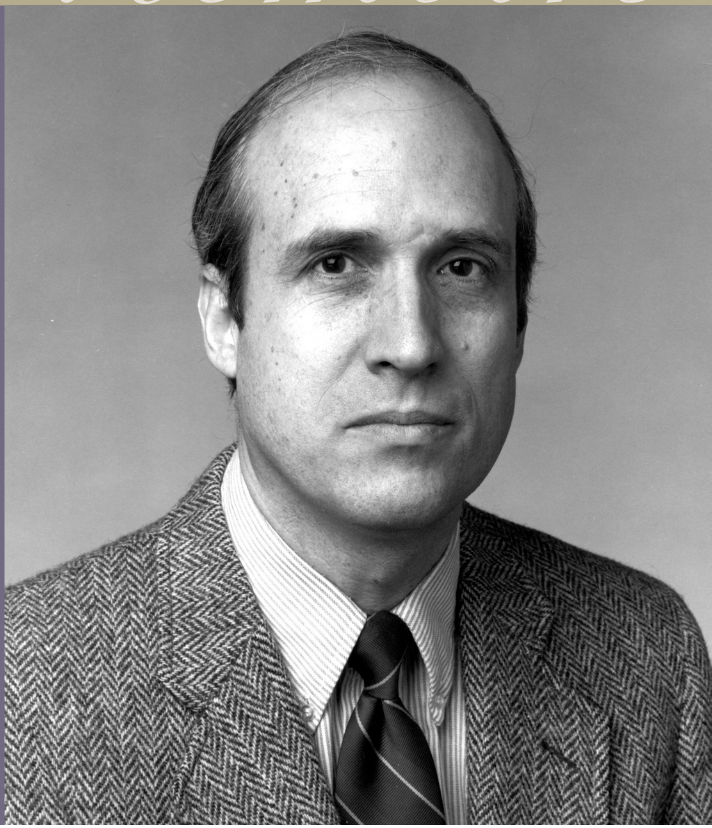
Robert A. Berner

1935–2015

BIOGRAPHICAL *Memoirs*



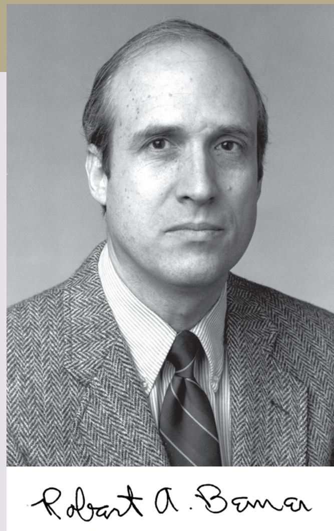
A Biographical Memoir by
Donald E. Canfield

NATIONAL ACADEMY OF SCIENCES

ROBERT ARBUCKLE **BERNER**

November 25, 1935–January 10, 2015

Elected to the NAS, 1987



By Donald E. Canfield

Bob Berner passed away on January 10, 2015, closing the final chapter of one of the most influential and colorful geochemists of all time. I had the pleasure of working as a Ph.D. student with Bob from 1982 to 1988, a period of particular innovation and transformation in his scientific career. In what follows, I will provide some brief recollections of Bob as a person and mentor, while taking full account of his contributions to science. The reader is also directed to Bob's excellent autobiography (Berner 2013).

Bob as a Person

Bob Berner, walking down the hall, his grey turtle neck half untucked at the back, with a big toothy grin; that's the way I will remember him. Especially the grin. Bob was, of course, a scientific legend. We'll get to that in a moment. But, he was also very much more than that.

Bob was fun, and he loved a party. He also loved a good meal and good glass of wine. Bob was a decided Francophile and learned the basics of the French language. He had a favorite Parisian café where he knew the chef personally, and they knew Bob's favorite meal and wine. Back in New Haven, Bob longed for Paris, and he would often invite the students to the Whitney Winery, near the Yale Campus, for Friday Happy Hour. The winery had an outdoor terrace that reminded Bob of outdoor Paris cafés, so we would usually sit outside. The outdoor terrace was a bit more expensive than the indoor bar, stretching a graduate student's budget, but Bob would inevitably pay the bill. If the mood was right, Bob would invite the stragglers to his house for a late dinner of escargot, pasta, and a bottle or two of red wine from his cellar, a decent wine, but not too good; inevitably though, the wine was better than we were used to and better than we could usually appreciate. As the night wore on, we might taste test between fine Japanese whisky and Scotch, and on rare occasions, we would greet the sunrise with breakfast and coffee.

Bob had a fast tradition surrounding the successful completion of a Ph.D. defense. The student, a guest, and the thesis committee were invited for a fine dinner at Bob's house, prepared by Bob and Betty, his wife. At dinner, a bottle of special wine was served suited to the student's taste and sophistication (I honestly can't remember the wine served after my defense, which says something about my level of sophistication). The dinner was a small, but extremely cozy affair, and after dinner, the party began in earnest when the other students and interested individuals congregated at Bob's for drinks, laughs, and a late night of poker and fun.

Bob also had parties outside of these thesis defense affairs. For me, a particularly memorable party revolved around the retiring of the family Opel GT and the introduction of "Harvey," the new family car, a Honda Civic. As I recall, "Harvey" was chosen as it was one of the few cars that could contain Bob's height. The party started before the guests arrived with James, the youngest son and sporting a new driver's license, driving the Opel around the block and entering the Berner driveway just as the odometer turned 100,000 miles. After we arrived, Bob, a serious musician, showed off his new sound mixer, and his latest composition called "Harvey and the 4 Bobs." The song was played on Harvey's tape deck with the driver's door open. Bob had mixed four tracks of his playing on various instruments, with Harvey's door alarm pulsing the final rhythmic beat. It was an instant classic.

In another memorable car adventure, Betty inherited a powder–blue Chevy Nova after her mother's death. The car was notable in that it had a radio, a first for a Berner family car. Not that radios were uncommon then, but as Betty relayed to me, "we were a bunch of tightwads". Anyway, Bob was thrilled by the radio, and just after receiving the car, and after its first trip to the Geology Department, Bob collected fellow graduate Ellery Ingall and myself for a drive around New Haven listening to an oldies radio station. Bob was in bliss. Indeed, Bob had a special fondness for "oldies" and especially old movies. As I recall, he was particularly fond of W. C. Fields, Laurel and Hardy, and Mae West.

Bob was fond of stories, and he had lots of them. Some of the stories related to members of his colorful family. We heard many times about Aunt Esther and Uncle Howard, both extremely large in size and personality. Bob often joked about his middle name, Arbuckle, referring to the disgraced silent movie actor and comedian "Fatty Arbuckle," but insisting that there was no relation. We also heard about colorful members of the geochemical community, generally involved in some sort of alcohol-fueled debauchery, and we heard about transformational people in his life.

I was particularly fond of his recollections of A. C. Redfield, of "Redfield ratio" fame. Bob spent two summers at Woods Hole during his Ph.D. studies, where Redfield was also visiting. Redfield took a special liking to the young Berner, and Bob told of many trips with Redfield in the local marshes discussing tides and marsh development— but also digging into the sediment and speculating on the nature of black staining in the smelly muds just below surface. As Bob points out in his autobiography (Berner 2013), these early experiences shaped much of his scientific career, especially Redfield's willingness to always try something new and to work across many disciplines.

Bob's personal history

Bob was born Robert Arbuckle Berner on November 25, 1935, in Erie, Pennsylvania, to Paul Nau Berner and Priscilla (Arbuckle) Berner. Bob grew up with his older brother Paul C. Berner, who also became a geologist. After a childhood of living in various places in the Midwest, Bob started his undergraduate degree at Purdue University, but switched to the University of Michigan, obtaining his undergraduate degree in geology in 1958, followed by a master of science degree in 1959, also at the University of Michigan. Of special note was a structural geology class taken during Bob's master's studies. Early in the course, Bob noticed a fetching fellow student on the other side of the classroom. After some nervous initial eye contact, Bob mustered the nerve to introduce himself to one Betty Marshall Kay, daughter of the famous geologist Marshall Kay. One thing led to another and Betty Kay became Betty Berner in 1959, Bob's partner and soul mate for the remainder of his life.

Now married, Bob and Betty moved to Cambridge, Massachusetts, where Bob studied geochemistry with Raymond Siever at Harvard University. Bob's thesis focused on the mineralogy of those strange black iron sulfide minerals, noticed by Bob and A. C. Redfield, in the marsh sediments of Cape Cod. Through this work, Bob was the first to structurally and chemically characterize a common iron sulfide mineral found in sediments. The description of this mineral gave Bob his first *Science* paper (Berner 1962), but he was not allowed to name the mineral because he found it forming on a rusty shopping cart in the Mystic River, and this was not considered a natural occurrence. Thus, instead of Sieverite, the mineral was named Mackinawite a year later by a group of scientists at the USGS who found the mineral in a cave.

Bob, together with Raymond Siever and Kevin Beck, was the first to characterize the chemical composition of marine sediment pore waters utilizing the "Siever squeezer" (Siever 1965). This was a bulky metal contraption with a screw and piston that forced the pore water out of sediments as the screw was continuously turned. In my time, a second generation of this squeezer, fueled by gas pressure, occupied various spots on my lab bench, occasionally holding down loose papers. I hope it has ended up in a museum someplace. While Siever was Bob's official advisor, I feel that his greatest inspiration was Bob Garrels, who held a position as a professor in civil engineering at Harvard University at the time. I'm not sure how much contact they had, but they had some, and Bob's early interest in chemical thermodynamics certainly had a root in Bob Garrels' interests and work. Bob liked to tell a story where he and Garrels were together on a cruise, and Bob was struggling to measure the pH of seawater with an old Beckman model G (I think) potentiometric pH meter. Bob was having trouble getting the thing to work and asked Garrels for advice. Garrels promptly replied, "you are best just throwing it overboard." I'm not sure what Bob actually did.

Finishing his Ph.D. in 1962, Bob spent the next year on a Fellowship at The Scripps Institution of Oceanography. While at Scripps, Bob finished work on sediments he had collected the year before, while still a graduate student, on a Scripps cruise to the Gulf of California (more on this below) and worked with Mel Peterson on determining rates of lacustrine dolomite formation. After Scripps, Bob moved to an Assistant Professorship at The University of Chicago in 1963. While commuting from work, a rock thrown through an L-train window shattered glass over Bob. This caused Bob only minor injuries, but a woman sitting next to him was seriously injured. This incident convinced Bob and Betty that the neighborhood around the university was too dangerous, and Bob was all too happy to begin as the "young Turk" (as Karl Turekian once relayed to me) assistant professor of geochemistry in the Geology Department at Yale University in 1965. Here Bob stayed until his retirement in 2007. Once settled at Yale, Bob and Betty got on with having a family and raised their three children, John, Susan, and James.

Bob's scientific legacy

With this brief background, we now put structure into Bob's career and accomplishments. Broadly speaking, Bob's scientific career can be divided into several main areas of concentration that typically overlapped in time. Thus, with Redfield as an example, Bob worked on many different types of scientific problems and was always willing and eager to move into new scientific areas as necessary to address his scientific curiosity. These areas of concentration were always pioneering, and more than one of them developed into whole research fields of their own. Bob's career is also punctuated by a series of key publications, and these, together with his main areas of scientific endeavor, are outlined in Figure 1.

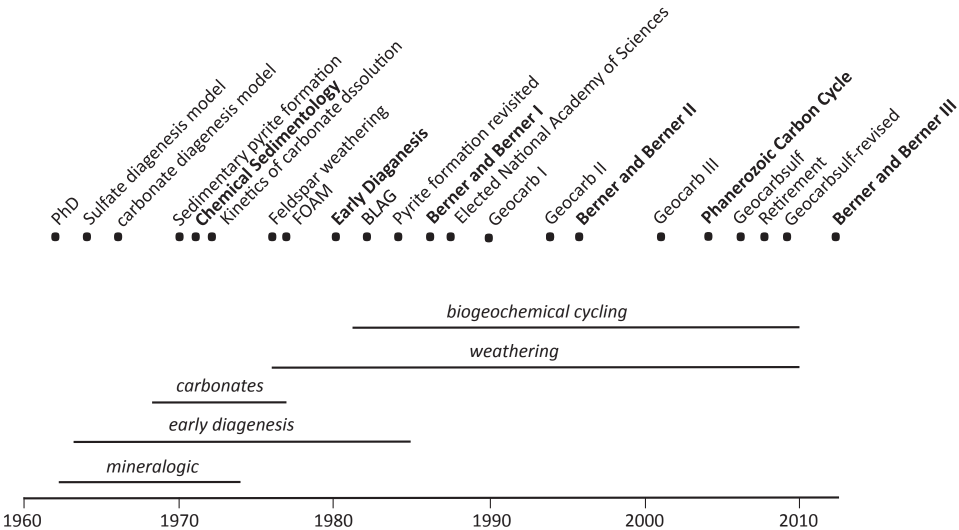


Figure 1. Key publication milestones along with time periods panning various areas of concentration of Bob Berner's scientific career.

We begin with Bob's "mineralogic" area of concentration. The period marking this area of work begins with his Ph.D., and we might consider this area of work to include both mineralogical and thermodynamic exploration. I believe that we see the influence of Bob Garrels here, especially with Bob's foray into thermodynamics. Within this area of concentration, and as mentioned above, Bob identified one of the main mineral products of sulfate reduction, the iron sulfide mineral mackinawite (Berner 1962). Bob also measured the solubilities of geothite and hematite in water and found goethite to be metastable under normal Earth surface conditions (Berner 1969). He determined the thermodynamics of Fe sulfide mineral solubility, determined the solubility product of calcite and aragonite, and also developed the first working sulfide electrode (Berner 1963). Bob also worked on other electrode ideas, including a ferrous Fe electrode (it did not work), and these experiments could still be found tucked into various lab cabinets during my time as a Ph.D. student at Yale. In other work, Bob experimentally showed that pyrite could form in sediments from the reaction between iron sulfide (his mackinawite) and elemental sulfur in a relatively slow reaction (Berner 1970). Bob's earliest award emerged from work in this area, the Mineralogical Society Award of the Mineralogical Society of America (1971). Bob joked that the committee for this award couldn't decide between the top two candidates and awarded it to him as a compromise. I seriously doubt this is true, but it is one of Bob's many stories.

As noted above, Bob spent some of 1962 working on sediments collected the year before in the Gulf of California. The focus was on sulfur chemistry, and in the resulting publication one could see Bob beginning to view sediment diagenesis in a dynamic quantitative way (Berner 1964). With mass balance calculations, he reasoned that diffusion must be a critical process supplying sulfate to the sediments, and furthermore, the shape of the sulfate profile indicated that the rate of sulfate reduction must decrease with increasing sediment depth. These were profound insights indeed, culminating in a remarkable paper published later in 1964 with the non-assuming title "An idealized model of dissolved sulfate distribution in recent sediments" (Berner 1964). This paper was the first application of differential equations to solving a problem of sediment diagenesis, and together with Bob's Gulf of California paper from earlier in the year, marked the beginning of early sediment diagenesis as a scientific field. This work also represents the beginning of Bob's focus on the kinetics of Earth surface chemical processes, an approach that informed the remainder of his career. In this Bob was also a true pioneer. Due to his early interest in chemical thermodynamics, Bob saw clearly that while all chemical systems tend towards equilibrium, the rates at which this happens governs, to a large extent, the chemistry of the environment.

Bob and his students and postdocs continued to lead research into early diagenesis for the next two decades. Some of this work has already been mentioned, and additional high points include work with postdoc Chris Martens showing, for the first time, the major controls on methane distribution in sediments[1] and his book *Early Diagenesis* (Berner 1980). This book, a citation classic, and Bob's most highly cited contribution to science, formally develops the principles behind modeling early diagenetic processes. *Early Diagenesis*, written nearly 40 years ago, is still the go-to book in the field. In the middle to late 1970s, there were several students and postdocs interested in early diagenesis in residence at Yale. These included Bob Aller, Marty Goldhaber, Joe Westrich, Jeff Rosenfeld, J. Kirk Cochran, and Chris Martens. This band of like-minded scientists, including Bob, formed a group known as the Friends of Anoxic Mud, or the FOAM group. Working as a team, they published a full diagenetic exploration of the "FOAM" site in Long Island Sound.[2] This paper is a classic, immortalizing the FOAM site and demonstrating that world-class science can also be fun.


Bob in the lab, probably 1990's.

After the publication of *Early Diagenesis*, Bob's personal involvement in early diagenetic research waned, and he was pretty much finished with this field by about 1985 after the publication of a pair of landmark papers from Joe Westrich's thesis exploring carbon and sulfur dynamics in sediments (although some of his students—like me for example—continued with diagenesis-related themes into the 1990s). These papers showed how organic matter should properly be viewed as a series of compounds with variable reactivity to microbial decomposition; in other words, organic matter ages as it decomposes.[6]

Before we go further, we need to go back and recognize that early diagenesis was not the only interest Bob developed in the 1960s. He also developed a keen interest in carbonate chemistry and in particular, the kinetic processes governing the formation and the dissolution of calcium carbonate minerals in the oceans. This interest may have been sparked during Bob's year-long postdoc at Scripps where, as already noted, he worked with Mel Peterson on rates of dolomite formation (a calcium/magnesium carbonate) in a lacustrine environment.

There were a number of outstanding problems in carbonate chemistry at the time, and one of them was understanding how carbonate concretions formed in sediments. Concretions are roundish massive carbonate precipitates that are common in the geologic record, but uncommon today. In a decisive paper in *Science*,[3] Bob reported some of his experimental results that linked their formation to the decomposition of organic matter in sediments, an idea that is still in vogue today. Another issue related to the reasons behind the so-called carbonate compensation depth (CCD) in the oceans. Basically, if the calcium carbonate formed by organisms in the surface waters of the oceans sinks far enough into the ocean depths, it will begin to dissolve, and below a certain depth, the CCD, none remains, forming a kind of deep-sea snow line.

With John Morse on board as Bob's first real Ph.D. student (an earlier student, Roger Doyle, did his Ph.D. work with Bob, but obtained his degree in biology at Yale), they devised an ingenious experimental approach to explore how the calcium carbonate dissolution rate responds to various degrees of calcium carbonate undersaturation. Decisive in this work was exploring both analytically and theoretically how the surface of carbonates changed during the dissolution process, thus providing an explanation for the experimental results. This was ground-breaking stuff, and their insights revealed that the CCD was the depth where carbonate dissolution rates balanced the carbonate flux to the sediments, but also that dissolution began much higher in the water column at a depth called the lysocline. Bob's interest in carbonates waned by the close of the 1970s, but the insights gained into the processes controlling carbonate dissolution proved critical in informing another of Bob's emerging scientific interests.

Indeed, in the early 1970s, Bob became interested in exploring the surface properties of minerals during weathering. He began by focusing on the common rock-forming mineral feldspar, where he and postdoc Radomir Petrovic employed the (then) state of the art technique of X-ray photoelectron spectroscopy (XPS) to explore the surface chemical properties of feldspar under weathering conditions. Combining XPS with SEM, Bob and Radomir concluded that weathering begins at crystallographic dislocations in minerals (Petrović 1976). Thus, this new theory viewed weathering as a physical-chemical process, governed by nano-scale processes occurring at the mineral surface. These insights opened a whole new way of thinking about the factors governing the rates of weathering reactions in nature. In various ways, weathering continued as a theme in Bob's research for the remainder of his career, although, as we will see below, his approach became more global and evolved in other important ways.

It is clear that Bob's work through the 1960s and 70s opened up many new ways of thinking about the physical-chemical (and biological, although this gains more importance later) processes impacting the chemistry of the environment, with a focus on the factors controlling the rates. Bob was nearly alone in opening up these new lines of inquiry, and they were made possible by an intense curiosity but also by some pretty impressive mathematical chops. Bob never viewed himself as a mathematician, and perhaps he was not one in the traditional sense, but he used math to elegantly express and to quantify all manner of (bio)geochemical processes in nature. Indeed, Bob's unique quantitative approach to science informed his first book, titled *Principles of Chemical Sedimentology*. This book presented sedimentology as never seen before. Instead of focusing on grain-size analysis, cross cutting relationships, and the building of sedimentological models of deposition, Bob introduced a quantitative description of sedimentological and geochemical processes. He sometimes quipped that the book was never really understood by its target audience, practicing sedimentologists. Perhaps not, but it was widely appreciated by the geochemical community and ushered in a new way—Bob's way—of thinking about sediments and the chemical/biological processes affecting them. It has also been cited nearly 2,000 times.

In 1981, Bob Garrels and Abe Lerman published a paper that caught Bob's attention. In this paper, Garrels and Lerman used the isotope records of carbon and sulfur to calculate the histories of the carbon and sulfur cycles over the last 600 million years. Bob was intrigued, and he also joked that in the Garrels and Lerman model things were calculated backwards such that, in Bob's words "the rivers were running upstream!" This is not to be taken as a criticism but a realization that there was much more that could be done with this type of modeling. Thus began the final chapter in Bob's scientific journey: global biogeochemical cycling. This work would occupy Bob (although not completely) for the remainder of his scientific career.


Bob and Rob Raiswell, Whitby, N. Yorkshire, UK.

Several decisive factors converged at about this time. For starters, Bob invited Garrels to come to Yale as a visiting professor, which he did for several months a year over about a four-year period. It must have been incredibly rewarding for Bob to have Garrels back in his scientific life. Also at this time, Rob Raiswell from the University of East Anglia (at the time) spent a one- year sabbatical leave at Yale, followed by summer visits over many years. Finally, the Berners, with Betty taking the lead, were compiling a massive data base on the chemistry of rivers, lakes, and the ocean. This research led to the publication of *The Global Water Cycle* in 1987 (followed by a second edition in 1996 and a final edition in 2012).

One could argue that Bob's first paper on global biogeochemical cycling was a careful assessment of the global burial fluxes of organic carbon and sulfide sulfur in marine sediments (Berner 1982). This paper, together with the compilations of Berner and Berner, established many of the key parameters that Bob would use in subsequent modeling. Indeed, carbon and sulfur burial fluxes were put to good use in a remarkable paper by Bob and Rob Raiswell[5] that reimagined the model of Garrels and Lerman, but with rivers running downhill this time! This paper provided many new insights into the evolution of ocean chemistry and the global cycling of carbon and sulfur through time.

This leads us to Bob's most remarkable paper from this period. Together with Bob Garrels and Tony Lasaga (then at the University of Pennsylvania), Bob constructed a model hindcasting the concentrations of atmospheric CO_2 over the past 100 million years.[4] This paper was dubbed by Wally Broecker (a pioneering climate scientist from the Lamont Doherty Geological Observatory) the BLAG (Berner, Lasaga, and Garrels) model, and the name stuck. The idea that one could even model the history of atmospheric CO_2 was brash and bold, but the "BLAG" model was so logically well-founded that it has informed virtually all subsequent attempts to model the history of Earth's geochemical evolution. The model required identifying all processes that contribute CO_2 to the atmosphere and those that remove it, assigning drivers (like plate tectonics) and kinetic expressions to the controlling processes. None of this was trivial, and I do not believe that this model would have been possible without Bob's previous contributions into the kinetics of mineral weathering reactions and Betty and Bob's compilation of the chemistry of rivers and the oceans. In its initial version, the model contained no biology, but in subsequent versions, reaching back into the whole of the Phanerozoic Eon, biology was introduced and even became an important driver for many of the transitions in atmospheric CO_2 concentrations.

The paper landed with a huge impact. Even those working on recent atmospheric CO_2 dynamics and climate change took notice. As mentioned, Wally Broecker took immediate notice and gave the paper its name. However, I should also expand on what Wally said in his letter to Bob following the publication of "BLAG:" "Long live BLAG, but as you know, I am a notorious backslider!" Bob was delighted by this letter and shared it with us all. Truth be told, Wally did indeed backslide, but that's another story for another time.

BLAG was a big paper requiring an enormous amount of theory to inform the modeling approach, as well as a great deal of sensitivity analyses, etc. One would think that such a paper would take quite some time to write. But, here is one of the most impressive things about Bob; he was a remarkably efficient writer. Indeed, it typically took him just a weekend to write a draft of a paper. He would sit in his favorite chair in his bedroom, listen to classical music, and write. BLAG, being much more complex, did take longer to write, but the whole thing was finished in the span of a week! What most of us wouldn't give for such a clear and concise scientific mind.

The BLAG model evolved into the GEOCARB model, which considered the history of atmospheric CO_2 over all of Phanerozoic time (Berner 1990). There were many iterations to the GEOCARB model, including reconstructions of atmospheric oxygen (Berner 2001) as well as the evolution of the major ion chemistry of seawater (Berner 2004). Biology was also introduced, including the evolution of land plants and their influence on weathering (Berner 1993; Berner 1997), as well as the influence of oxygen and CO_2 levels on the ability of the enzyme RUBSICO to fractionate the isotopes of carbon (an important input parameter in the model). There were many other innovations as well (e.g. Berner 2001). Finally, the GEOCARB model morphed into GEOCARBSULF (Berner 2006), where the behavior of sulfur was now highlighted. The final iteration of GEOCARBSULF was published in 2009 (Berner 2009), and this can be considered Bob's ultimate statement on the history of atmospheric and ocean chemistry.

Bob worked on many other things after beginning his journey into the biogeochemical modeling of Earth history. In the beginning, as noted above, he was still pursuing his interest in sediment diagenesis. However, as Bob's work became more focused on Earth history modeling, his Ph.D. students became more focused on experimental work predicated by fundamental uncertainties in the cycling of elements, as illuminated through Bob's modeling. Thus, student projects included issues such as the influence of land plants on weathering, the oxidation kinetics of fossilized organic matter with oxygen, and the biogeochemical cycling of phosphorus in the oceans. Each of these projects produced fodder for Bob's models, and importantly, novel insights into the biogeochemical workings of our planet.

As many of these processes require biology, Bob began to identify himself as a "geobiologist" demonstrating a slow, but important shift from earlier days, and earlier modeling, where inorganic geochemical processes were paramount. By embracing biology, Bob was once again a pioneer in exploring the interface between biological and geological processes.

Bob as a colleague and mentor

As described above, Bob was a very social person. He loved people and he loved a party. Therefore, it might seem paradoxical that in his science, he had very few colleagues and wrote a large percentage of his papers alone. I believe that there are at least two reasons for this. One reason is that Bob didn't really need colleagues that much. His mind was always very clear on which direction his science was moving, and he was a master at penetrating new scientific fields as needed.

The other reason is that Bob was fundamentally impatient. When he wanted input, he wanted it now. Probably all students working with Bob remember coming to their desk and seeing a series of pencils and pens pointing towards the center of a piece of paper with "SEE ME NOW" in capital letters. I also imagine this was so with colleagues; Bob's mind was moving so fast that he couldn't wait for others to both comprehend what he was after and to respond.

Bob did, however, have some good colleagues, and outside of his Ph.D. students and postdocs, these included Bob Garrels, Tony Lasaga, David Ward, Rob Raiswell, and David Beerling. Of these, the most important were Rob Raiswell in the 1980s and David Beerling in the 1990s and onward. Rob Raiswell was a critical colleague as Bob transitioned into Earth history modeling, and he helped to define many of the relationships between carbon and sulfur dynamics that Bob would use in his models. David Beerling is a specialist in the evolution of plants and their role as geobiological agents. This fit perfectly with Bob's evolving embrace of biology in his modeling.

If one word was to describe Bob as a Ph.D. advisor, it would be "inspiring." As a student of Bob's, you felt you were a part of something extraordinary. Important ideas were crowding the atmosphere, and you couldn't help but be affected by the energy. However, Bob was NOT the person to go to for advice on a laboratory procedure or details of experimental design. He expected his students to figure these things out on their own. Also, while Bob's door was always open, discussions quickly turned to his work, or to what was otherwise on his mind. However, each of Bob's students presented their work in the weekly "Sediment Seminar" two to three times a year, and in this forum, Bob focused on your work like a laser beam. In this circumstance you got the best feedback imaginable.

Bob was also very proud of his students and cared for them both as scientists and as people. I recall a couple of rather telling instances. In the first I was nearing the completion of my Ph.D. and was having a great deal of difficulty landing a job. I went into Bob's office one day after my n^{th} rejection notice and said something rather glib like, "maybe I should give all this up and open up a bike shop in the Adirondacks." Bob paused for a second, gave me a stern look and said very deliberately: "I don't ever want to hear you say that again." Bob liked to brag about students, and I saw this first hand during a break from a National Academy of Sciences-sponsored meeting on the carbon cycle, where I was together with Bob and the late organic geochemist John Hedges. I was still a postdoc, and Bob and John were talking about current Ph.D. students. Bob, with his typical enthusiasm, said, "Oh yeah, I have a great crop of students now, probably the best ever, and I think that X is probably the best I've ever had." John looked at me, and

Bob looked at us both, sputtered and quickly changed the subject. Although somewhat awkward, this scene shows how much Bob thought of his students and liked to brag about them!

But yes, while Bob wasn't a typical modern-day "hands-on" advisor, he cared for his students deeply and inspired them to be the absolute best that they could. The inspiration didn't come from any concrete demand from Bob, it came from our appreciation for Bob's excellence and his caring and our desire to not disappoint him. In many ways, Bob was the ideal advisor.

A few additional thoughts on Bob's approach to science

Bob had no scientific agendas and could change his mind if the reasoning was good and the arguments were compelling. I remember one instance. As a second-year graduate student I was taking Bob's "early diagenesis" course, and Bob had just presented his model for sulfate reduction in marine sediments. At the same time, Bob's graduate student Bernie Boudreau was also visiting this model, but he was troubled by the fact that under some circumstances the model could generate negative concentrations for sulfate. Bob had noted this too, but he reasoned that it wasn't a serious problem, and Tony Lasaga, mentioned above, even published a paper rationalizing why this was okay. Bernie, however, was not convinced, and in the evening after Bob's lecture, Bernie showed me a new model with boundary conditions different from Bob's and where sulfate did not go to zero. Bernie's model also produced very different predictions for sulfate reduction rate compared with Bob's original model. Bernie decided that he would tell Bob about this the next day.

I can't remember if I was stalking, or if it was by chance, but I do remember walking past Bob's office when in a loud voice he said, "You mean to tell me that everything I've done for the past 20 years is wrong!?" Bernie said, "Yes, Bob," and I scuttled away. Bob insisted that there be an arbitrator and invited Yale's physical oceanographer, George Veronis, to hear Bernie's arguments. George listened carefully and agreed that Bernie was right. The next day, with a smile on his face, Bob came into the classroom and said something like, "Everything I told you yesterday was wrong. Bernie just came up with a proper sulfate reduction model." He proceeded to outline what Bernie had done. Science won and Bob was happy.

I end with some final words about publications and publication strategy. Bob Berner's publication record would be the envy of almost anybody in science. He ranks among the most highly cited geochemists, and his most-cited contributions have garnered thousands of citations each. However, unlike in current publication trends, most of Bob Berner's most-cited and well-known contributions list him as first author, and most commonly, single author. Bob believed deeply that he would only be involved with publications to which he made a major if not primary contribution. He was not interested in amassing publications. Indeed, Bob is not even a co-author on the majority of his Ph.D. students' publications. Unlike modern trends where some scientists often seek authorship on papers where they have had minimal contribution, when Bob Berner was an author on a paper, the paper truly represented part of his scientific oeuvre.

Concluding remarks

In many ways, Bob Berner was bigger than life. He was a huge personality with an unmistakable presence and whose scientific insights fundamentally defined modern low-temperature biogeochemistry. The scientific world would have looked very different without him. Bob was well-loved, well-respected, and highly decorated for his scientific accomplishments. Bob was probably most proud of his election to the National Academy of Sciences, but he also garnered much other scientific recognition including the Mineralogical Society of America Award (as noted above), the V. M. Goldschmidt Award of the Geochemical Society, the Vernadsky Medal of the European Geosciences Union, the Benjamin Franklin Medal in Earth and Environmental Science from the Franklin Institute, the A. G. Huntsman Award for Excellence in the Marine Sciences, the Murchison Medal of the Geological Society of London, the Arthur L. Day Medal of the Geological Society of America, and a Doctor Honoris Causa from Université Aix-Marseille III.

Bob enjoying his 70th birthday dinner.

We miss Bob terribly, but he left much to remember. Indeed, Bob is a rare scientist who shaped the landscape of current knowledge and left a legacy, both personal and scientific, that will live long into the future.

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