Robert H. Burris

1914–2010

BIOGRAPHICAL *Memoirs*

A Biographical Memoir by
Paul W. Ludden

NATIONAL ACADEMY OF SCIENCES

ROBERT HARZA **BURRIS**

April 13, 1914–May 11, 2010

Elected to the NAS, 1961

By Paul W. Ludden

Robert Harza "Bob" Burris was born April 13, 1914, in Brookings, South Dakota, to Francis (Harza) and Edward Thomas Burris. Bob devoted his life to science, education, and academic leadership, and with the exception of postdoctoral work at Columbia (1941) and a Guggenheim Fellowship (1953–1954) split between Helsinki, Finland, and Cambridge, England, he spent his entire career at the University of Wisconsin-Madison.

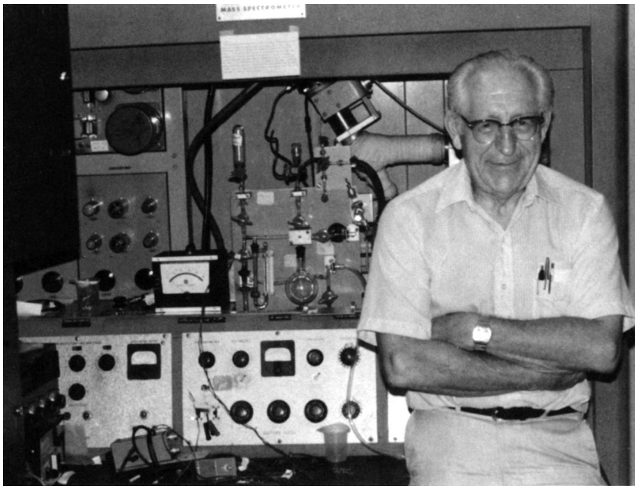
Bob's unpublished draft autobiography gives every indication that he relished his youth in a small Midwestern university town. His older brother, Dick, was an accomplished musician, undoubtedly benefiting from the tutelage of their mother, who taught piano and played for their local church. Bob himself played the trumpet through high school and college, and although he deferred to his brother as the musician in the family, he was good enough to play in bands that were hired for events around Brookings. He was an enthusiastic Boy Scout, proud of his Eagle Scout Award and of the years he and future fellow scientist Irwin Gunsalus were Scout Masters together in Brookings. Bob was an outstanding student, and his scholarly talent was recognized early and often by all who came in contact with him.

According to Bob his father also had broad intellectual interests. Edward Burris ran a print shop in Brookings, where Bob worked until he was hired by the chemistry labs at South Dakota State University. Bob noted in his autobiography that in order to set type it was absolutely essential that he be able to read a page upside down and backward. When word processors became the norm in writing manuscripts, he marveled at the ability to correct a mistake simply by backspacing on the screen. Lead type was a far less forgiving medium, and Bob's phenomenal abilities as an editor reflected a typesetter's need to get it right the first time. Any of Bob's students would give testament to his editing skill and journals including *JBC, Science, Nature, J. Bacteriology,* and others called on him frequently to edit.

Bob took his undergraduate degree in chemistry in his hometown of Brookings at South Dakota State College (later South Dakota State University). By good fortune or coincidence, under just the right circumstances and with great benefit for all, Bob was part of an outstanding group of students who came together at South Dakota State and went on to earn graduate degrees at Wisconsin-Madison. Bob and Henry Lardy each received the National Medal of Science from the President of the United States, as well as the Wolf Prize from the State of Israel. Bob, Lardy, and Van Potter all became members of the National Academy of Sciences. Irwin Gunsalus, who left Brookings after high school for Cornell, went on to significant success at the University of Illinois and also became a member of the National Academy of Sciences. In a town of 4,000 during the depths of the Depression, when a small fraction of the population attended college and a much smaller fraction went on to graduate studies, it is amazing that such a small cadre had such an extraordinary impact on the development of biochemistry as a science across a broad spectrum of areas that included plant and microbial biochemistry (Bob), reproductive biology and enzyme function (Lardy), cancer biology (Potter), and microbial metabolism (Gunsalus). Bob never forgot his South Dakota roots, and years later he established at his undergraduate alma mater the “Bob and Katherine Burris Lectureship”—named for him and his wife, whom he married in 1945.

Bob in the lab.

In 1936 Van Potter recruited Bob to Madison for a summer job running Kjeldahl analyses—the standard method for assessing nitrogen content in chemical substances. Bob jumped at the opportunity, and the remainder of his life centered around the area. His technical and intellectual talents were soon recognized by members of the Bacteriology Department, and Perry Wilson, the leading researcher in biological nitrogen fixation at that time, recruited Bob as a student. Wilson himself had been invited to Madison by E. B. Fred, who had come to Wisconsin in 1913 to begin studies on soil fertility (biological nitrogen fixation) in an ongoing project that persisted for almost 100 years at the university. Under Wilson's guidance, Bob completed his Ph.D. in bacteriology in 1940 and received what was then a rarity, a National Research Council postdoctoral fellowship to work at Columbia University with Harold Urey, one of the great physical chemists of his generation. Bob later noted that he was the only member of that year's crop of Ph.D. students from the department to have a job.



Bob with the Consolidated Van Nier Mass Spectometer.

Urey's primary interest was in isotopes of hydrogen, and Bob had to persuade him of the importance of the project on biological nitrogen fixation and the utility of a stable isotope of nitrogen as an approach. In their discussions, Bob and Urey quickly realized that it was necessary to demonstrate the lack of exchange of atoms of nitrogen from the dinitrogen molecule if the approach were to work. The boldness of the 26-year-old, newly minted Ph.D.'s approach to Urey, to use mass spectrometry—then in its infancy—to address the mechanism of biological nitrogen fixation remains impressive even after Bob's own accomplishments are taken into account. He continued his work in Urey's lab during 1940 and 1941, paving the way for use of ^{15}N in nitrogen fixation and a range of other pathways of the nitrogen cycle. He also learned mass spectrometry and became a proficient glass blower, which was essential for the care and maintenance of the homemade mass spectrometers of that time.

When he returned to Wisconsin in 1941, Bob assumed something of a "super" or "senior postdoc" position in Perry Wilson's Bacteriology Department laboratory. Wilson was not many years older than Bob, and they became close colleagues. They published many papers together, their blend of talents producing a powerful collaboration, as both researchers and intellectual leaders in the area of biological nitrogen fixation. Working with the Physics Department, Bob set up a homemade mass spectrometer that would serve as his major tool until he acquired a commercially made instrument from Consolidated Van Nier in 1948. Bob's would be just the third instrument off the line from Van Nier, and it produced publishable results from 1948 until its semi-retirement in 1980. During that time, he maintained the instrument personally without any service contract or visits by service personnel.

Bob was hired to fill in teaching in the Biochemistry Department when the plant biochemist, W. E. Tottingham, died suddenly in 1944. His appointment was made permanent the following year, and he moved his lab into the department as an assistant professor. He progressed rapidly, with promotion to associate professor in 1946 and professor in 1951. When he established ammonia as the first product of nitrogen fixation, Bob became a leader in the study of nitrogen fixation—and his lab became the arbiter for nitrogen fixation.

Bob's results, however, were not immediately accepted by all. Most notably, the Finnish chemist Artturi Virtanen held fast to the hydroxylamine hypothesis until Bob went to his lab in 1953 on a Guggenheim Fellowship to convince him of the ammonia hypothesis. Bob's lab's *Science* paper in 1941 demonstrated conclusively the failure of barley to fix molecular nitrogen. The lab also debunked claims that quail eggs fixed biological nitrogen, among other agents. Using ^{15}N mass spectrometry, Bob and his group were able to confirm, on the other hand, reports by Martin Kamen and Howard Gest that photosynthetic bacteria were capable of fixing nitrogen. In fact, Gest and Kamen's the initial experiments led to the discovery that with very few exceptions, all purple sulfur and purple nonsulfur photosynthetic bacteria are capable of biological nitrogen fixation.

Although Bob is renowned for his work in biological nitrogen fixation, the absolute cornerstone of his career was in methods development. His very first publication in the lab of Professor Kurt Franke (with co-author R. S. Hutton, from the South Dakota State Experiment Station) was entitled "A New Colorimetric Procedure Adapted to Selenium Determination." This would be the first of many Burris papers that included the words "method," "procedure," and/or "technique."

Bob also participated in the university's teaching programs in bacteriology and biochemistry. He taught plant biochemistry every spring from 1944 to 1984, except when he was on leave in 1953. In addition to his work on a team project to study the metabolism and growth of penicillium cultures (to support the war effort during World War II), Bob developed interest in a wide range of topics, and these would play out in his career.

In 1960 the field of nitrogen fixation took a huge step forward when Wilson's former student Leonard Mortenson, working with a group at DuPont, devised conditions for producing consistently active, cell-free preparations showing nitrogenase activity. The technique involved using heat-dried cell paste from the anaerobic bacterium *Clostridium pasteurianum* and extraction of the enzyme under anaerobic conditions in the presence of high concentrations of pyruvate. A number of steps employed by Mortenson had been tried by Bob's lab, but not all together in a combined process.

Over the next two decades, in a competitive but collaborative effort with a number of labs around the world, Bob's lab worked out the basic enzymology of the nitrogenase enzyme system. The enzyme was found to consist of two easily separable components, a molybdenum-iron protein and an iron-containing protein, both oxygen-labile but otherwise relatively stable. Bob's lab demonstrated that the activity required magnesium adenosine triphosphate (ATP), although other labs reported inhibition by this cofactor. With time it came to be understood that the product of ATP hydrolysis by the enzyme (ADP) was a potent inhibitor of the enzyme activity. The component proteins were individually purified to homogeneity from a number of organisms, with Bob's lab initially focusing on *Clostridium pasteurianum*, which gave the highest-activity extracts. Bob's lab teamed with EPR (electron paramagnetic resonance) spectroscopist Bill Orme-Johnson of the Biochemistry Department and the Enzyme Institute, as well as with Vinod Shah and Winston Brill from Bacteriology, to study the enzyme.

Bob's skill as a glass blower was very valuable in designing and fabricating the oxygen-free vacuum line necessary to manipulate the liquids and gases required for handling the enzyme. He adapted other techniques from his colleague Helmut Beinert, who ran the spectroscopy lab at the Enzyme Institute. Bob taught all of the students the proper way to grease a stopcock and experimented with various stopcock greases to find the perfect material. He eventually hit upon a 50/50 mix of Apiezon W and a rubber-containing compound that gave just the right consistency and low vapor pressure necessary for repeated turns of the stopcock without endless regreasing. In the mid-1960s, with enzyme preparations available, Bob's lab began investigating alternative substrates for the enzyme. In 1967 Bob and R. Schöllhorn reported in *Proceedings of the National Academy of Sciences* that acetylene was a competitive inhibitor for N_2 fixation, and shortly thereafter, Bob, along with visiting postdoc W. D. P. Stewart and undergraduate student

G. P. Fitzgerald, used the reduction of acetylene to ethylene to monitor nitrogen fixation activity in waters from Lake Mendota in Madison. Once again, Bob drew on the cornerstone of his career, methods development, to catalyze a paradigm shift in the study of the enzyme.

Before the development of the acetylene education assay, few of Bob's students remained in the field of nitrogen fixation for long, because mass spectometric analysis required several hours per sample, as well as the need to maintain a complicated instrument. After the development of the acetylene reduction technique in Bob's lab, most of his students spent part or all the rest of their careers studying the enzyme.

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Although he continued his active and productive work in the laboratory on the properties and mechanism of the nitrogenase enzyme, he realized the enormous potential for the acetylene technique and convinced the National Science Foundation to provide him with a camper/trailer outfitted with a gas chromatograph and laboratory materials. With collaborator Larry Vanderhoef from the University of Illinois (and later president of UC Davis), Bob embarked on a study of bodies of water around Wisconsin, including Green Bay and a set of lakes at various stages of eutrophication—the infusion of nutrients that promote excessive vegetation—in northern Wisconsin. John Tjepkema in Bob's lab provided the first extensive and reliable analyses of nitrogen fixation in prairieland using the university's arboretum as his test laboratory, and other analyses were carried out during algae blooms on Lake Mendota and other areas. This work had a tremendous impact on our thinking about the nitrogen cycle on earth.

The development of alternative substrates for the enzyme also provided a new tool for the analysis of the activity of the enzyme. Schöllhorn and Bob showed that acetylene was reduced to ethylene in a two-electron reduction. Azide was reduced to N_2 and ammonium, and N_2O was reduced to N_2 and H_2O. Cyanide was reduced to methane and ammonium and provided an alternative technique, but the Km—a measure of enzyme kinetics—for cyanide was significantly higher than that of acetylene. As described in a pair of masterful papers, Bob's lab carried out kinetic analysis of the interaction of the various substrates of the enzyme, which revealed the complexity of the enzyme mechanism. Acetylene, for example, was a "competitive inhibitor" versus N_2, whereas N_2 was "uncompetitive" versus acetylene. Oxygen was a substrate and killer of the enzyme and was "noncompetitive" versus N_2. H_2 was found to be a competitor versus N_2 but did not inhibit proton reduction to H_2. Carbon monoxide inhibited all substrate reductions except proton reduction to H_2. H_2 failed to inhibit acetylene reduction. Only now, as we understand the three-dimensional structure of the enzyme and its complicated metalo-organic cluster, are we beginning to understand the bases for these complex interactions.

The development of the acetylene reduction technique also revealed Bob's extraordinary generosity. When he learned that one of his former postdocs, Mike Dilworth of Australia, had also discovered that acetylene was reduced to ethylene by the enzyme, Bob took great pains to ensure that Dilworth received equal credit for the independent discovery of acetylene reduction, with all that followed from that important discovery.

The availability of a technique such as acetylene reduction to measure nitrogen fixation *in situ* led Bob to participate in two oceanographic cruises off the Great Barrier Reef of Australia. The first cruise looked at carbon and nitrogen cycles in ocean waters and led to a significantly new understanding of nutrient cycles in the Barrier Reef. On the second cruise, Bob reveled at being able to participate in a scientific expedition organized by his son, John, then a faculty member at Penn State University.

Throughout his career, Bob was a brilliantly inventive scientist, developing techniques and equipment for use in scientific studies. In 1948 he collaborated with Henry Lardy, J. Hipple, and Warren E. Gilson (the inventor of the Pipetman) to design a circular shaking bath for a manometric microapparatus. Bob's inventiveness was a good match for his frugality, and when he finally replaced the Consolidated Van Nier mass spec with a more sensitive and advanced MAT instrument, he obtained a jewelers cone and gold wire and tapped out perfectly functioning replacement washers of his own design and making.

Bob's accomplishments and contributions were recognized in many ways through awards and election to prestigious societies and academies. At the relatively young age of 47 he was elected to the National Academy of Sciences in 1961, and subsequently to the American Academy of Arts and Sciences (1975) and the American Philosophical Society (1979). In 1980 he was awarded the National Medal of Science, and in 1985 he received the Wolf Prize in Agriculture. That same year, he was elected as a Foreign Fellow of the Indian National Academy of Sciences. Bob was quite proud that in 1988 he was recognized by both his undergraduate and graduate schools, winning their campuses' distinguished alumni awards. He also received significant awards from the Botanical Society of America, the American Society of Plant Physiologists, the American Society of Agronomy, and the American Chemical Society.

Bob accepted recognition with humility but also with a great sense of obligation. He was a consummate citizen scientist who carried a strong conviction that along with the benefits associated with the recognition he received came great responsibility to serve his university, his community, his nation, and the world. His induction into the National Academy of Sciences at a young age facilitated his commitment to service, and his inclusion as a member of various NAS committees, advisory boards, and review panels added luster to those committees. Bob became known in various organizations as an extraordinarily dedicated committee member and often served on the writing or editing subcommittees for various reports. Among the notable panels on which he served were the Committee on Nitrogen Fixation and Photosynthesis Research in the People's Republic of China, chairman 1979-1981 (NAS Committee on Scholarly Communication with the People's Republic of China); the Committee of 6 (Senior Scientific Panel) to represent President Reagan's Executive Science Office in establishing with Indian Prime Minister Indira Gandhi an Indo-U.S. Science and Technology Cooperation in January 1983; the Board on Science and Technology for International Development; the National Science Foundation Committee to Evaluate Alternative Biological Resources (as chair); the National Research Council on Agriculture to Evaluate the Agricultural Research Service in 1985; and the National Research Council Committee Report on Field Testing Genetically Modified Organisms. He served as chair of the Committee Advising on the Reorganization of the College of Natural Resources at UC Berkeley. In 1992 he served on the committee that provided the Report of the Workshop on Biology-Based Technology to Enhance Human Well-Being and Function in Extended Space Exploration. He was an invitee and presenter at the landmark Asilomar Conference on Genetic Methods in 1975. This group has essentially set the directions for research and safeguards in the use of new technologies in molecular biology and genomics for the coming generation. He also served on the committee that recommended standards in labeling with respect to foods prepared from genetically modified crops, the recommendations of which have withstood challenges until very recently.

In 1958 Bob succeeded Conrad Elvehjem as chair of the Department of Biochemistry and served in that capacity for 12 years. During that time, he was an excellent steward of that great department and nurtured the careers of an extraordinary group of colleagues who would go on to become the next generation of department, college, and campus leaders. Included among these were Hector DeLuca, Julius Adler, and W. W. Cleland, all of whom went on to become members of the National Academy of Sciences. In the period following his chairmanship he worked to establish the USDA Competitive Grants program to provide funding in areas of agricultural research on a competitive basis. Bob firmly held that the best research would be done based on investigator-initiated proposals and a competition of the best ideas. He and a group of other scientists had to convince the U.S. Department of Agriculture establishment and Congress of the wisdom of this approach. In the early days of that program, he was a strong and consistent voice that it must be used to foster the careers of scientists entering the field. He even declined a grant one year to ensure that a young colleague at another university would receive funding.

Bob was an extraordinary mentor to his students, and he carried out this role largely through personal example. He was extremely disciplined in his own life, and no student could be in the lab for long without recognizing the powerful example he set. Long after the rest of the world had gone to a five-day workweek, he continued both teaching and working in the laboratory on Saturdays. For many students, their favorite lectures offered by Professor Burris were on the personalities involved in plant biochemistry. These sessions included photographs that he had taken of major scientists in the field of biochemistry, anecdotes about their scientific approaches, their personalities—and, in some cases, the disagreements among them. Bob's usual form of transportation was a four-speed bicycle he had purchased while on sabbatical in Cambridge in 1954. He got approximately the same number of years of service from his bicycle as he did from his Consolidated Van Nier mass spec.

Bob held weekly lab meetings with his graduate students, at which several students reported on the results of their experiments. The carbon copies of lab notebooks, "yellow sheets," were due in his office the Thursday before a report was to be made, and new students quickly learned that he took those reports home and read them the evening before the meeting. These events were informal and positive, but students knew that their ideas were on the line when it was their turn to present. The process imparted a discipline to students to keep their lab notebooks up to date, and I do not recall an instance where anyone writing a thesis was unable to find experimental results or was forced to redo an experiment because a piece of data had been lost.

The original data was required, as well as the methods and conclusions section for each report. Occasionally Bob would return the sheets with editorial comments on experimental design, grammar, or validity of conclusions. We students might not have recognized it at the time, but he was teaching discipline, intellectual rigor, experimental design, collaborative discussion, and creativity. In the period I was in his lab, the meetings were extraordinarily congenial, and all evidence supports my belief that this was pretty much always the case. Students were treated well and supported, and they thrived. Occasionally students would wonder if 8 a.m. Friday, following a late night of experimentation (or other activity), was the optimal time for lab meeting. Bob would note that Friday at 8 a.m. was all the relaxation of discipline that he could bring himself to provide given that when he was in Perry Wilson's lab, the meetings were at 8 o'clock Sunday morning. Bob also shared that he had experienced the wrath of Conrad Elvehjem when he moved the department seminar from noon on Saturdays to Wednesday afternoons. In Bob's telling, Elvehjem was pretty certain that this spelled the end of quality in biochemistry at Wisconsin. (My own lab meetings began at 8:30 a.m. on Fridays, but Bob graciously spared me a lecture on softening of discipline.)

The other weekly event in the lab was Journal Club, which was held at noon on Fridays. We were encouraged to report on key articles we had read that were relevant to our own thesis and the laboratory, but also to have an interest outside of our lab to broaden us. Bob occasionally noted that graduate school was an intellectual enterprise where one became increasingly focused in a narrower and narrower area, and there was a need to jolt oneself occasionally to read more broadly. He, himself, would occasionally bring in articles quite far afield from our work. When there was a flurry of reports about cold fusion in the 1970s, Bob reported on one of those in a Journal Club. A few weeks later in lab meeting, Bob pulled out a few pages of experimental data, where he had gone down to his isotope ratio mass spec and used a Teflon leak inlet to the mass spec like connected to a chamber where he ran an electrical current under a mixture of H_2O and D_2O. He set his conditions to those of the original report and tested for the generation of ^3He to see if any fusion could be detected. Finding none, he reported in lab meeting that he was unable to reproduce the results of previous reports. He did not, however, call a news conference or publish this result in *The New York Times*.

Many people came to Bob's lab over the years to learn how to perform mass spectrometry, manometric techniques, anaerobic techniques, and the acetylene reduction assay. He trained more than 70 Ph.D. students in his career, as well as quite a few master's students (from the time when the master's degree was an obligatory step before the Ph.D.), and as many postdocs. There were always a number of undergraduates in the laboratory. Bob took a personal interest in each individual, and I know of no former student who did not value the experience he or she gained there. Everyone left his lab knowing they had been mentored, and over time the personal value of that mentoring increased for virtually every one of them.

Bob enjoyed the laboratory and the process of generating new information much more than he did the secondary organization of information. Although he was a disciplined writer, he managed to avoid almost all of the many invitations that came his way to write textbooks. He had a pact with his lifelong friend, Pete Peterson, who was born on the same day that he was in Brookings, South Dakota, and who went on to become a professor of plant pathology at the University of Minnesota. According to their pact, if one of them were to receive an offer to write a major textbook or to edit a series, etc., he would call the other exclaiming the honor that was about to be bestowed upon him, and in return the responsibility of the other was to say, "Don't do it, you damn fool," and then to talk the other out of taking on such a task.

Bob did contribute a number of seminal review articles, including a 1941 piece on biological nitrogen fixation with Dean Burk. Given that techniques were the cornerstone of Bob's career, it was fitting that he collaborated with Wayne Umbreit and J. E. Stauffer to produce the classic methodology book *Manometric Techniques*. Bob wrote a number of the chapters in the original volume and its revisions on techniques, use of radioisotopes, and use of stable isotopes. That volume was a valuable resource for many scientists.

Bob also wrote the chapter on biological nitrogen fixation for the classic textbook *Plant Biochemistry*, by Bonner and Varner. That book contained chapters from the leading experts across the spectrum of plant biochemistry and was reprinted in at least three editions. The graphical presentations of biological nitrogen fixation and the nitrogen cycle on earth have influenced and still persist among those teaching the subject today. Bob was never one to let his lectures go stale, and he frequently updated his notes and his lectures for Plant Biochemistry 621, which he taught for 44 years on Tuesdays, Thursdays, and Saturdays. For many years Heinrich Schnoes shared the course with him.

Bob's students always kept his handouts; they became the basis for the development of plant biochemistry courses around the country.

Bob served on the steering committee that developed the International Conference on Biological Nitrogen Fixation, which began in 1974 in Pullman, Washington, with subsequent meetings spaced at two or two-and-a-half years. The advent of the acetylene reduction assay and the increased cost of industrially produced nitrogen in response to the first oil crisis led to extraordinary expansion of interest and funding for nitrogen fixation research, and soon the meetings became the major scientific event for hundreds of people around the world with an interest in the field.

Bob was very active in professional societies and belonged to the American Society of Plant Biologists (previously the American Society of Plant Physiologists), the American Society for Microbiology, and the American Chemical Society. He also created one society on his own, which he called the "Fred, Baldwin and McCoy Society," and he selected the members, each of whom received a signed copy of the classic 1940 monograph by E. B. Fred, I. L. Baldwin, and Elizabeth McCoy on biological nitrogen fixation. Members included Gary Roberts (professor of bacteriology in Madison); Professor Yaacov Okon in Rehovot, Israel; Professor Johanna Döbereiner in Brazil, and me. Each recipient of this honor was deeply appreciative of having been chosen.

There was never a time when Bob was not an environmentalist, even before the word was adopted to describe those concerned about climate change and degradation of the environment. Even in the 1960s he was including information on the increase of CO_2 in the atmosphere and its potential long-term impacts on the earth's climate. At the local level he was very involved in maintaining the quality of Lake Mendota in Madison and the northern lakes of Wisconsin. His concern for the environment reflected his enjoyment of nature, and he took great pride in his cabin on the Wisconsin River, a 90-minute drive from his home in Madison. The closest town to the cabin was Muscoda, where everyone at Ike's Store knew him as Bob, not as Professor Burris or as a member of the National Academy of Sciences. During the early days of détente with the Soviet Union, Bob hosted a distinguished Russian scientist, whose departure was delayed until the State Department could find Muscoda and the surrounding area on the map. Bob also owned a farm, and in their retirement he and Katherine donated it to the university, a most generous gift from a couple who had already given so much. Bob and Katherine raised three children. Jeanne Burris Eloranta, now retired, was a librarian at Wisconsin-Madison. John Burris is president of the Burroughs Wellcome Fund. Ellen Burris

Heneghan was a forester before stopping to raise her family with husband Mike, an engineering faculty member at St. Cloud State University.

Outside of the lab Bob had a number of interests, including gardening, which led him to maintain large gardens at his home in Madison on University Bay Drive, and later at the cabin by the Wisconsin River. He and Katherine collected hand-blown glass paperweights during their travels and visited the workshops of leading artisans around the world. Late in his career Bob developed a penchant for jewelry making, perhaps stemming from his work fabricating gold seals for the mass spectrometer. Also, he had a variety of outdoor interests, including kayaking and canoeing. But his enduring passion was with photography, and he especially delighted in taking photos of his fellow scientists at national and international meetings. He had a number of cameras, but I believe he was most fond of his Hasselblad, which allowed him to shoot half frames. This connected his passion for photography with his frugal side, allowing him to capture two images in portrait format on each 35mm frame of film. Among his favorite photographs was a picture of a spring trillium, which he often used in cards or notes.

Bob evolved over the course of his career from the bright young intellectual risk taker, to a leading young voice in his field, to the world-renown scientist at the peak of his career, to the scientific leader and administrator, and, finally, to the aging elder statesman of his field. He progressed to each stage with grace and goodwill to all. Science is a competitive endeavor, and he was not without his critics and rivals. It is likely there were people he favored over others in his personal view, but one would never have known that from the fairness of his grant reviews or refereeing of submitted manuscripts. He never asked more of those around him than he asked of himself, and if he had a failing it was in the sense that he failed to live up to a favorite quotation of his that, "It was a poor teacher who was not soon surpassed by his students." Individually and collectively, students would agree that they spent their careers striving to raise themselves near his example. Bob passed away peacefully on May 11, 2010, in Madison, where he had lived, studied, taught, invented, mentored, and made discoveries since 1936.

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