked the following question by Elizabeth: *"Did Harvard perceive your education mainly as a scientist or did you also get a liberal background there?"*

Considering the diversity of his subsequent scientific work, his reply is fascinating: *"Well, there I think something unfortunate happened. I think the letters which Whitmer Stone wrote to Professor Parker indicated to Parker that I was destined to become a museum man. The result was no mathematics, no chemistry, no physics, and all these things which I should have had in order to become a versatile scientist."*

The interview goes on:

*"So you missed out on the physical sciences."*

*"Yes. I had the introductory course in chemistry, in which I learned almost nothing and of course an elementary course in organic chemistry. That was all the chemistry I had. I had the freshman physics course. I'm not aware that I learned anything I hadn't already learned in school. Mostly mechanics and a little bit about light and sound and so on.*

*"Electricity?"*

*"Just enough to ring a bell. I had to start perfectly fresh"*

This, from a man who has made contributions in pure physics, physiology and every single aspect (chemistry, physics, biology and geology) of marine science. One can but conclude that he learned his science by doing it.

When asked about training in ecology he replies: *"At that time there was no ecology. I remember discussing ecology [in the 1930s] with Tom Barbour, who was the director of the Museum of Comparative Zoology, and suggesting that perhaps we should do something about ecology.*

*And he [Barbour] said, "Ecology? Oh, I think we can leave that to the institutions in the middle west." It was developing there. Botanists were looking at plant communities."*

## GRADUATE SCHOOL AT HARVARD (1913-1918)

Redfield decided to continue studies at Harvard, joining Harvard Graduate School. We gather from the interview that, at that time, this was not the usual progression since biology graduates conventionally continued into medicine. He studied under a Dr. Parker. His account of the initial interview runs *"Well, when I was ready to do graduate work I went to Parker and declared my desire. And he said "Yes, that would be very nice. And did you want a morphological or physiological problem?" I had become acquainted with physiological problems through an advanced course he gave on the physiology of the nervous system. And there was another, I forget the exact title, Animal Behavior or something of that sort.*

*And Dr. Parker reached up into his desk and pulled out from a cubbyhole a little bunch of cards and he fingered through them and finally he pulled out a card and he gave it to me and said "Here, read this, and think and see if you could do anything more with it".*

*Well. This paper Parker gave me was about the control of the chromatophors which controlled the color of the horned toad, which is a lizard. The problem as Parker and everybody else conceived it was to what extent do these cells react to the nervous system and to what extent do they react directly to the light or the temperature or whatever it is which would cause a thing to expand or contract."*

*He then follows with the comment: "...One day there came a turning point, probably the most clear-cut turning point in my whole career.*

*Walter Cannon came over from the medical school to talk to our journal club Friday afternoon. He had just, you might say, completed his work on the role of the adrenal glands in the physiological reactions to the emotions. And he had shown that in a variety of ways the adrenalin which was produced by these glands produced reactions which were characteristic of great emotion.*

*It wasn't much the idea of stress so much as the idea of the mechanism of hormones, which was a relatively recent development, you see. And it had not yet reached out into the physiology of the lower organisms. So what I realized is that if I wanted to make the horned toad turn pale, the quickest and easiest way to do it was to hold it upside down. And it would struggle to right itself. And it would turn pale. And then I had a reaction which I could produce and control, you see. And I had something which I could study systematically, you see."*

*I suspect the contact with Cannon had a greater influence on his scientific career, than opening the door to the Horned toad problem.*

*He started experimenting with the adrenal glands and injecting adrenalin into the animal. He relates a humorous anecdote regarding his attempts to prepare his own samples of adrenalin*

*Elizabeth: "Where did you get the adrenalin?"*

*"Well, I think you could buy it in a little bottle made by Parker [sic] Davis, but Walter said, "Well, now you want to make some adrenalin yourself. Go down to the slaughterhouse and get some adrenal glands." So I went down and there was a great big fellow in a bloody apron, with a beard. He was a Rabbi of course, and he showed me where the ox was hanging by its heels, and he said, "There he is; go see if you can find 'em."*

*And I hunted and hunted and I couldn't find anything. [Then] the manager told us they'd been all carefully removed to be sent to Parker [sic] Davis. But I got 'em ultimately and I ground them up and I extracted them with salt solution or something and made my adrenal extract and tried the experiment and it worked as expected."*

*The work was written up and published in 1916 in Science as a single author paper. Over the next two years three more papers were to follow on the topic.*

*There was, in the transition from his undergraduate period, an event that one may see as a highly significant one in his career "I guess that at the end of my college period, certainly in this transition from undergraduate to graduate, I came to Woods Hole, and took the course in physiology. [In the interview with Sichel and Roslansky, Redfield was uncertain over the date referring to it as "either as an undergraduate or during the first year of my graduate study" – which would put it as 1913-14. However, the year is given as 1915 in an obituary in the Woods Hole Weekly.]. And here I was being taught by four people from other parts of the country, all pretty capable people, and I discovered I*

*needed to know some chemistry! So I went back to Cambridge and began taking some additional qualitative chemical analysis. And I couldn't fit it in ... at that time I was a teaching assistant. I couldn't fit in to the Harvard course. But I went to Professor Baxter who taught quantitative analysis at Harvard but also taught at Radcliffe. And he invited me to attend his lectures at Radcliffe and put me into his laboratory at Harvard. So I must have been one of the first Harvard students to go to Radcliffe! [Radcliffe at that point in time was an all women's college] That was very fortunate because he was a marvelous teacher and it was from him that I learned this quantitative analysis. And, boy, we really did learn it. He taught entirely from the point of view of physical chemistry. So I learned a great deal of physical chemistry. You see the separations you make all depend on differences in solubility. And so I really understood what was going on. That was invaluable! ... That was all very fortunate."*

*He received his Ph.D. degree in 1918 "Well, that was the first stage in my experience. I finished up my thesis. I couldn't see where it led to except more of the same." Not an uncommon situation, but all too seldom recognized!*

## **HARVARD MEDICAL SCHOOL (1918-1931)**

The chronology of events over the next few years is a little difficult to sort out and there are inconsistencies between the various accounts. What follows is as accurate as I am able to achieve.

*In 1918-1919, he took up a post as instructor in physiology at Harvard Medical School with Professor Cannon. "And at the end of that time, while I was getting my degree, he then offered me a teaching fellowship to teach in his course. I'd never taken the course, or any other course in physiology. But that I did. For ten years I was teacher of physiology in the medical school. That's where I learned my basic physiology. Cannon was one of the pioneers of the concept of homeostatic control – indeed he invented the word. It seems very probable that Redfield's association with him, and also later with Barcroft, was significant in influencing Redfield's ideas over biological control of nitrogen and phosphorus in the oceans. From 1919 to 1920, he was Assistant Professor at the University of Toronto. We know that at this time Elizabeth died, one of the victims of the influenza pandemic that took so many lives after the First World War. In 1921, he joined the Harvard faculty as an Assistant Professor of Physiology and worked on radiation: "I was given the opportunity to take up the study of radium radiation and similar radiations through Alec [sic] Forbes, who was teaching in the same department. And it so happened that his brother had died as a relatively young man, of a cancer. And there was a natural desire in the family to do something about cancer, you see. And the use of radium for treating cancer was a new idea which was being pursued. And they put up the money to provide for an instructorship in the department, with the understanding that I would work on this problem."*

*His appointment at the Medical School appeared to put him in charge of preparing the radium needles for cancer treatment. He became interested in quantifying and understanding the effect of radiation. He describes his approach: "My thought was: We are going to study a reaction which is unknown. And we'd better start studying with something that is known. Now what is known is about the physics of radiation. You know what kind of radiation you had. You knew their penetrating power. You could measure those*

things. So my problem was to see how different rays, with different physical properties, how the effects on some sort of living tissue vary. And I came to Woods Hole looking for marine material which I could use. And I found perfect material which is the egg of the worm Nereis, a particular species of Nereis, which when it is fertilized secretes a jelly. You have an egg, which is so large, produce a mass of jelly which is, we'll say, ten times or twenty times the radius [of the egg.] If you put the egg in sea-water that has a little ink in it, the jelly pushes the ink back. So you can measure the size of the jelly.

What the radium did was to prevent some of this jelly from coming out. The result was the membrane of the egg was lifted off the central mass of protoplasm by a small but measurable distance, so what I had to do was to measure this distance, and compare it with the strength or the physical intensity of the radiation.

I had a physiological reaction which I could quantify, and compare it quantitatively with some physical property of the ray.

Now, the question was, really – these rays were already known, even at that time, as ionizing radiation. That was because as you passed them through air, the air would become ionized and would become a conductor of electricity, and that you could again measure, so I had a method of measuring the ionization.

But also at that time there was a growing interest in colloid chemistry. But colloid chemistry at that time depended very much on the idea that the elements became charged and alpha rays carried positive charges and beta rays carried negative charges and X-rays didn't carry any charge. So here was the possibility of distinguishing between ionizing power and charge.

So then I could say, "Maybe this effect is due to the ionizing power, or maybe it is due to the charge." Well, it turned out that I could establish a good proportionality between the ionizing power and the physiological effect, but could not distinguish any difference in the quality of the effect relating to the direction of the charge carried by the ray.

What I was doing was establishing that the physiological action of the ray was due to its ionizing power. The ionizing power would depend on the energy of the ray and the degree to which this energy was absorbed in producing the ions. So I did experiments with alpha rays and gamma rays and also with ultraviolet light. They all produced the same fundamental actions. The only difference was that alpha rays and ultra-violet rays only produced a bubble on one side, because they didn't penetrate.

What I did was to hang these cells in a hanging drop and then radiate them from underneath. And it was only on the side which was actually on the surface which these rays could penetrate, whereas beta rays could go right through and affect both sides essentially equally, and the same with X-rays.

Well, I think that was – in some ways – the most original and basic work I ever did. And it's of great interest now."

Much of this work was undertaken in collaboration with an E. M. Bright. Altogether they published some 12 joint papers on the effect of radiation over the period 1918 to 1924. There is no mention of Elizabeth Bright in his writing and interviews, so we are left in the dark of the details of this extensive collaboration.

## PERIOD ABROAD (1921-22)

It would appear that at the start of his tenure of this post, he was awarded a fellowship (the John White Browne Fellowship) which enabled him to visit Rutherford at the Cavendish

Laboratory at Cambridge in the UK. He shared a laboratory there with James (later Sir James) Chadwick. Just ten years later, Chadwick was to "split the atom" and so discovering the neutron. For this he was awarded the Nobel Prize in 1935. Redfield's observations on Chadwick and the Cavendish lab certainly do not presage Chadwick's future successes: "So I went to Rutherford and he said, "Yes, we have a course on radioactive measurements. You go upstairs; you'll find Mr. Chadwick – is in charge." He was a lonely looking graduate student in a rather soiled gown. But he took me to a room about twice as big as this. And the only things which were in it were about half a dozen pine tables covered with little pieces of tubing and little coils of wire, that had accumulated. And in one corner there was a sink with cold water, and somewhere else there was one gas jet. As far as I can find out, Chadwick did not write an autobiography so we have no idea what he thought of Redfield – which would have been interesting.

Redfield, being very much of a practical bent, evidentially took the "string and sealing wax" culture of British science of that time very much in his stride. "And instead of [Rutherford] giving us a lot of instruments, we made – a gold leaf electroscope!"

He resided at Christ College – Redfield's observations on a Cambridge college in the 1920's would have been interesting to read but alas I have unearthed nothing.

A significant event, whilst he was at Cambridge, was through a colleague, Arlie Bock, who led to Redfield being invited to join the International High Altitude Expedition to Cerro de Pasco in Peru led by Sir Joseph Barcroft. Redfield describes his role: "But while I was there I was in contact with the physiological laboratory. And Joseph Barcroft was then organizing a trip to the high mountains (Peru) to study the physiological effects of low oxygen pressures. He wanted somebody to take X-rays. And I was a physicist! So he asked me if I would like to go along and take some X-rays. I knew nothing about X-rays, other than to press the button. But I did know photographic techniques, so that I had no trouble handling films and ideas of exposure and that kind of thing.

So I went to Peru with Joseph Barcroft and took some perfectly acceptable X-rays of our chests and hearts as we went up and down the mountains. I actually took a dental X-ray of myself on one occasion when I got a bad toothache. I decided I wouldn't ask my medical friends to pull the tooth out."

The expedition (1921-22) was a joint US/UK venture – five participants from the US and three from the UK. The US contingent sailed on November 16<sup>th</sup> 1921, New York to Lima, where they joined their UK colleagues. The study site in the Andes was chosen, as the Central Pacific Railway gave them easy access to high altitude work camps and towns. Redfield and the group travelled by train to Oroya (12,000ft), then onto Cerro de Pasco (14,200ft). The Americans departed on the January 18<sup>th</sup> aboard the steamship *Ebro*.

A passing comment by Barcroft was to cast the die for the next ten years of Redfield's research career. "During this experience with Barcroft he said to me, "You know, there are a lot of substances in the blood of invertebrates which do what hemoglobin does, but which are copper compounds. It would be very interesting to examine those."

## RESPIRATION WORK AT HARVARD (1922-1930)

On his return to Harvard Medical School in 1922, Redfield took up his duties as now Assistant Professor of Physiology. His respiration work, which spanned some ten years, was diverse. Following Barcroft's pointer he gave particular attention to hemocyanin and the respiration of invertebrates. Quite a few papers dealt with aspects of metabolism of the horseshoe crab, *Limulus*, which occurs in the salt marshes and beaches in the Woods Hole area. His daughter records that her earliest memory is collecting these organisms at the town beach in Barnstable in 1925. Elizabeth would have been 2 years old at the time and one would imagine the horseshoe crab to be a fascinating, if not terrifying, sight for a 2 year old to behold. Redfield's work of that period was not restricted to *Limulus*; he was interested in the comparative metabolism of hemocyanin in organisms of varying metabolic activity and thus extended his studies to other invertebrates. *"I had four different kinds of hemocyanin: lobster, horseshoe crab, squid, and snail. And each of these had this common ratio [O<sub>2</sub>/Cu ratio] but the physical chemistry of their combining with oxygen was quite different in the different cases. Some were designed to operate at low oxygen pressure and others at high oxygen pressures which related more or less to the activity of the animal. The squid for example, had a hemocyanin which required a lot of oxygen to oxygenate it, but it would deliver it at high pressure. A very active organism, you see. Whereas a horseshoe crab would absorb oxygen at a very low pressure when it was down in the mud, but it never delivered it at a very high pressure, the partial pressure of gas that determined how fast it would move into the cell, how fast it could supply for the metabolism of the cell."*

There was also work on the respiratory function of large marine animals – the porpoise, turtle, sea lions, as well as two earlier papers on humans. All told some 26 papers were produced on respiration – by far the largest proportion of his published work. His work lasted – his 1934 review on hemocyanin long remained a standard work.

At the turn of the 1920s there were to be a number of significant events in his life. In 1930 he became a senior biologist at the newly formed Woods Hole Oceanographic Institution, with Bigelow as its first director. Bigelow was a Harvard Professor and recruited Redfield along with Carl Rossby, Selman Waxman (later awarded the Nobel Prize for the discovery of streptomycin), Columbus Iselin, Mary Sears and Norris Rakestraw. Roger Revelle in his obituary of Redfield notes *"other than Bigelow, Iselin and Seiwel, none of the rest of the staff had much sea going experience, let alone oceanographic experience"*. Of Selman Waxman, I once read: *"Food was wasted on Selman at sea."* No need to read between the lines here, and this doubtless explains why his successes were in soil, rather than marine microbiology!

After a year as Associate Professor at Harvard, in 1931 Redfield was appointed Professor in the Biology Department, then promoted to Director in 1934 and directed the department until 1938. Prior to taking up his positions at Harvard and WHOI, the Redfield family had a sojourn in Europe, where Redfield took a sabbatical in Munich in the winter of

1931–32. This was followed by a visit to Cambridge in the spring.

At Harvard he brought to bear his broad vision of science (Redfield refers to himself as a naturalist) and was clearly intolerant of boundaries between the various aspects of science. He was instrumental in dissipating the compartmentalization he saw existing at Harvard at that time: *"...., when I moved back to Harvard, Biology was in a beautiful new building divided into three sections: one for zoology, one for botany, and one for physiology. And a student went into these, but there was nothing to bring out the inter-relationships between them, you see. And so my job [he was Head of Biology at the time] was to take part in combining them into a single department in biology. Where a student was not inhibited by any organizational lines from going across these gaps."*

The WHOI and Harvard post was a joint appointment and it saw Redfield move from marine biology to oceanography: *"Well, that was the next phase. As I got that wound up, I was under a certain rather intangible pressure to do work which was more specifically oceanographic. And I spent a winter making a survey of the Gulf of Maine, of the chemistry and physics of water."*

This shift from marine biology to oceanography saw the production of a remarkable series of papers, amongst them the seminal paper on nutrients. His major output comprises three papers on separate topics and a cluster of three papers on the control of zooplankton populations by water mass movement. With the exception of a paper on the processes controlling the concentrations of oxygen, phosphate and nitrate, he gives us little insight into how and why they came about. Viewed from an oceanographic perspective this was one of his most creative periods. Although his seminal 1958 paper in *American Scientist* was published outside this era (interestingly in his formal retirement), the seeds of the idea are to be found in the 1934 paper.

1938 sees a short paper with his then student, and subsequent long-standing collaborator, Bostwick "Buck" Ketchum on the mass (27 liters) culture of algae. It is still cited in mariculture as the first paper on mass algae culture. This work led on to two further papers with Ketchum on mass algae culture (published in 1949 and 1950). Following the 1938 paper with Ketchum, Redfield published three papers describing the distribution of zooplankton: *Limnacia*, chaetognaths and calanoids in the Gulf of Maine and the effect of water movements. These reports are regarded as landmark papers by contemporary fisheries scientists (*pers. com.* Michael Sinclair).

For me another landmark paper was that with Ketchum (plus Homer P. Smith as a third author): it is a detailed and beautiful analysis of the budget of inorganic and organic phosphorus in the Gulf of Maine. It is hard to think of a more well-rounded piece of work. It very likely inspired H.W. Harvey in the UK to look at the overall distribution of phosphorus within the planktonic community. It is Harvey's paper with Armstrong in 1950 that gave us the first real insight into the apportionment of organic material within various biotic and abiotic components of the plankton. They, like Redfield, were far ahead of the times in their thinking.

This remarkable exchange of ideas between the groups at WHOI and the Marine Biological Association (MBA) at

Plymouth in the UK contributed to Redfield's seminal work on the nitrogen to phosphorus ratios in the sea. Redfield was elected an honorary member of the MBA in 1956. The 1934 paper, published in the James Johnstone Memorial Volume, was the first of a chain of three papers (the 1958 paper in *American Scientist* and the 1963 paper in *The Sea*, Volume II were the subsequent ones). It is logical to consider them together, as one largely leads into the other. It was Harvey's 1926 paper, in the first case, which set Redfield thinking about the observed ratios and their significance. As far as I am aware there is no record that the two men met (The Plymouth MBA laboratory has kept detailed records of visitors and there is no record of Redfield visiting the Laboratory in the 1931 period, when he stayed at Cambridge (UK) on his way back from his sabbatical in Munich.). If they had met, I would have thought Redfield would have mentioned it, as Harvey also was an unforgettable individual. Redfield notes in his 1934 paper that: "In 1926 Harvey wrote: 'It is a remarkable fact that plant growth should be able to strip sea water of both nitrates and phosphates, and that in the English Channel the store of these nutrient salts, formed during autumn and winter, should be used up at about the same time.'" The relation noted by Harvey is thus of much wider application than he could have known at that time. It appears to mean that the relative quantities of nitrate and phosphate occurring in the oceans of the world are just those which are required for the composition of the animals and plants which live in the sea."

Redfield found the molar ratio of N:P in the North Atlantic to be 20:1. Using a procedure later referred to as the Apparent Oxygen Utilization (AOU), he calculated the molar  $O_2:N$  ratio as 6:1 (In Figure 2 in the 1934 paper, the oxygen units are not specified but can be worked out to be mmolar  $\times 10^{-2}$ , whereas nitrate concentrations are given in units of mmolar  $\times 10^{-3}$ ).

From pH measurements, he estimated the total  $CO_2$  concentrations and established a  $\Sigma CO_2:N$  ratio of 7:1. He points out that dividing the  $O_2:N$  ratio by the C:N ratio would give a value equivalent to the photosynthetic quotient. He realized that the value he obtained (0.86) was low and attributed it to errors in the C:N ratio. Given these ratios, he concluded that the proportions of C:N:P in organic material would be 140:20:1. He compared this with his and von Brand's analyses of plankton material, which matched well (137:18:1, expressed on a molar basis). By the time of the 1958 paper, Cooper's improvement of the phosphate analysis and the publication of Fleming's analysis of plankton, gave rise to a revision of these ratios to 105:15:1 for seawater and 106:16:1 for plankton.

In many respects the details are of lesser importance, what is of unquestionable importance is his observation:

*"That two compounds of such great importance in the synthesis of living matter are so exactly balanced in the marine environment is a unique fact and one which calls for some explanation, if it is not to be regarded as a mere coincidence. It is as though the seas had been created and populated with animals and plants and all of the nitrate and phosphate which the water contains had been derived from the decomposition of this original population."*

It would have been all too easy for Redfield to have left the matter there, as one of those irresolvable "chicken and egg" mysteries. It was probably his training as a physiologist that led him to try to puzzle it out. His experience with hormones, and his interactions with Cannon and Barcroft, would have brought him into contact with the concepts of the homeostatic mechanisms that exist in biology and it would appear that he projected these concepts to the whole ocean. In the 1934 paper, he starts by examining two possible explanations.

The first, which comes from Huntsman, that the N/P ratio in the oceans for some reason is, and that there is a range of N/P requirements within the algae and the population simply adjusts its biological composition to the prevailing chemical composition. This low level explanation of course would give no insight into how the N/P ratio was set up in the first case. The second explanation is very much his own and best explained in his own words. "Another explanation of the correspondence between the proportions of nitrate and phosphate in the sea and the composition of living matter may be sought in the activities of those bacteria which form nitrogenous compounds from atmospheric nitrogen or liberate nitrogen in the course of the decomposition of organic matter. In this case of the nitrogen-fixing bacterium, *Azotobacter*, it is known that for every unit of nitrogen fixed or assimilated and synthesized into microbial protein, about one half a unit of  $P_2O_5$  must be available. The physiological activity of such organisms must tend to bring the relative proportions of organic nitrogen and phosphorus toward the ratio in which these substances occur in the bacterial protoplasm. Comparable studies upon the physiology of the denitrifying bacteria do not appear to have been made. It is evident, however, that the composition of these organisms must be more or less fixed in regard to their relative phosphorus and nitrogen content and that when living in an environment containing an excess of nitrate, considered in relation to phosphate, the growth and assimilation of the organisms may continually tend to bring the proportions of nitrogen and phosphorus near to that characteristic of their own substances. It would appear inevitable



Photograph of Redfield taken about 1939 by C.E. Renn. Kindly provided by Redfield's daughter, Elizabeth Marsh.

that in a world populated by organisms of these two types the relative proportion of phosphate and nitrate must tend to approach that characteristic of protoplasm in general and that, given time enough and freedom from systematic disturbing influences, a relationship between phosphate and nitrate such as that observed to occur in the sea must inevitably have arisen. On this view the quantity of nitrate in the sea may be regulated by biological agencies and its absolute value determined by the quantity of phosphate present.” This is an altogether much more interesting explanation for there is an element of control. There is a problem with the explanation as there is nothing to redress high N/P ratios. Redfield was well aware of this and addresses the problem in his 1958 paper.

He concludes the 1934 paper with, “Whatever its explanation, the correspondence between the quantities of biologically available nitrogen and phosphorus in the sea and the proportions in which they are utilized by the plankton is a phenomenon of the greatest interest.” I don’t think anyone would dissent from that view.

He revisits the matter in his 1958 paper, but by which time he has rejected Huntsman’s suggestion. One can only speculate why: his (with Ketchum, 1949) and Fleming’s analyses of phytoplankton may have convinced him that the idea of a wide range of N:P ratios in algae was not sound. There are two methodological details to be noted in this later paper. First, the N:P ratio for the seawater analysis is now given as 15:1, reduced from 20:1 given in his earlier paper. This reflects the salt correction for phosphate measurements in seawater introduced by Cooper. Second, for the first time he outlines the procedure for calculating the oxygen consumption. There was a partial mention in a 1942 paper, although the first use of the term Apparent Oxygen Utilization and a full explanation of the procedure was left until the 1963 paper.

In the 1958 paper, Redfield essentially posed and explored answers to two questions surrounding concentrations of nitrate and phosphate in the oceans: first what set the ratio of the two elements, second what sets their maximum concentrations.

He puts three possible explanations to the first question. In his words, “In discussing the remarkable coincidence in the supply and demand for nitrogen and phosphorus it has been pointed out that it might arise from: (1) a coincidence dependent on the accidents of geochemical history; (2) adaptation on the part of the organisms; or (3) organic processes which tend in some way to control the proportions of these elements in the water.

Of the first alternative not much can be said except that the probability that the ratio in the sea be what it is rather than any other is obviously small. That the coincidence applies to the oxygen as well as to the nutrient elements compounds the improbability.

For the second alternative, it may be said that the phytoplankton do have some ability to vary their elementary composition when one element or another is deficient in the medium in which they grow. Such physiology might account for the coincidence in the nitrogen-phosphorus ratios. However, it is not evident how adaptation could determine the oxygen relation since this depends more on the quantity than the quality of the organic matter formed, and the oxygen requirement is felt only after the death of the living plant.

For these reasons the third alternative deserves serious consideration.”

Thus he discounts Huntsman’s explanation. He then goes on “Mechanisms should be examined by which organic processes may have

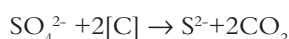
tended to control the proportions of phosphorus, nitrogen, and oxygen available for life in the sea.” That to me is a clear statement of the principle that a decade and a half later in 1974 would be formalized by James Lovelock and Lynn Margulis as the Gaia Hypothesis.

In “GAIA, a New Look at Life on Earth”, published in 1979, Lovelock defines the Gaia Hypothesis: “This postulates that the physical and chemical condition of the surface of the Earth, of the atmosphere, and of the oceans has been and is actively made fit and comfortable by the presence of life itself. This is in contrast to the conventional wisdom which held that life adapted to the planetary conditions as it and they evolved their separate ways. Although Lovelock includes the oceans in his definition no reference is made to Redfield’s work in the first edition of his book, which I have always felt to be a major omission. There is reference to Redfield in its sequel, “Ages of Gaia”, but it’s only a passing one. Although the concept of what is now termed geophysiology dates back to the writings of Vernadsky, possibly beyond, the proposition laid down in Redfield’s 1958 paper was the first clear example of a homeostatic control on a global scale and in many respects is still one of the most completely justified mechanisms.

Redfield’s explanation for the matching of the algae’s need for N and P in particular proportions and the presence of these elements in the near exact proportion in seawater is elegant in its simplicity. He argues in the minimum number of words that N/P ratios could be readjusted upwards by nitrogen fixation. It was very much a leap into the void as there was no experimental evidence for nitrogen fixation in the ocean at that point in time, or indeed for a number of years to come. He opted for the  $N_2$ -fixing bacterium *Azotobacter* as an agent, which was then the most likely candidate. Waxman, who was at WHOI at that point in time, was a soil microbiologist, and may well have influenced Redfield’s thinking. Evidence for the role of the cyanobacteria in  $N_2$ -fixation came two or so decades later. Most importantly Redfield realized this would be an unstable form of control and he writes, “The difficulty is rather in explaining why there is not a great excess of nitrate nitrogen in the water. The ratio of nitrogen to phosphorus in fresh waters is higher than that in ocean water, while the ratio in sedimentary rocks is very much lower. Consequently the ratio in the sea must tend to increase, unless some process is returning nitrogen to the atmosphere. Denitrifying bacteria might operate in this sense, in which case the phosphorus-nitrogen ratio is fixed by a complex balance.” Again this was a bold speculation as, although some anoxic zones in the ocean were known (the Black Sea, the Cariaco Trench and Kaoe Bay in the East Indies), they were marginal areas and, other than the Black Sea which is outside oceanic circulation, not large volumes. The understanding of the importance of denitrification in the much more extensive intermediate oxygen minimum zones was to come a decade or so later. Thus subsequent understanding was largely confirmed by Redfield’s speculations over the control of the N/P ratio in the oceans.

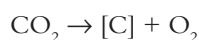
In his 1958 paper Redfield posed the second level question. His thesis above implied that phosphate concentration anchored the relative concentrations of nitrate and phosphorus; the question that then logically arose was what was the control on concentration of phosphate? Redfield found that the winter phosphate (and accordingly also nitrate) and the oxygen

contents of surface seawater fell very close to the predictions of the stoichiometric equation for respiration and photosynthesis. The oxygen content of surface water is controlled by the oxygen solubility (in turn set by temperature and salinity) and the oxygen content of the atmosphere. If it is assumed that temperature and salinity are two properties largely out of the control of the ocean biogeochemical cycle, then this suggests a link between the phosphate content of the oceans and that of the atmosphere. This led Redfield to ask the question we continue to ask, what set and controls the oxygen concentration in the atmosphere? Redfield opts for a two step process, which starts from sulfate reduction, in his format:



where  $[\text{C}]$  is his notation for organic material.

The carbon dioxide produced as a by-product of the reaction will contribute to that available for photosynthesis, oxygen being a product, i.e.:



Current thinking would suggest that although the second reaction is the source of free oxygen in the biosphere (of course the source strictly is water, not carbon dioxide), sulfur metabolism is a sink not a source for oxygen. Further it is difficult to see how, in the oceans where  $\Sigma\text{CO}_2$  is in some tenfold excess, the first of Redfield's reactions could give rise to net oxygen production.

These two comparatively short papers are followed by the 1963 *tour de force* published in *The Sea, Volume II* in which the stoichiometry and the dynamics are very thoroughly explored. By contrast the control mechanisms on the relative and absolute concentrations of nitrate and phosphate receive scant attention.

It seems to me that the genius, if that is the right word, of Redfield's work on the factors controlling the plant nutrients in the sea, is in the 1934 and 1958 papers. If it were genius, then where did it lie? Every step in the argument is in many respects commonplace and in themselves nothing remarkable. Redfield had some 20 years experience as a teacher and researcher in physiology, and thus the stoichiometric calculations of  $\text{CO}_2/\text{O}_2$  ratios would have been routine to him. What was it that makes those two papers seminal? Redfield summarizes his approach is the 1958 paper as follows: "*This conclusion [the ratios] permits one to approach the biochemical cycle in the sea in much the same way as a physiologist examines the general metabolism of an individual organism.*" In principle he was choosing to ignore all the geographical, physical, biological and biochemical complexities that exist in the open oceans and treat them as a single organism. The beauty is in its simplicity, the power in its robustness. There is a second proposition, that the biology is not only the prime controller of the fluxes of these compounds but also sets their relative and absolute concentrations to an optimum – a pure Gaian concept. The fact that Redfield probably elected the wrong explanation for two out of the three control mechanisms to me in no way diminishes the brilliance of his thinking. Often in science it is more important to ask the question than to answer it.

The stoichiometric equations are more formally laid out in the 1963 paper. Redfield assumes that 1 mole of  $\text{O}_2$  was consumed per mole of organic carbon, and that this was the determinant of the  $\text{C}:\text{O}_2$  ratio. This makes an implicit assumption that hydrogen and oxygen in organic material are in the same 2:1 ratio as they are in water. It of course makes no difference to the  $\text{C}:\text{N}:\text{P}$  ratios, as they are used to set up the basic equations in the first case, but it will materially affect the  $\text{CO}_2:\text{O}_2$  ratio. There have been revisions of the elemental composition of the model organic material over the past decade, with particular attention being given to the proportions of hydrogen and oxygen. These are known to depart from the simple 2:1 ratio, characteristic of carbohydrates. However, it is interesting to note that in one of the 1949 papers with Ketchum, they undertook an extensive set of analyses of the elemental composition, which included the tricky measurement of elemental oxygen and hydrogen, so they would have been very aware of the limitations of the simplifying assumption. If Redfield and Ketchum had felt there was any virtue in revising the elemental composition of the model organic compound, they had the information to do so. It's very unlikely to have been an oversight and we may suppose that they felt the simplification to be acceptable.

The second question a biographer will ask is what gave rise to Redfield's ideas: was it an inevitable consequence of the evolution of the subject, was it a product of the dynamics of the remarkable group of people who were the founding members of WHOI, or was it something special to Redfield? It seems to me that although there will have been elements of the first two, it is really about Redfield himself. The first two elements were also present in the very creative group at the MBA at Plymouth in the UK, who had pondered the same problem but never quite made the next bold step.

## THE WAR YEARS

The US entry into World War II bought a massive expansion at WHOI and it changed significantly the face of that institution. Redfield was appointed as a consultant to the US Navy Department's Bureau of Ships with the task of improving the effectiveness of antifouling paints. Aided again by his longstanding colleague Buck Ketchum, he threw himself into the work. It is interesting, but perhaps not surprising, that Harvey, his counterpart in the UK, also took up the study of antifouling during the war period. It is estimated that the consequence of the antifouling work was a 10% saving in fuel usage; that would represent not only a savings in money, but also seaman's lives. The findings of this work were eventually published in "*Marine Fouling and its Prevention*" edited and largely written by Redfield and Ketchum. This served for many decades, and still stands, as the reference book on marine fouling. Redfield made contributions to other defense-related matters. With Dean Bumpus and Alan Vine, he worked on the compressibility of submarines. He was also instrumental in passing on the oceanographer's understanding of density distributions to submariners. The latter took the form of a supplement to the Naval Sailing Instructions written specifically for submariners. This was, it would appear, welcomed by the

Navy, Redfield however did not welcome the eventual format in which it appeared: “Well, the Navy liked that idea and they turned it over to people at the Scripps Institution and they turned it over to people of the Walt Disney type, and they made [it] perfectly horrid... You know all full of vulgar cartoons. They assumed the Naval officers needed to be entertained before they would read anything. Well, these damn fellows were sticking their neck out to go to a place where they might very well be killed. And they wanted the information just as concisely as they could get it. I don’t know what’s become of the whole thing.”

## THE POST-WAR PERIOD

This period, of course, saw the completion of the trilogy on nutrient ratios: the 1963 paper some 7 years into Redfield’s retirement.

The success of the applied work during the war at WHOI and the post-war rise in the importance of submarine warfare had a major effect on the size and distribution of disciplines in the institution. Prior to the war there was something of an equal distribution between chemistry, biology, physics, meteorology and geology. Post war, physical oceanography had grown enormously.

Both Gordon Riley, in his provocative unpublished personal oceanographic history, and Redfield note the difficulty of funding biological oceanography in the post-war period. For biological oceanography, it was the doldrums between the pre-war period when ship time was largely free, and the large major national and international programs (such as the Indian Ocean Expedition, International Decade of Ocean Exploration, e.g. GEOSECS, MODE, CUEE, the Rings Program and others, leading eventually to JGOFS) that were to come in the 1960s and onwards.

Redfield generally took a positive slant on it: “I learned one thing from [work during war], particularly on the paint thing, and that was that it was pretty good fun to work on an applied problem. Because if you had an applied problem which couldn’t be solved by existing engineering principles, it meant that you didn’t know what the fundamental problems were.”



Redfield (c.1947) involved in what he tells us was a futile exercise—the plowing-in of clams on Barnstable mud flats. From the WHOI archive, reprinted with permission.

In his characteristic versatile manner, Redfield shifted his interest from the large scale fundamental topics, which were the hallmark of his science in the pre-war period, to issues of more local and applied interest. He worked on a scheme for a development of a clam fishery in Barnstable by the introduction of clams. It was not to be a success: “What we did on this clam business was, I think, to show what you shouldn’t put your money into. What we found was that the predators of the clams ate ‘em as fast as you put them there. We used to bring clams down from Boston and put them on the flats. And the horseshoe crabs and the snails would destroy them. Of course if there were enough clams some would survive. But we didn’t find that it was effective to transplant clams into Barnstable. It was practically impossible to keep these things [predators] out.”

He was involved in the successful opposition to the setting up of a plant for the processing of nuclear material “The Atomic Park” at Camp Edwards on Cape Cod. He also became involved as a consultant in the eutrophication of inshore waters and the discharge of chemical waste into coastal waters. On the more fundamental front, he undertook an interesting study on the distribution of deuterium in the oceans. This was complemented with samples from freshwater collected during a journey made in 1956 (the year of his retirement) across the US to the west coast. “The other thing which was perhaps of more general interest: I took occasion in a drive across the country in 1956 to spend the greater part of a winter at La Jolla, I took a whole case of bottles with me, and every time we went over a river, I lowered a bottle from a bridge. So I had a very good series of samples going across the northern part of the country and also coming back across the southern part. And supplementing that with other samples which Irving Friedman got from various sites, we got a pretty good picture of the distribution of deuterium in the continental United States.”

## RETIREMENT

Two major projects captivated much of his interest in this period. The first was the general evolution of salt marshes:

“Of course I’d known these marshes [the salt marsh at Barnstable] since I was a boy, gunning and what not. Thought I was an expert. Then I saw them again [when we did the work] with the clams and I was able to recognize one or two obvious changes which had occurred. And when I retired, I wanted something that I could do, which did not require having an organization behind me, did not require assistants, something I could do all myself, and at the pace which came naturally to me.

And so I started going over to Barnstable, walking around on these marshes, looking at interesting phenomena which I could study. That is I would look around and say, “Do I understand what is happening here or don’t I?” I had no problem, except dealing with something which was of interest.

There was a great deal of botanical work on marshes but very little geomorphology, geological work, or hydrographic work.”

This gave rise to five papers, culminating with a monograph in 1972 on the “Development of a New England salt marsh”. The monograph is still regarded as a seminal work on salt marshes. One of the

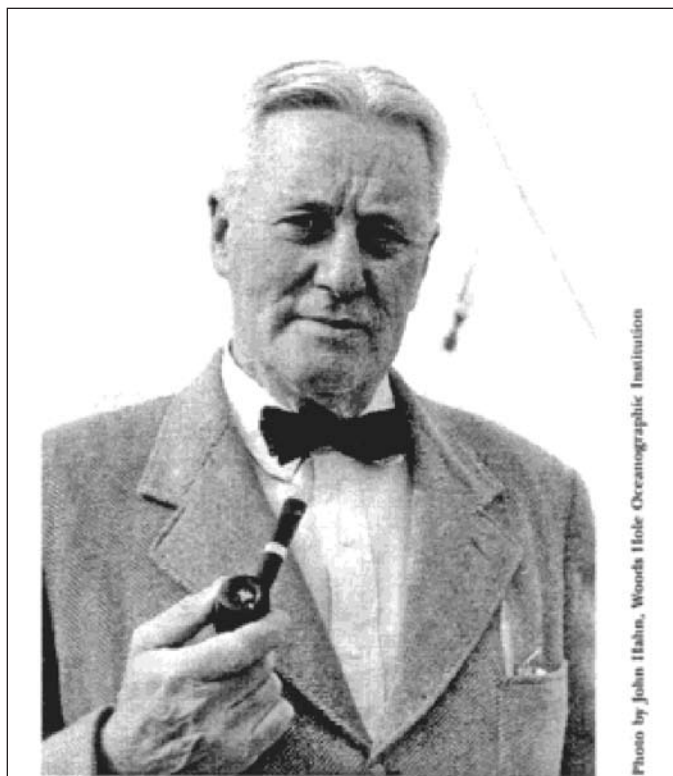
many outcomes of this work on salt marshes was Redfield's realization that the thermal insulation and isolation of salt marshes was such that they enabled the measurement of terrestrial heat flow. He discusses this with his daughter in her interview.

Elizabeth: "Could you detect the radiation of the Earth?"

"Yes. Quite surprisingly I found that it was there. Because if you took these measurements over a year, and averaged what was the average temperature at this depth, and this depth, the average temperature got higher and higher as you went down. And of course that required a great deal of work, took a long time.

So then I said to myself, if we treat this as a wave, which we've shown you can, and we make two measurements exactly six months apart, the average of those will be the average temperature. So I went back to another place where the marsh was deeper and I could get a greater range, and I went in, perhaps it was November, and made a careful study of the temperature with depth. I went back exactly six months later and did the same thing. You average them out and you get this straight line, which gave you the quantity of heat, the rate of heat diffusion, and from that you could get the quantity of heat which was coming out.

And it [heat flow] came out exactly the way you'd expect it, from the various methods you can use to measure the heat from the Earth in the off-lying ocean. You could do this thing which didn't take an elaborate ship going out and working in deep water, 'cause you can only do it where the water is essentially constant in depth and temperature. You can do it anywhere where you can find deep peat. And it took two days to do the work."



*Alfred Redfield*

Alfred Redfield, aged 65. From the WHOI archive, reprinted with permission.

"Does the heat flow differ in different places?"

"Yes, it differs a great deal throughout the world. Yes, I guess you could [do it in freshwater peat] if the water wasn't moving."

"That's very powerful. Do they use it much?"

"Nope."

The second topic that captivated his interest in this period stems from a lifelong interest in tides.

"Then when I got through all of that I thought: 'Well, now this is a very interesting coast [New England and New York]. There are a lot of intelligent people on it. It's a varied coast, and the tide is very varied and practically all the interesting tidal phenomena are exhibited. We have the highest tide in the world, and we have what are just about the lowest tides in the world, and so on, in these very varied situations.

And it seemed to me that there are very large numbers of people would be interested to know about this, yachtsmen and summer people and fishermen. A popular book. A popular book has got to make many statements which the reader must take for granted on your authority.

But I didn't think you should make those statements unless the experts would know what the authority was. But you can't give an expert a book which says "This is all so and so," and he hasn't any idea why you think so, do you see? You've got to give him something of the scientific argument.

So the book got a little more sophisticated and a good bit harder to write. But I've been working at it, and I'm now on the next to the last chapter, I think. And I don't know whether I'll ever get it done. Work more slowly every day, it seems to me. But I've got it all pretty well thought out."

He did finish it and it was published in 1980, when Redfield was 90 years of age.

## EPILOGUE

How can we sum up his achievements?

Everything else aside, a man who lives to the age of nearly 93 and is writing science close to the end, defines himself as exceptional. Redfield however was more than this. His work on the rationalization of the nutrient ratios was unquestionably seminal. One gets the feeling that we are more impressed by the concepts he put forward than he was. His comment in the interview runs: "The whole thing is in the paper [American Scientist paper]. It's been on the whole well received; those who didn't like it haven't said so. And I think that's my greatest claim to fame, that idea." It is unlikely to be false modesty, as that would not appear to be Redfield's style, however, he does recognize that that it was probably his most important idea: "Well, that's probably the most important idea I've ever had, the fixed ratios. I think that's here to stay." The brilliance in the two early papers was not the fixed ratio, but the homeostatic control mechanisms and this does not appear to be rated highly by Redfield. An interesting insight comes up in an answer to a subsequent question by Elizabeth.

"Did you feel you influenced [the ecologist] Odum?"

"I don't know. You'd have to ask him. You know, I got to thinking about the ecology of the ocean a few years before the war when I was asked to give some Lowell Institute lectures, and I have them all written in various stages in my filing cabinet. And the final stage was submitted to the Harvard University Press and they said, yes, they would publish it if it was put into suitable form. It obviously needed

going over again. And the war came and I got busy with other things. And before I got back to it, Odum's book came out. And I felt that he had wiped [opened] my eye. That he had grasped the same general ideas, but there wasn't really a place. I told him so. I told him I thought it was wonderful."

By good fortune, the text he refers to, which runs for some 200 typed pages, has been preserved in the Woods Hole Archives. It's incomplete, as there are no accompanying figures, but nonetheless, it makes interesting, although not easy, reading. Many of the concepts seem to me very advanced for the time, the nutrient ratio story is, of course, spelt out, as are a number of other ecological principles, but there is no specific development of his ideas of the control mechanisms. It may be that he did not regard them as especially interesting or perhaps he felt that the evidence for them was so tenuous that they should not be set in stone, as it were, in a text book. I have to leave this as something of a puzzle.

The thing that perhaps strikes me most reading his scientific career is the breadth of his interest, and more particularly, of his competence. He made contributions in the fields of whole organism physiology, and radiation physics. He laid the basis of ecological stoichiometry, essentially anticipated the Gaia hypothesis by a decade and a half, produced the first rigorous analysis of the organic cycle, undertook landmark work on zooplankton biogeography, worked on tides, and quaternary geology. When Redfield referred to himself as a naturalist, one can be sure he meant this in the rigorous sense. Sadly the concept of a naturalist has been dumbed down to the gum-booted generalist with no particular specialty. Redfield was the complete antithesis of this – without question he had a sound grasp of the full spectrum of the sciences he embraced.

I would like to finish with something of a teaser. In the interview with Elizabeth, he discusses an applied project that dealt with the discharge of waste from titanium mining.

"Well, then at this time [the early 1950s] the *Oceanographic* was very poor, at least so far as the biologicals. The Navy was continuing support for the hydrographic work. But there was no support for biology. And what we did was to find that support for studies, essentially in the interest of pollution. It was a problem.

I think the first one of these was a contract with the National Lead Company who was mining titanium in the Adirondack Mountains. Titanium is a white pigment which is very white and used in bathrooms to make it pretty. Used in other ways, too, of course. And this ore is brought down to the Raritan River below New Brunswick [New Jersey], where it is extracted.

And the process produced large amounts of sulphuric acid and iron, iron being a component of the ore and sulphuric acid used in the extraction process. And the result was that there was a great quantity of ferric sulphate produced."

The pollution of the Raritan River and Raritan Bay by industrial wastes and domestic sewage led to an order restraining the National Lead Company from continuing to discharge certain wastes from its titanium plant at Sayreville into Raritan River. They proposed to barge the material out to sea. The company had secured permission from the Captain of the Port of New York to discharge the waste ten miles off the New Jersey coast. Serious opposition to the proposal developed on the part of the commercial and sport fishing interests, who felt that the operations would

greatly interfere with their activities. The National Lead Company requested the National Research Council to sponsor an independent investigation of the operation and its consequences. The Council contracted the Fish and Wildlife Service and WHOI to investigate and report on the problem.

Redfield explains "*And we studied, in the first place, what would happen if you put so many thousands of gallons in the sea in the wake of a barge. The thing was that the elements being put in were iron and sulphur. Sulphur is, I guess, next to sodium and chloride, the most abundant element in the sea. Iron, I guess, is not present in large quantities, but is an important nutrient. We couldn't see any harm whatsoever. We actually estimated what the concentrations would be behind the barge as it spread out in a liquid form. So that was all favorable.*" He then goes on to observe: "*What happened was this came out as a green liquid which was dissolved iron sulphate, ferrous iron. When it hit the water it was precipitated as ferric, which is iron rust, so the trail of this boat was bright red! Very visible. And I said, "Oh my, you're still in trouble."*

*I subsequently flew over the area on my way somewhere in an airplane. And I could look down and see [the barge with] this great red trail out behind."*

The fascinating thing is the passing comment by Redfield: "*But then it eventuated that this was the best place to catch fish! The boys came back and found they could catch more fish in what they called the "acid ground" than anywhere else!"*

It is all too tempting to conclude that Redfield had unknowingly aided and abetted the first iron enrichment experiment. I am well aware it is fanciful but it is, however, a thought worth savoring and perhaps we can allow ourselves this indulgence. However my impression is that Redfield himself would not altogether approve of such lack of rigor.

So, what do we make of this remarkable man? I don't think one can find a more fitting end to Alfred Redfield's biography than to cite again the beautiful piece of verse by R. Lee Sharpe, Dean Bumpus used in an appreciation of Redfield at his memorial service:

*Isn't it strange how princes and kings,  
and clowns that caper in sawdust rings,  
and common people, like you and me,  
are builders for eternity?*

*Each is given a list of rules;  
a shapeless mass; a bag of tools.  
And each must fashion, ere life is flown,  
a stumbling block, or a stepping-stone*

Without doubt Redfield left us with important stepping stones that has enabled us to convert a shapeless mass of chemical observations into an understanding of the biogeochemistry of the oceans

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## ASLO NEWS

### MESSAGE FROM THE PRESIDENT

**Sybil P. Seitzinger**, Rutgers University, Institute of Marine and Coastal Sciences, Rutgers/NOAA CMER Program, 71 Dudley Rd, New Brunswick, NJ, USA, 08901; [president@aslo.org](mailto:president@aslo.org)



#### AWARDS

ASLO Awards recognize outstanding achievement in the aquatic sciences by our early, mid and later career scientists.

I would like to thank everyone who participated in the process this year, including all those who prepared and submitted nomination packages and the

award subcommittee members who had the difficult task of selecting the final awardees from among the slate of excellent nominees. The awards will be officially presented at the ASLO Aquatic Sciences meeting in Santa Fe in February, at which time the award recipients will give a talk discussing the science that lead up to their award as well as their vision of the way forward. Please join me in congratulating this year's ASLO award recipients.

### ADVANCING THE SCIENCE OF LIMNOLOGY AND OCEANOGRAPHY AROUND THE WORLD

Participation in ASLO of scientists from outside of North America continues to expand. In the past decade our European membership has increased markedly. Our first meeting outside of North America was only a little over six years ago (2000) in Copenhagen; last year we met in Spain. Both facilitated additional participation of European limnologists and oceanographers in ASLO. For example, not only did many Europeans become new members, but the number of submissions to L&O from European scientists showed a considerable increase following the meeting in Spain. The next meeting of ASLO outside of North America will be in Nice, France, in January 2009. Our first ASLO President from outside North America, Carlos Duarte (currently President-elect), will take office in July 2008.

How can ASLO more effectively engage the leading limnologists and oceanographers in Asia, Latin America, Africa and Australia? Currently, we have a total of 448 ASLO members and 118 institutional subscriptions to L&O and L&O:Methods from these regions. India alone has 5 institutional subscriptions to ASLO journals, including the National Institute of Oceanography in Goa which I recently visited (see photo above). The ASLO Board is exploring ways to enhance participation of scientists from these regions, including increasing the number of journal subscriptions and exploring potential future meeting venues.

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## ASK AN ETHICIST COLUMN

The ASLO Professional Ethics Committee launched a new feature, "Ask the ASLO Ethicist," in the last issue of the *Bulletin*. This feature will continue in the spring issue of the *Bulletin*. Members are encouraged to write to the committee (anonymously; [ethicscom@aslo.org](mailto:ethicscom@aslo.org)) regarding an ethical issue of concern; the issue could be one that you are facing or simply one that you know repeatedly arises (e.g., authorship on publications). The committee will then research the issue and prepare a report for the *Bulletin*, incorporating all relevant points of view. The articles also will be posted on the ASLO website under a Members Only page for future reference as Professional Ethics FAQs.

## WEB-ACCESSIBLE CLASSROOM LECTURES, BOOKS, AND PLENARY TALKS

ASLO continues to seek ways to enhance the accessibility of new information to our members and to the scientific community at large. Of course, all our journals are available electronically, and the ASLO website is accessed well over 1 million times a year for journal access, announcements of meetings, job opportunities, student activities, policy updates, images, and much more. A new benefit for ASLO members, ASLO Web Lectures, will be launched soon. The first set of lectures will be available this winter. Topics slated for inclusion are: molecular tools for a non-molecular audience, ecological stoichiometry, use of lake sediment records to understand long-term environmental change, metal bioaccumulation and toxicity, among others. This peer-reviewed lecture material has been developed by experts in these areas, and will include graphical presentation

as well as explanatory text. In addition, the Board is considering having plenary talks kept on digital videotape for download and viewing from the ASLO website.

This past summer the ASLO Board approved a motion to consider proposals for web-based book publishing (see last issue of the *Bulletin*). A proposal has now been submitted to and approved by the ASLO board for the first such ASLO web-based book. "Marine Virology Methods", edited by Curtis Suttle, Markus Weinbauer and Steven Wilhelm, will be housed on the ASLO website.

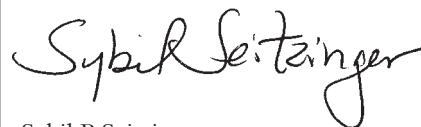
The electronic format of these new ASLO web-based products has many advantages. They will have wide and rapid availability, they offer the potential for use of enhanced visualization tools such as video clips and dynamic models, and direct links to additional material and cited references, among others.

## ASLO WEBSITE RECOGNIZED FOR ITS HIGH-QUALITY SCHOLARLY CONTENT

The ASLO website now has a link from ISI Web of KnowledgeSM and Current Contents Connect®. This is the result of Thomson Scientific selecting the ASLO web site for inclusion in their Current Web Contents™, which is a list of scholarly research-oriented Web sites that have passed stringent and objective content selection standards. Searchers will be able to review a short description of the Web site before linking to it, and be assured of its authenticity, quality, and timeliness of data. High praise goes to Paul Kemp and Sue Feng who are constantly enhancing and maintaining the ASLO website and to everyone who contributes the high quality of material that is contained therein.

## COOPERATION WITH OTHER ORGANIZATIONS

ASLO has signed a Memorandum of Cooperative (MoC) to become an affiliate of the International Council of Zoological Nomenclature (ICZN). This does not require any financial commitment from ASLO; rather it is an endorsement of the important work that ICZN does in producing and periodically revising the International Code of Zoological Nomenclature, and in ruling on cases where animal nomenclature requires resolution.



Sybil P. Seitzinger  
Rutgers University,  
Institute of Marine  
and Coastal Sciences



Sybil with Wajih Naqvi at NIO in Goa, India

## MESSAGE FROM THE BUSINESS OFFICE

*Helen Schneider Lemay, ASLO Business Office, 5400 Bosque Blvd., Suite 680, Waco, TX 76710-4446; Tel.: 254-399-9635 or 800-929-2756, Fax: 254-776-3767; [business@also.org](mailto:business@also.org)*



Have you renewed your ASLO membership? The ASLO membership year runs from January 1 to December 31, so now is the time to renew. We encourage you to renew online. This saves you time and also the society in not having to print and mail notices. This, in turn, allows ASLO to keep your dues as low as possible while

continuing to offer you benefits from society membership. (There is no dues increase this year!). To renew, logon through the Renew Membership option of the ASLO website or wait for your printed form to reach you.

ASLO elections will be coming up this spring and one of your membership benefits is voting for society officers and board members. Watch for information and be sure to vote. As a result of last year's elections, Carlos Duarte, ASLO president-elect, will be our first non-North American president! Remember, ASLO is now the society that is "advancing the science of limnology and oceanography."

The business office is also involved in organizing the ASLO meetings and there is lots going on. In February, WATER ROCKS will be the theme for the 2007 Aquatic Sciences Meeting in Santa Fe ([www.aslo.org/santafe2007](http://www.aslo.org/santafe2007)). There's still time to register – and sign up your bowling team! Be sure to check out the program on the conference website. This meeting has many new and exciting events, including an art exhibition and band formed with ASLO members.

The next Ocean Sciences Meeting will be organized by ASLO and co-sponsored by AGU and TOS. It will be held in Orlando, Florida, in February 2008. The convention center is wonderful and Orlando has a lot to offer that you might not even know about. Other meetings in the works are the ASLO Summer Meeting in 2008 in St. John's, Newfoundland, Canada, and the 2009 Aquatic Sciences Meeting in Nice, France.

Lots of other interesting things are happening in publications, committees, public policy, etc., so visit the website and keep in touch.

Please let the Business Office know how we can be of service!

Regards,



Helen Schneider Lemay, ASLO Business Office

## 2007 ASLO AWARDS

### ALFRED C. REDFIELD LIFETIME ACHIEVEMENT AWARD TO JÖRG IMBERGER

*Cited by David Hamilton, University of Waikato, New Zealand*



Jörg Imberger has dedicated his career to understanding the transport and motion of water and how this understanding can be scaled to whole systems, particularly in relation to the dynamics of lakes. He has made an outstanding and sustained contribution to physical limnology over a period of 35 years, commencing initially with fundamental

investigations into selective withdrawal from stratified fluids and extending to more holistic understanding of ecosystems and sustainability.

Jörg graduated with a Ph.D. from the University of California, Berkeley, in 1970, with his dissertation work with Hugo Fisher still forming a primary scientific basis for the design of structures and reservoir operations that provide important water quality benefits throughout the world. He then held a postdoctoral appointment at the California Institute of Technology, followed by a faculty position at Berkeley. Jörg was appointed to full Professor at the University of Western Australia in 1978 at the age of 36, at that time the youngest full professor in Australia in the field of engineering.

Jörg has used a combination of fundamental observations, theory and modeling to define the field of physical limnology. Key papers still provide essential references on the subject and include:

- His 1980 Journal of Physical Oceanography paper with former Ph.D. student Bob Spigel, which provided new insights into the role of wind and basin shape in lake mixing processes;
- His 1985 L&O paper on the diurnal mixed layer, which provided the basis of future developments of hydrodynamic models such as DYRESM, which was instrumented through Jörg's innovative computational approaches, and
- Arguably *the reference* on the subject of physical limnology in his 1990 paper in Advances in Applied Mechanics with colleague John Patterson.

Jörg's research and insights, as well as the models that he has developed, have led to improvements in the quality of many waterbodies, which have had benefits to a large number of communities around the world. Jörg has undertaken major collaborative research projects on lakes, estuaries and coastal areas in many countries, and students and colleagues who have worked alongside Jörg on these projects will know of his inexhaustible drive to understand these systems, to collect data on them and to pursue scientific excellence. Jörg has been recognized internationally for these contributions, and included amongst his many awards are the Stockholm Water Prize from the King of Sweden in 1996, for his contribution to Environmental Fluid Dynamics, and the Onassis Prize

from the President of Greece in the category “Man and his Environment”.

Given Jörg’s international sphere of influence, it is important not to overlook his influence locally. Jörg played the primary role in establishing the first Department of Environmental Engineering in Australia in 1992, built on a premise that all engineers needed to incorporate elements of design and sustainability in all of their functions. Graduates from this rigorous undergraduate and graduate program are widely acclaimed by industry and in academia, while graduate students and post-docs advised and mentored by Jörg now provide a new generation of internationally renowned researchers in environmental fluid dynamics.

Jörg’s insights continue to influence the way that limnology is conducted today and his innovation and dedication make him a truly worthy recipient of the Redfield Award for Lifetime Achievement.

### **RUTH PATRICK AWARD FOR ENVIRONMENTAL PROBLEM SOLVING TO GEORGE KLING**

*Cited by Anne Giblin and John E. Hobbie, Marine Biological Laboratory, Woods Hole, Massachusetts, USA*



Throughout his career Prof. George Kling has used excellent science to make a sustained contribution to solving environmental problems in three areas: Degassing of Cameroon lakes, regional carbon balance in the Arctic, and lake responses to environmental perturbation. George Kling’s strong research and diligent and sustained leadership in the implementation of a solution to this serious life-threatening problem at Lake Nyos stand out as exemplifying the combination of excellence in science and substantive and direct engagement with environmental problem solving that this award is intended to recognize. In addition, we value George’s effort in bringing scientific expertise to Cameroon and appreciate the challenges that were overcome in finding sustainable solutions for that region.

George was part of the first team of scientists who went to Cameroon after the 1986 catastrophic degassing of Lake Nyos when 1700 people were killed when toxic gases were suddenly released from the lake. At the time he was still a graduate student working on Cameroon lakes as part of his thesis. In spite of his junior status, he soon became a leader and was the senior author on the group’s finding. Subsequently, George and co-workers continued to study the problem and determined that the lakes in the region were again recharging so that another event could occur. In spite of government warnings, the valleys of these lakes were being re-populated so another degassing event would again result in a tragically large loss of life. He realized that science alone would not solve the problem and helped keep the issue alive by giving numerous TV and radio interviews and was quoted in dozens of newspaper and magazine articles.

Prof. Kling became chair of an international advisory group to examine ways to safely degas the lakes. Eventually, the first

permanent degassing column was assembled and set up in January 2001 at Lake Nyos by a French team with pipes that lift deep water to the surface to create a soda fountain. The main part of the funding came from the U.S. Office of Foreign Disaster Assistance with contributions from the French Embassy in Cameroon and the Cameroonian Government participated in the financial and logistical support. Prof. Kling continues to study the problem and continues to push for additional pipes to further reduce the potential for disaster.

His work is extraordinary because he didn’t stop once he had identified the problem, but continues to take an active role in the solution. Therefore, the citation for the Ruth Patrick Award reads “George Kling has made a major contribution to our understanding of catastrophic lake degassing in Cameroon and for the last 20 years has tirelessly worked to implement the political and engineering solutions necessary to prevent a repetition of this catastrophe.”

### **G. EVELYN HUTCHINSON AWARD FOR A MID-CAREER SCIENTIST TO JOHN P. SMOL**

*Cited by Brian F. Cumming, Queen’s University, Kingston, Ontario, Canada*



The G. Evelyn Hutchinson Award is given to an aquatic scientist who has excelled, following the traditions set down by Hutchinson himself, in holistic research. An outstanding researcher, mentor and ambassador for science, John P. Smol is a truly exceptional recipient for the *G. Evelyn Hutchinson Award*. Few could rival his record in research, teaching, service, and public outreach. John received his Ph.D. in 1982. Since 1980, he has authored over 320 journal papers and chapters (including 10 in *Science* and *Nature*, and 3 in *PNAS*) dealing with a suite of topics on limnology, paleolimnology, and global environmental change. He has also completed 15 books – almost all in multiple printings – and another book nearing completion. In addition, his recent and highly praised textbook: *Pollution of Lakes and Rivers: A Paleoenvironmental Perspective* had sold out so quickly, he is now rapidly writing the 2nd edition. Four books that he has recently edited summarize the myriad of new approaches available to paleolimnologists. He has authored almost 600 conference presentations; many of these were invited and several were opening keynote addresses. John is listed as an *ISI Highly Cited Researcher*, one of only a handful of limnologists (and the only paleolimnologist) to reach this designation.

John was instrumental in moving paleolimnology from a largely descriptive science to a quantitative and precise science. He has played the pre-eminent role at melding paleoecological techniques with limnological studies. His work encompasses many aspects of fundamental and applied limnology. He founded and now co-directs the *Paleoecological Environmental Assessment and Research Lab – PEARL*, with over 30 researchers, consisting of students and research associates, devoting their efforts to studying the history of lake environments. His lab is not only world-renowned for its outstanding research excellence, but also for its high morale and contagious enthusiasm.

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As noted by Dan Livingstone “he does this in an easy, friendly way while maintaining a spirit of camaraderie that is the envy of all who visit his lab”. John’s team works on projects that range from the description of new bioindicators of environmental change, to the study of lake trajectories in the Arctic, to problems such as lake acidification and eutrophication, to studies of climatic change and UV penetration, to developing paleolimnological techniques to track salmon and bird populations, to working with archaeologists to decipher how past cultures have affected freshwater ecosystems. He has pursued an integrated approach, using several sub-disciplines (as reflected in his eclectic list of awards, from botany to geology to limnology to environmental issues), and multiple lines of evidence to interpret the paleolimnological histories of lakes. Amongst John’s greatest achievements, though, would include his massive work on the limnology and paleolimnology of Arctic regions, and he has now completed over 20 high Arctic field seasons.

John’s pre-eminence in science is recognized world-wide. Nothing can speak more strongly about the esteem in which he is held by his colleagues than the fact that, at the age of 30, he was asked to be founding editor (and continues on as co-editor-in chief with W. Last) of the *Journal of Paleolimnology*. This journal is now celebrating 20 years of publication. Some of the success of this international journal (currently ranked 2nd of the 17 limnology journals ranked by ISI for impact factors, and 1st of 17 in the immediacy factor) results from John’s continued stewardship as editor. John is now also Editor-in-Chief of a second journal: *Environmental Reviews*. In addition, he is involved with the editing of books (e.g., series co-editor of the new book series “*Developments in Paleoenvironmental Research*”, which currently has 13 titles) and is or has been on the editorial boards of a number of other journals (e.g., *Limnology and Oceanography - Methods*, *J. Phycology*, *Int. J. Salt Lake Research*, *J. Limnology*, etc.). He has recently enthusiastically agreed to co-edit an *L&O* special issue on climate change. In addition, he is a frequent organizer of conference sessions, including several at ASLO, as well on the scientific advisory committee for the upcoming SIL conference. John has served or serves on a large number of panels and review committees, which are simply too numerous to mention.

John has been awarded over 25 medals, awards, and fellowships since 1990. For example, John’s pre-eminence in science has been recognized with the award (1990) of the *E.W.R. Steacie Memorial Fellowship*, the Natural Sciences and Engineering Research Council’s highest award for young scientists or engineers. In 1991 he received a citation for “...outstanding contributions ...” to the U.S. National Acid Precipitation Program (NAPAP). The Botanical Society of America awarded John their 1992 *Darbaker Prize* for his work on algae. In 1993, he was presented with the *North American Lake Management Society (NALMS) Research Award* for “outstanding research in lake restoration, protection and management”. In 1993 he was also awarded the National Research Council’s *Steacie Prize*, as Canada’s most outstanding young scientist. John was elected a Fellow of the Arctic Institute of North America in 1993. In 1994 he was presented with the *Queen’s University Prize for Excellence in Research*. He has chosen

by the Canadian Society of Limnologists as the 1995 *Rigler Prize* winner, and the Canada Council awarded him a *Killam Fellowship* (1995–1997). In 1996, he was elected a Fellow of the Royal Society of Canada, Academy of Sciences. In 1997, he was presented with the *University of Helsinki Award Medal*, representing the field of physical geography, as well as the Geological Association of Canada’s (GAC) *Past-Presidents’ Medal*, for outstanding contributions to the geosciences. The GAC also elected him as one of their few *Distinguished Fellows*. In 1999, he was presented with the *Turku Academia Medal*. In 2000, he was named the *Canada Research Chair in Environmental Change*, and in 2001 he was presented with the Royal Society of Canada’s *Miroslav Romanowski Medal* (environment medal), and in 2002 with an *Ontario Researcher of Distinction* award. In June 2003, St Francis Xavier University conferred an *honorary Doctor of Laws (LLD)* degree on John for “...for his contributions to lake ecology and understanding the underlying causes of environmental change...”. In 2003, he was awarded one of three *NSERC Award for Excellence* prizes, and in December 2004 he was awarded Canada’s highest scientific and engineering honor: the *Gerhard Herzberg Canada Gold Medal for Science and Engineering* “...for his efforts in bringing paleolimnology to world attention, and for discoveries, innovative techniques and research protocols that are influencing public policy on issues related to climate change, water pollution and the protection of the Arctic environment”. Last summer, the Canadian Quaternary Association of the Geological Association of Canada presented John with the *W.A. Johnston Medal*, which is the life-time achievement award offered by CANQUA for professional excellence in Quaternary research for his work on paleolimnology. In May 2007, the University of Helsinki will bestow on John the degree *PhD (honoris causa)* for his outstanding contributions to aquatic sciences.

One must add that, in addition to his research prowess, John is an excellent and inspirational teacher and mentor. Perhaps the most telling evidence of his talents as a mentor is the continued and steady success of his graduates at securing excellent academic jobs. His dedication to his graduate students and post docs (he has mentored over 70 of them, and without a single withdrawal!) is legendary. This fall, John was the recipient of the inaugural *Award for Excellence in Graduate Supervision*; a new award recognizing excellence in graduate student supervision. Meanwhile, his undergraduate courses are often over-subscribed, and he was twice awarded the Biology Department’s *Best Professor Award*. In March 2000, he was awarded the *W.J. Barnes Teaching Excellence Award*, Arts and Science Undergraduate Society, which is “the highest tribute the Society can pay to an individual for teaching excellence”. Completing a large sweep of teaching awards, John was also the recipient of the 2006 *Chancellor A. Charles Baillie Teaching Award*, perhaps the university’s highest award for education. His teaching, however, goes beyond Queen’s boundaries. For example, he is frequently a visiting professor or guest lecturer at many universities. He is well known in political circles as a never tiring advocate for the environment and the importance of scientific research to reach sound policy decisions. Rarely does a week go by that John is not being interviewed by the me-

dia. He has worked tirelessly for a number of aquatic societies. John also frequently volunteers his time to local environmental groups, as well as public education.

As evidenced by the impressive range of his publications, awards, and other accomplishments, John Smol exemplifies excellence in scholarly limnological research. Perhaps John was best summarized by David Schindler, one of his nominators, who concluded his letter by saying “John is simply a fine human being, who always has time to discuss problems of others. No task is too large or too small for him. He is one of the most generous individuals I know, regardless of discipline”.

This is a remarkable career record for any scientist. However, the award holds special significance to John, as he is an “academic grandson” to the Hutchinson legacy, as his Ph.D. supervisor was S.R. (Ted) Brown, one of Hutchinson’s students. John is committed to demonstrating the importance of sound scientific principles when it comes to addressing such critically important issues as water quality and ecosystem management. We can only wonder what else he has in store for his future research, teaching, and outreach activities.

#### **LINDEMAN AWARD FOR THE OUTSTANDING PAPER IN AQUATIC SCIENCE BY A YOUNG SCIENTIST TO KELLY M. DORGAN**

*Cited by Peter A. Jumars, School of Marine Sciences, University of Maine, Walpole, Maine, USA*

DORGAN, K.M., P.A. JUMARS, B. JOHNSON, B.P. BOUDREAU, and E. LANDIS. 2005. Burrowing by crack propagation through muddy sediment. *Nature* 433: 475.



Kelly Dorgan anticipates obtaining her Ph.D. in 2007 from the School of Marine Sciences of the University of Maine. Dorgan’s research addresses the mechanics of burrowing in marine sediments, with an eye toward sedimentary consequences as well as functional morphology. The significance of her work can best be ap-

preciated by comparing current understanding of swimming mechanics to those of burrowing. No serious analysis of animal swimming would omit critical properties of the fluid, namely dynamic viscosity and fluid density. Yet, paradoxically, no prior study of burrowing had incorporated relevant physical properties of the medium in analysis of the mechanisms and costs of burrowing. When she began her work it was not even evident what physical parameters were most relevant. The problem was “clear as mud.” How animals moved through soils was an issue that troubled Darwin in his lesser-known book on worms (reviewed in 2006 by Filip Meysman in *Trends in Ecology and Evolution*), and Darwin hypothesized that the only way that this difficult feat could be accomplished was by literally eating through the medium.

The issue is important in several ways. From a physiological standpoint, earlier studies had led to the conclusion that burrowing is the most energetically expensive means of animal locomotion per body length moved. Consequences of burrow-

ing are many. It is not yet clear, for example, to what extent feeding and burrowing, respectively, contribute to the process known as bioturbation that literally gates the burial of organic matter as well as pollutants.

I’ve tried to get several students started on this problem before, but the time was finally right. Bruce Johnson and Bernie Boudreau with their coworkers had recently discovered that bubbles move through sediments by propagating cracks using only the forces of their buoyancy and surface tension. Simple linear elastic theory of crack propagation pointed to two parameters as key: Young’s modulus (stress to produce a given strain) and the stress intensity at which the material would fail (resulting in a crack). Moreover, these parameters also were sufficient to predict the shape of the crack. The forces involved were modest and appeared well within the realm of animal capabilities.

The problem Dorgan had to solve was how to analyze forces in the geometrically complicated setting of a burrowing animal. Dorgan started with the same approach that Johnson and Boudreau had used, employing seawater-gelatin as a sediment mimic that allowed visualization. She added another step, however, by applying a technique called photoelasticity that uses polarized light to locate and quantify the stress fields produced in birefringent, transparent solids. After calibration, this method allows direct estimation of the forces of burrowing. The results show the mechanical costs of burrowing to be over an order of magnitude lower than previously thought. Cracks in mud propagate relatively easily, just like the ones in automobile windshields.

The time to approach this problem also is right from several other perspectives. Nanotechnology has greatly improved the art of making artificial materials with novel mechanical properties, and Dorgan is using its recipes to make better mechanical mimics of mud. Engineering of complex materials has also greatly advanced through numerical modeling of complex composites (e.g., the literally concrete example). Dorgan has taken the engineering courses that allow her to apply these approaches to the burrowing problem.

An immediate consequence of the results published in this “ground-breaking” paper was a revolution in understanding the morphologies of burrowing animals. Clams are wedges. A mud-burrowing amphipod is shaped just like half of an oblate spheroidal bubble with legs; the carapace is driven by the legs to form the crack. Subterminal expansions of worms exert lateral or radial forces that propagate cracks axially. The insights garnered from an understanding of mud mechanics quickly produced an entire review paper on burrowing (Dorgan et al. 2006 in *Oceanography and Marine Biology: An Annual Review*). Analogous mechanics appear to govern root growth, including the making of cracks in the mortar of ivy-covered walls.

Like any good solution to long-standing scientific problems, this new understanding of burrowing raises further questions and hypotheses. One that Dorgan is including within her Ph.D. work is how the biomechanics of burrowing and the exerted forces change with animal morphology and size. We also know that simple behavior of sediments as ideal solids breaks down on even fairly short time scales due to creep (incomplete elastic

rebound) and frictional dissipation of elastic stored energy, and Dorgan is incorporating some experimental work along these lines as well. In general, the interaction of animal movements and sediment responses need new parameterizations in more accurate models of bioturbation.

Dorgan's work has received other rapid recognition, some of it more than a little unusual. *Popular Science* magazine chose her in its October 2006 issue as one of its "Fifth Annual Brilliant 10," giving her the unenviable nickname of "Worm Whisperer." Fans of intelligent design immediately seized upon her refutation of Darwin's hypothesis of burrowing mechanism to challenge Darwin's credibility in his better known theory of evolution. The resulting controversy, involving a newly elected member of the Ohio State Board of Education, is chronicled in the Panda's Thumb blog. Less seriously, the work is a punster's dream: Worms go better with Jell-O; Worms make wise cracks.

How Dorgan got so far so fast is certainly due in major ways to her abilities and efforts, but they have been well supported by a succession of mentors and practical experiences. She became interested in marine science as a high school intern working with Linda Schaffner at Virginia Institute of Marine Sciences (VIMS) on the effects of disturbance on benthic communities, later incorporating the effects of tidal and wave energy on benthic communities with the help of Carl Friedrichs. She also worked with Jacques van Montfrans at VIMS for two summers on the role of seagrass beds as nurseries for recreationally important fish species. As an undergraduate at UC Santa Cruz, she worked with Don Potts, completing a senior thesis on accumulation of heavy metals by mussels in Elkhorn Slough, CA. She also interned with Terry Gosliner and Angel Valdes at the California Academy of Sciences studying nudibranch taxonomy. Dorgan was a summer intern here at the Darling Marine Center with me before deciding on graduate school.

Another reason for the quick start was funding stability. She was given broad latitude to attack a difficult question by competing successfully for both the National Science Foundation Graduate Research Fellowship and the National Defense Science and Engineering Fellowship, giving her five years of funding without concerns of delivering on a particular proposal topic. We were able to tailor a very talented advisory committee on this particular question, including Eric Landis, a structural engineer at the University of Maine chosen by the Council for the Advancement and Support of Education for its 2006 Professor of the Year Award for his outstanding teaching. His teaching and advising have given Kelly the solid base and advanced engineering research tools needed to make a spectacular advance in mechanics of burrowing. As but one example, he arranged with a colleague, Sanjay Arwade, then of Johns Hopkins University to tutor Kelly for six weeks in advanced numerical modeling of crack propagation to complement her array of quantitative skills.

Dorgan is actively applying for postdoctoral positions. Her first priority would be to test her understanding of mechanical costs by measuring physiological costs of burrowing, which has not been possible in natural sediments (due to the geometrically and biologically complicated sinks for oxygen), but seems feasible in properly simulated muds.

## ASLO DISTINGUISHED SERVICE AWARD TO SUSAN WEILER

Cited by **John Dower**, University of Victoria, Canada, and **Robert Campbell**, University of Hamburg, Germany



This award recognizes members who have displayed exceptional efforts that support the professional goals and enhance the stature of ASLO. Anyone who has been involved with ASLO over the past 20 years will immediately recognize this as a perfect description of Sue Weiler. Through the establishment of the DIALOG and DISCCRS programs, and her ongoing contributions to the ASLO MAS program, Sue has

acted as mentor to literally hundreds of young limnologists and oceanographers, climate researchers, and visible minorities within the aquatic sciences.

Recognizing the need for formal mentoring of the best and brightest students in the aquatic sciences, and the need to break down the barriers that still separate the sub-disciplines of limnology and oceanography, Sue established the first DIALOG (Dissertations Initiative for the Advancement of Limnology and Oceanography) Symposium in 1994. As Sue herself put it, the aim of DIALOG was to "...reduce the historical, institutional and philosophical barriers that limit the exchange of information between limnologists and oceanographers, and to foster interdisciplinary and inter-institutional research."

In the intervening 12 years DIALOG has been held six more times, and has brought together several hundred budding researchers to learn from each other and to form lifelong peer groups. More recently, Sue founded DISCCRS (DISsertations initiative for the advancement of Climate Change ReSearch) in 2003 to bring together recent PhDs in the subdisciplines that partake in climate research. Here too, Sue's goal was to build bridges between these young scientists. The DISCCRS symposium has been held twice (DISCCRS III is scheduled for 2007), and has been just as successful as the DIALOG program.

Although an impressive accomplishment, the DIALOG and DISCCRS programs are only one way in which Sue has helped to mentor young researchers. Since 2000, Sue has coordinated a database of more than 1500 Ph.D. dissertation abstracts on ASLO's web site. She also maintains a monthly aquatic sciences electronic newsletter that is read by thousands of young scientists, and which offers tips on funding and job opportunities, upcoming aquatic science meetings, and the latest news stories on aquatic science from around the world.

The ASLO Distinguished Service Award serves as a fitting acknowledgement of Sue's leadership, her dedication, and her boundless enthusiasm, all of which have greatly contributed to making ASLO what it is today: the premier international professional association for aquatic scientists.

# JOHN MARTIN AWARD FOR A HIGH IMPACT PAPER IN THE AQUATIC SCIENCES TO VANNOTE ET AL. 1980

Cited by *Elizabeth W. Boyer*, University of California, Berkeley, California, USA.

VANNOTE, R.L., G.W. MINSHALL, K.W. CUMMINS, J.R. SEDELL, and C.E. CUSHING, 1980. The river continuum concept. *Canadian Journal of Fisheries and Aquatic Sciences*, 37 (1): 130-137.

For their benchmark paper introducing the River Continuum Concept, authors Robin L. Vannote, G. Wayne Minshall, Kenneth W. Cummins, James R. Sedell, and Colbert E. Cushing are the recipients of the second annual John Martin Award. Among those who study inland waters, these authors are legendary figures who have made many contributions to the field over the years, and their *River Continuum Concept* (or RCC) is a household phrase. The paper is theoretical in nature, presenting a general framework to describe changes in the structure and function of aquatic ecosystems that occur along the length of a stream or river, from the headwaters to the lower reaches. Emphasizing the relationships among physical, chemical, and biological processes in ecosystems, the authors hypothesized that energy inputs vary in a predictable way along the longitudinal profile of rivers, that the consequent structure of the biological communities is predictable, and that biotic communities adapt to the particular conditions of a stretch of stream. In the decades following its publication, the

RCC dominated river studies and has been evaluated, supported, criticized, and modified. Though many specific predictions based on the concept have been met with limited success when tested in specific contexts, most would agree that the RCC is a useful tool providing general principles that are applicable to riverine environments worldwide. Today there is no question that a synthetic, watershed-scale perspective is required in order to interpret processes in aquatic ecosystems that occur along river networks, as Vannote et al. originally highlighted. The RCC still continues to provide a conceptual context and to be a stimulus for data collection and ideas in the watershed sciences. The paper has been cited repeatedly (1811 times to date, per the ISI web of science accessed in December 2006), underscoring the fact that the RCC has become one of the aquatic ecology's most influential conceptual models. This paper truly embodies the spirit of the John Martin Award, which recognizes an important paper in aquatic sciences in the last 10-30 years that has had high impact on subsequent research in the field.

AT. SCI. VOL. 37, 1980

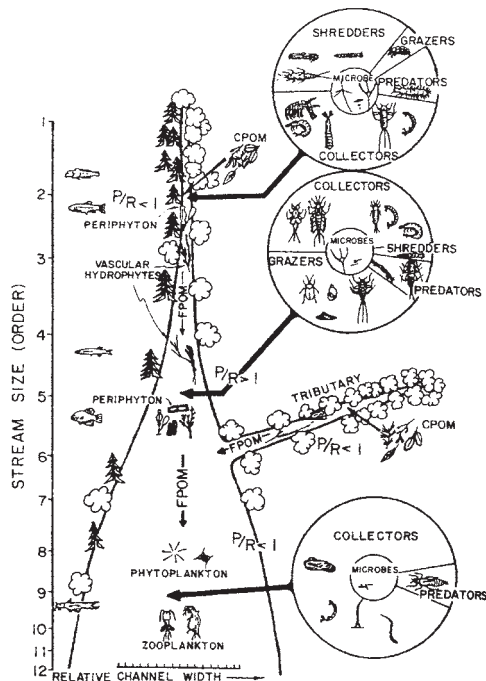


FIG. 1. A proposed relationship between stream size and the progressive shift in structural and functional attributes of lotic communities. See text for fuller explanation.

## PERSPECTIVES

### The River Continuum Concept<sup>1</sup>

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VANNOTE, R. L., G. W. MINSHALL, K. W. CUMMINS, J. R. SEDELL, AND C. E. CUSHING. 1980. The river continuum concept. *Can. J. Fish. Aquat. Sci.* 37: 130-137.

From headwaters to mouth, the physical variables within a river system present a continuous gradient of physical conditions. This gradient should elicit a series of responses within

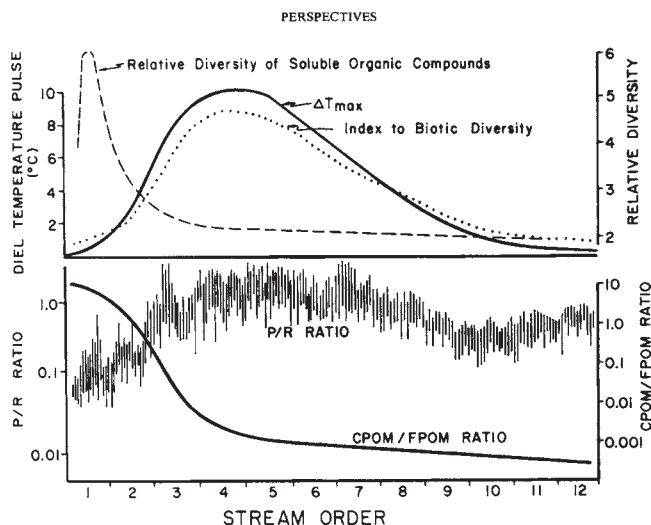
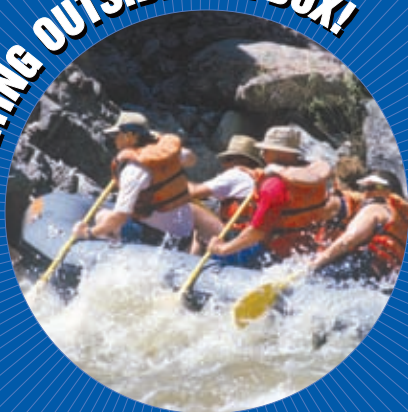


FIG. 2. Hypothetical distribution of selected parameters through the river continuum from headwater seeps to a twelfth order river. Parameters include heterogeneity of soluble organic matter, maximum diel temperature pulse, total biotic diversity within the river channel, coarse to fine particulate organic matter ratio, and the gross photosynthesis/respiration ratio.

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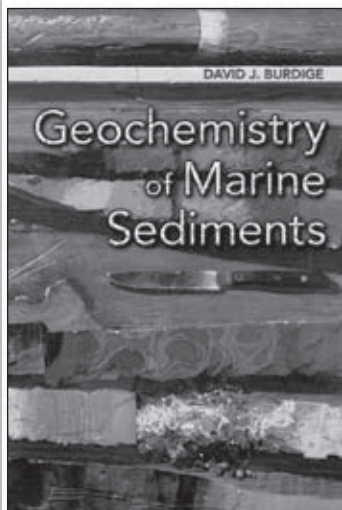
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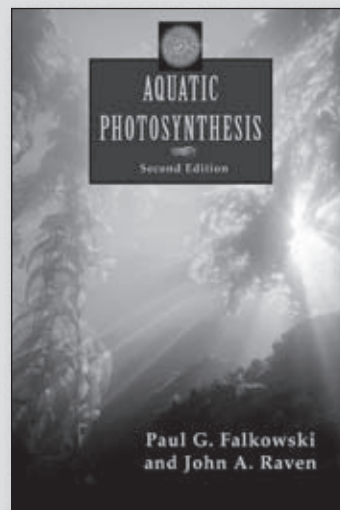
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## Questions

C. Susan Weiler  
phd@whitman.edu

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