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# IN MEMORIAM JOHN HOOD RYTHER 1922-2006

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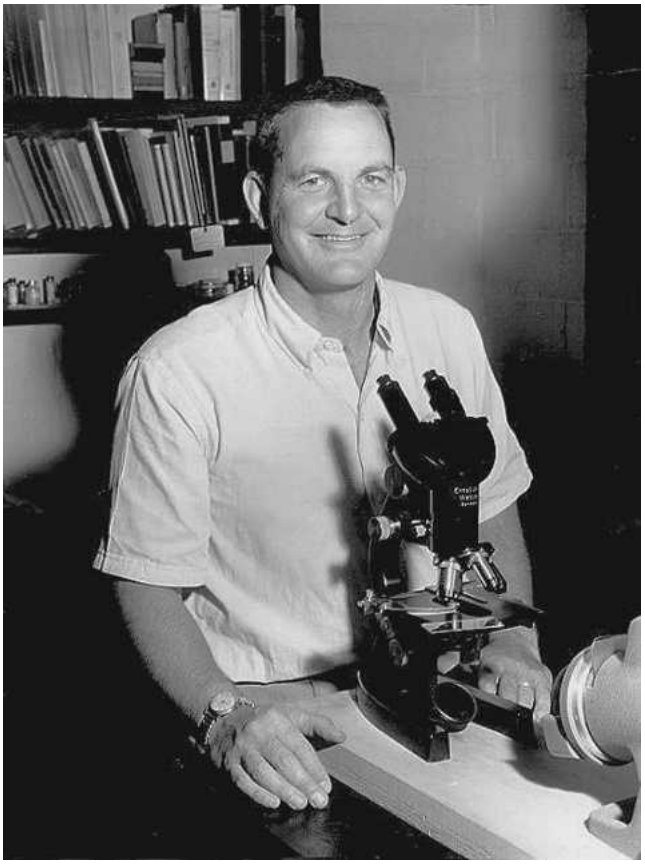

# IN MEMORIAM
# JOHN HOOD RYTHER
# 1922–2006

In 1997 I had the distinct privilege of writing the nomination of John H. Ryther for Honored Life Member in the National Shellfisheries Association. During his research career John was a proverbial giant in the fields of oceanography and aquaculture, making seminal contributions in both. John passed away in July 2006, leaving sadness in the hearts and minds of the many people who he had influenced and mentored. While I will always be thankful for John's brilliant intellect, that guided me during my post doctoral and early faculty years at Woods Hole Oceanographic Institution, my lasting memories of John will be his love of family, life, science and fishing, and his ability to offer truth in brief commentary that would serve me throughout my career. John once observed that "…The problem with marine biology is that it is so complicated that it takes you until you are about 45 to 50 years old to understand how all the pieces fit together. Then you only have about 10 productive years before they farm you out to retirement!" Hearing this daunting statement as a post-doctoral fellow made my recently acquired degree seem rather minimal in value. But I persevered and with advancing years I have often revisited John's comment and come to understand its basis in experience but, more so, its directive as a plea to have patience as we pursue understanding of the world around us. What better legacy could a mentor leave to his student?

In preparing this memoriam I am again privileged to share the task with two of John's life long friends, Charlie Yentsch and Brian Lapointe. They offer tribute in their own words.

ROGER MANN
Gloucester Point, Virginia

## THE WOODS HOLE DAYS OF JOHN H. RYTHER

About the time John Ryther returned to Woods Hole Oceanographic Institution (WHOI) from Harbor Branch Oceanographic Institution (HBOI), John told a newspaper reporter from the *Boston Globe* that a benefit of returning to WHOI was the freedom of moving from one area of research to another. I had seen John in Florida about six months prior and was aware that a move might be forthcoming: he missed his family in New England that was understandable.

However, I found the comments in the article strange. As I recall from that article, the director of WHOI stated that John Ryther had made major contributions in the field of primary production and emphasized the degree of excellence in his research in general. No doubt about this: his research catalyzed and propelled the field like no one else at that time. Yet in my opinion what was important and not voiced nor appreciated was that his unique insights and creativity helped to propel plankton research from a field

dominated by classical taxonomy into a major feature of biological oceanography. This covers the time period of the introduction of carbon-14 analysis to satellite oceanography.

My introduction to John came while I was fresh from graduate school at the Department of Oceanography at the University of Washington. Francis Richards came from WHOI to recruit. Richards (a former student of Tommy Thompson at UW) talked to me about an affiliation with WHOI. I was very much aware of the debates between Gordon Riley and Steeman-Nielsen and I was an admirer of Alfred Redfield, Bostwick Ketchum and Henry Stommel's research. Also I noted that John Ryther had recently published a very interesting article in *Scientific American* on the productivity of the Sargasso Sea.

It seemed to me that WHOI was where the action was and with Richard's offer I headed East. By the time I arrived at WHOI John Ryther and Ralph Vaccaro had published papers on the methodology of oxygen and carbon-14 measurements of carbon fixation and respiration. With these results the arguments between Steeman-Nielsen and Gordon Riley somewhat paled and the sources of the differences of the two methods appeared to be due to measurements of respiration/dark fixation.

The research that followed by others, in my view, was an outgrowth of this. With some certainty I can state that John and my attempt in 1957 to develop a somewhat universal productivity method based on the measurement of chlorophyll (see Marra et. al. 2007) was definitely due to John's previous findings and the fact that the chlorophyll method was reasonable and somewhat less complicated. The high-water mark for us was reached when Henry Stommel asked if he could have a set of apparatus for filtering. Stommel was on a *Discovery* cruise from Woods Hole to Plymouth and had nothing to do between pub hours (which were then observed aboard the ship while at sea). John and I were elated. Henry filtered samples for our research from off Cape Cod across the North Atlantic to Plymouth!

At this time we were part of B. Ketchum's plankton group with other members Ralph Vaccaro, Nat Corwin and Mac Hulbert. The group was a formal outgrowth of the Atomic Energy Commission (AEC) contractual need for more information of the transfer nuclides through the marine food chain. The ocean was considered a likely repository for nuclear wastes. Later Bob Guillard, Stan Watson, Herb Curl and George Grice became part of this group to round out measurements of the events in the ecosystem. The AEC counterpart on the West coast was at Scripps Institution and was headed by J. D. H. Strickland with Richard Eppley, Osmund Holm-Hansen, A. F. Carlucci, F. M. H. Reid, J. R. Beers, and M. M. Mullin. There was a collegial competition, in the best sense, between the East and West Coast groups.

Ketchum's plan for WHOI was that of seasonal studies of the coastal waters off Long Island using the specific talents within this group. The sea experiences were awful for most of the time. The research vessels were small, physically uncomfortable and probably unsafe. During winter months John and I joined the crew breaking sea ice off the shroud. We would go below to thaw out and bitch about the futility of working in gale-force winds in the North Atlantic. I remember hearing "deep sea oceanography sucks".

Of course there were good times. After one extremely heavy gale we put in at Cape May to ride out the storm. John and I borrowed money from the Captain and caught the ferry to New York. It was around Christmas and we spent most of the money in several jazz bars and watching the Rockettes on stage.

In my opinion John was not interested in sustaining the AEC program. John had assumed much of the AEC responsibilities of writing proposals and reports. He would rather play with ideas and do the science. It was probably John's idea to develop a permanent biological observation group at the Bermuda Biological Station. WHOI had operated a hydrographic station (S) off Bermuda. John employed David Menzel and Jane Spaeth for seasonal sampling of chlorophyll, carbon-14, bionutrients, and zooplankton and instituted a productivity sampling program. This program has grown into one of the long-term and major components for monitoring oceanography and climatology presently operating today.

My professional relationship with John waned and was correlated with personal feelings I held about WHOI. During an abbreviated period, WHOI moved away from being an idealistic place to do oceanographic inquiry. This was mostly due to the fact that the inherent disciplines of this subject were being spread around among academic institutions within this country as well as in Europe. Thus dynamics of funding, development of facilities and bureaucracies and demand for public attention we now label "education and outreach" all changed. In a few short years oceanography rivaled astronomy as the most costly of the sciences.

Oddly these stresses pushed academics, bureaucrats and politicians to settle on definitions of ocean science. Ryther and Ketchum spent undue time and energy trying to define biological oceanography. Was it all marine biology? Was it marine ecology? Was not all of this biology? John's role was chairman of a department whose boundaries within WHOI were vague. He was being asked by a board of trustees (made up of elites from major academic institutions) to erase the vagueness and develop a plan for ocean biology at WHOI. He got little leeway from the board that almost by tradition favored the university departmental model. And, in my opinion John got little help from the biological giants at WHOI such as Redfield and Ketchum. In retrospect this is surprising because at an earlier date Redfield had asked John and myself to help him in the preparation of a paper on the Education and Recruitment of Oceanographers prepared for the Society of Limnology and Oceanography.

During this period of transition a large majority of the scientists had what would now be called "a palace revolution." It was a short-lived protest against the director who appeared not to recognize the past glories of WHOI. Our revolt was based on the idea that freedom of interdisciplinary research was a hallmark of the greatness at the institution and should be preserved at all costs. The Board was firm. The "University Plan" was to prevail. Those of us with contrary opinions should go: many of us did.

With increasing uncertainty of funding and all of the above chaos, John did not find the job of department chair enjoyable. Before my departure I had discussions with him concerning the status of the biology department. I should have been more diplomatic, but I bluntly expressed my despair. Our status among international and national oceanographers was declining. Morale was low, back biting the name of the game and goals uncertain. I did extend that my time with him had been great and that I would always remember the glory days and his rigorous pursuit of answers on organic growth in the ocean. In a very stoic manner he responded that his goals were now elsewhere. But where?

Reflecting on why I believe John returned to WHOI—perhaps it is quite simple. John had always had been somewhat indifferent to oceanography, at least as I saw it. He once told me he did not understand why persons like he and I were funded. Although he was proud of his own achievements in research and the development of programs such as the Indian Ocean Expedition, they were not his love. What John truly loved was fishing. On all cruises John showed up with his fishing pole. He by all measures was a good fisherman and student of the sea. Moreover, the first time I met John he pointed out that he and Ralph Vaccaro had placed a sack of oyster shells in Falmouth Harbor. Perhaps this was John's first attempt at aquaculture. Something "practical" he liked to say.

But why return to WHOI? Had he forgotten the dark periods of trouble that existed there? I think not. I think he found worse situations elsewhere. Moreover, he was coming back to a place where his legacy had persisted. The persistence of legacy is interesting. In my mind it persists because of incidents.

How does one thank another? John did it by 1) showing vision and defining the importance of individual research 2) setting an example of excellent experimental design 3) injections of humor in situations where it was badly in need. I close with a few examples:

* After returning from a meeting in Copenhagen, I asked him "what was new?" his retort "God thinks he is Steeman-Nielsen."
* While standing in a cafeteria line watching John accidently dump the garbage off his tray into the wrong bin—the one containing hot rolls.
* The reaming out by the Captain of *R.V. Atlantis I*, we both received, because we mistakenly hooked the carbon-14 cooling- light incubator to the ship's freshwater spigot.
* Both of us being thrown off the beach in Hawaii for drinking beer.
* The disappointment that we both felt after retrieving a carbon-14 experiment at sea with only the dark bottles remaining.

- John laughing uncontrollably when a waiter asked me if I wanted natural juices on my steak.
- I never took the opportunity to tell him that he had charisma—but indeed he did. In every avenue of his professional adventures he left a legacy. Yet my guess, if asked, his ultimate goal was to put that hook in a large striped bass in Woods Hole Harbor.

CHARLES S. YENTSCH
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**THE RESEARCH OF JOHN H. RYTHER: AN UNBROKEN CIRCLE**

I first learned of John Ryther's research on nutrients, algal growth and eutrophication in a conservation class at Boston University (BU) in the Fall of 1972. His classic paper on nitrogen, phosphorus, and eutrophication in the marine environment just appeared in *Science* the previous year and was required reading for the class. I was so inspired by that article that I drove down to Woods Hole at my first opportunity to learn more about this extraordinary scientist. I did not meet John on that first trip, but the salty and cerebral Woods Hole environs led me to apply for a research assistant position at Woods Hole Oceanographic Institution (WHOI). After all, I would be graduating from BU in May of 1973 and needed to find employment. Several months later I got a phone call from WHOI indicating that a certain scientist in the Biology Department was seeking an assistant on a wastewater-recycling aquaculture project. Like a dream coming true, that scientist turned out to be John—and I ended up meeting him on my first day of employment in September 1973. Not realizing it at the time, my years ahead with John as a mentor and friend would be a life-changing odyssey.

John was 51 at the time and segueing from his esteemed career in biological oceanography to more applied research in aquaculture. John was born in Newton, MA on July 17, 1922, and graduated from Newton High School. From 1942 to 1945 he served in the US Army Air Force, flew 83 combat missions in Europe, and was discharged in 1945 with the rank of captain. John had a great sense of humor and shared with me amazing stories—like the time he unknowingly landed his P-51 Mustang during a combat mission on a German-held airfield—only to "touch-and-go" once he saw the Swastika's up close! After the war, John received his A.B. degree in 1947, his M.A. in 1950, and Ph.D. in 1951 from Harvard University. In the winter of 1949-1950, he worked on a project stocking the Mashpee River with hatchery-raised trout to force the native trout out to sea. John received a small scholarship from Harvard to continue his work on the "salter" trout project at WHOI, spending a memorable summer of 1951 with his wife Jean at a little fishing camp on the Mashpee River. He joined the WHOI staff as a research associate in marine biology in October 1951, working with Al Redfield, Buck Ketchum, and Charlie Yentsch, among others.

At about this time, the townships of Islip and Brookhaven, Long Island, NY, approached WHOI about investigating the decline and collapse of the once-prosperous oyster industry in Great South Bay and Moriches Bay. John applied for a grant from the National Science Foundation to help support his research on "the etiology of the phytoplankton blooms of Great South Bay"; he received the award, making him the first scientist at WHOI to receive NSF funds. It was this research, which John published several years later (Ryther, 1954), that represents the first case study of nitrogen-driven coastal eutrophication involving harmful algal blooms (a result of excessive biomass, not toxicity). The source of the nitrogen (including dissolved organic nitrogen) was the burgeoning Long Island duckling industry that lined the tributaries of these bays. Following John's compelling studies, the duck farms were moved away and, together with additional efforts to improve flushing of the bays, led to recovery of water quality and overall health of these bays. Following a twenty-year hiatus in eutrophication research, John re-awakened his interest in coastal nutrient pollution, demonstrating with colleague Bill Dunstan (Ryther and Dunstan 1971) that nitrogen was the critical limiting factor for phytoplankton growth in temperate coastal waters. This lead article in *Science* caused a bit of commotion. Because the phosphorus in detergents at the time was widely blamed for eutrophication, manufacturers were busy replacing phosphorus with nitrogen, which, as John pointed out, would exacerbate—not solve—the problem in marine waters. When the nitrogen-driven

coastal eutrophication problem was described in an editorial in *Science* in 2001 as a recent problem "that snuck up on us", John was amused, replying "Yeah, a thirty-year sneak by my reckoning !"

Following the Long Island project, John began work on another problem. In the early 1950's the Danish oceanographer Einer Steeman Nielsen introduced the $C^{14}$ method for measuring primary productivity of the oceans. Using this tequnique on a round-the-world cruise aboard the Danish research vessel *Galathea*, Steeman-Nielsen estimated oceanic productivity to be an order-of-magnitude lower than estimates of Gordon Riley, then of Yale but formerly of WHOI, who had used relatively crude and less sensitive methods. Because Riley was at WHOI when he made his estimates, Redfield felt someone at WHOI should resolve the discrepancy … and John was elected. This increasingly took John to sea on research cruises and in 1961 he was asked to serve as director of the U.S. Biological Program of the International Indian Ocean Expedition (IIOE). The U.S. participation in the IIOE was funded by NSF, with John running it out of WHOI with the help of his deputy Ed Chin and close colleague David Menzel. This program, which involved 150 scientists from the U.S. and abroad, utilized the presidential yacht *Williamsburg* and the recommissioned *Anton Brunn*, to collect data on particulate and dissolved organic carbon in the Indian Ocean and Arabian Sea.

John's work on nutrients and marine productivity evolved further in the years following the IIOE. He made several cruises on WHOI ships in highly productive coastal upwelling areas (Peru, SW Africa) and in highly-impoverished central-gyre oceanic regions (Sargasso Sea). John was struck by the fact that much of the oceans could be characterized as extremely nutrient-poor and unproductive. As John had always been interested in fishing, he naturally felt compelled to relate his measurements of primary production to fish production in the sea. At the time, some prominent U. S. and Russian fishery biologists were predicting potential global harvests of a billion tons per year. John developed a simple model based on primary production and food-chain dynamics that predicted a sustainable yield of $\sim$ 100 million tons per year. Many consider this paper as John's *opus magnus* and it appeared as another lead article in *Science* (Ryther 1969). At the time his paper was published, the annual harvest was $\sim$ 60 million tons per year although in the last several decades it has been hovering around 100 million tons per year. Although that paper was an important milestone in oceanography, it also brought hostility towards John from fisheries biologists who at the time did not want to hear this message—and some still do not. Recently, a noted fisheries biologist published an article in *Science* suggesting that the annual fisheries yield has been close to John's estimate of 100 million tons per year—but did not cite John's seminal work which was also published in *Science*—in 1969. John taught me to review and cite the pertinent scientific literature, no matter how old it is, as a part of publishing process—or risk "re-inventing the wheel". It seems that in these days of Google, important historical research is being lost to students and researchers who do not take the time to use libraries for literature searches.

In the early 1970's, John's interests shifted towards "controlled eutrophication"—using nutrients in partially-treated wastewater to grow phytoplankton as food for commercially-valuable filter-feeders (molluscs) and growing commercially-valuable seaweeds on the nutrients regenerated by the molluscs. My tenure with John at WHOI between 1973 and 1977 involved growing commercially valuable red seaweeds such as *Gracilaria* and *Agardhiella* as part of this experimental wastewater-recycling aquaculture system. After some exploratory research, John raised enough money to build a moderately-sized pilot-scale system of ponds and raceways—the WHOI Environmental Systems Laboratory (ESL). The testing of this system was supported by a new program at NSF called RANN (Research Applied to National Needs). Although initial trials were encouraging, especially with the mass cultures of fast-growing seaweeds, the RANN program was discontinued and no other source of funding was available. Meanwhile, Ed Link and Seward Johnson, Sr., directors at the Harbor Branch Foundation in Ft. Pierce, FL., became interested in the wastewater recycling aquaulture system and several of us at WHOI moved to Harbor Branch in the winter of 1974 to set up a small experimental system. In 1977 I decided to go back to college for my M.S. and Ph.D., which turned out to be one of the most difficult decisions of my life—leaving both John and WHOI.

Just as I was entering graduate school, my previous seaweed culture work with John began to pay off—if only temporarily. During the oil crisis of the mid-1970's, alternative energy sources became fashionable and funding emerged for production of fuels from biomass. Because our research had shown that *Gracilaria* had one of the highest rates of organic production of any plant on earth (Lapointe et al. 1976), we forgot about wastewater recycling and began to focus on growing *Gracilaria* and other seaweeds, maximizing their yields, and fermenting them to methane, both at Harbor Branch and at WHOI (Lapointe and Ryther 1978, 1979; Ryther et al. 1978). John and I kept in touch through my four years of graduate studies at University of Florida (UF) and University of South Florida. When I finished my Ph.D. in 1982, John called and offered me a post-doctoral position to continue my dissertation work on productivity and nutrition of seaweeds—in the Florida Keys. Being an avid SCUBA diver and recognizing the great opportunities for experimental field work with John, I jumped at the opportunity. John had just received a grant from the Gas Research Institute (GRI) through the Institute of Food and Agricultural Sciences (IFAS) at UF. So, after 30 productive years at WHOI, John retired to Gainesville, FL to oversee the program as I was settling in to my new post-doc in the Florida Keys. Meanwhile, Bob Jones at Harbor Branch in Ft. Pierce enticed John with a position as director of the new "Division of Applied Biology" and in January, 1983, both John and I became employed by Harbor Branch. Research in this division included aquaculture, natural products chemistry, and water quality monitoring. The GRI project ended in 1984 under the new Reagan administration. As John noted, "The Republican's weren't interested in alternative energy, DOE dropped its support of biomass research, and we were broke again". In 1987, John fully retired and returned to Falmouth, MA, accepting a Scientist Emeritus position at WHOI in 1988 where he worked at his own pace "without ever having to write another proposal".

In his retirement on Cape Cod, John came full circle. *Trout Unlimited* asked him to supervise a summer student fellow in an update and state-of-the-knowledge survey of the behavior of anadromous brook trout in saltwater. John chose to do the job himself and spent a summer in the library and visiting trout streams from Long Island to the Canadian Maritimes to find out what had been done in the past 50 years since his early days on the Mashpee River. This last project exemplifies the original and creative thinker that John always was. During his career that spanned five decades, John published more than 120 scientific publications and

co-authored one of the first comprehensive books on aquaculture. Through the years John served as a consultant to numerous government and state agencies, utility companies and state water projects, including the National Science Foundation, National Institutes of Health, Bureau of Sport Fisheries and Wildlife, New York State Department of Education, National Council on Marine Resources and Engineering Development in Aquaculture, Boston Edision Company, and Maine Yankee Atomic Power Company. John's visionary contributions made a major impact on a wide variety of scientists spanning the fields of oceanography, environmental science, and aquaculture.

Beyond John's many academic contributions, he also gave generously of his personal time to help me grow personally. My father died when I was nine years old and I grew up without a male figure to teach me even the most basic things, like fishing and boating. During the early 1970's while I was working with John in Woods Hole, he took time to take me out on the water fishing and clamming experiences I recall so fondly. He was always there for advice on whatever problems arose. The Ryther family unofficially adopted me and we have many wonderful memories of times together on Cape Cod, Florida, Bahamas, and Costa Rica. In January 1994, John and his family visited my wife, Lee, and I on Green Turtle Cay in the Abacos, Bahamas. Nine months later my own son, Sebastian, was born. It was an honor and privilege to have been so close to John for so many years. I am eternally grateful for the wisdom he shared with me and I wish him gentle seas ahead.

**Acknowledgements.** Parts of this essay were taken from an interview with John Ryther by Elizabeth H. Gladfelter on October 27, 1998. Additional information was obtained from the obituary by Shelley Dawicki that appeared in the Marine Technology Reporter. This article was written between December 27 and 29, 2006 on Green Turtle Cay, Abacos, Bahamas, a lovely place where John and I fished together.

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