

the chemists club

Spring 2017



We Can Do It Ourselves

R. Stephen Berry talks macro-to-micro, thermodynamics, and environmental policy

R. Stephen Berry converses with Irene C. Hsiao on 1 March 2017

R. Stephen Berry, James Franck Distinguished Service Professor Emeritus of the Department of Chemistry and the James Franck Institute, MacArthur fellow, Guggenheim fellow, Sloan fellow, American Academy of the Arts and Sciences fellow and former Vice President, member and former Home Secretary of the National Academy of Science, member of the American Philosophical Society, foreign member of the Royal Danish Academy of Sciences, former Special Advisor to the Director for National Security at Argonne National Laboratory, is the author of many articles and books, including Understanding Energy: Energy, Entropy and Thermodynamics for Everyman (1991), Water and Energy as Linked Resources (co-author, 1978), TOSCA: Optimizing the Mix of Fossil and Nuclear Plants from Total Social Cost (1979), and Physical Chemistry (1980). Still actively seeking answers to a broad range of questions daily, he also plays the recorder and collects art.

What are you working on these days?

The main thing I'm working on is using an approach we developed to connect the micro and macro approaches to physical problems by examining phenomena that are well-described by the macro picture but break down for very small systems. We got into this because we were studying atomic and molecular clusters for a long time. Some early simulations showed—and we verified and explained and explored the phenomenon—that very small systems, clusters of atoms or molecules, 10-20 atoms, do not obey what had been accepted as a universal rule, the Gibbs' phase rule, discovered by J. Willard Gibbs at Yale. It is absolute doctrine—the simplest formula in all of natural science. It just says that the number of degrees of freedom that you can vary is given by the number of individual components in the sample minus the number of phases (solid, liquid, gas, or different forms of solids), plus two. That was a great discovery, and it's a great mystery.

Where does the two come from?

The two gives the right answer! It's this rule that says that water—liquid and ice—can be in equilibrium at only a single temperature at a given pressure. It's exactly 32 Fahrenheit or 0 Celsius at 1 atmosphere of pressure. Change the pressure, and you have to change the temperature. But it turns out that very small systems violate this rule, and you can have solid and liquid in observable amounts over a range of temperature and pressure. And so we were able to explain this on the basis that the Gibbs' phase rule, which is valid for large systems, is specifically a consequence of large numbers.

When numbers are large—you're typically dealing with 10^{10} to 10^{20} atoms (a grain of sand is 10^{20} atoms)—although formally the passage from solid to liquid is smooth, it actually occurs over such a tiny range that you couldn't possibly observe anything except at the point where the two are in equilibrium. The Gibbs' phase rule is exact in the sense that the smooth change between phases is in an unobservably narrow region. But when you have very small systems, then the difference in

free energy between the solid and liquid when they're in disequilibrium can be small enough that you can observe both phases—you can have water crystals and water liquid in equilibrium in a grain, in a tiny particle, over a range of temperatures and pressures, which the Gibbs' phase rule says you can't do.

We worked on a variety of things and figured out a basic reason for this, and then we got around to asking a question that we could have asked much earlier on: what determines the largest size of the smallest system for which you could actually observe the violation of the Gibbs' phase rule? What fixes that approximate size depends on what your capabilities are for making observations. But if you say, for example that I could see 10% of the unfavored form in the presence of the favored form, that tells you that you could see the violation of the Gibbs' phase rule with up to 75 atoms, but you wouldn't be able to see it with 100 atoms. It's too sharp. The range where the solid and liquid coexist is too narrow to be observable. The governing factor that determines that maximum observable size is the inverse of the entropy change between the phases. When you go from solid to liquid, the entropy increases. One over the entropy is the physical quantity that determines how big is the biggest you can observe.

At the last session of [physics professor] Leo Kadanoff's 75th birthday meeting, someone from Urbana said, "You know, one of the big open questions in physics is matching the macro and micro approaches." We had just recently published our first paper on this maximum size phenomenon, so I had this sudden realization that we had found a way to address the question of matching the macro and micro by asking where the macro broke down. We found it for one phenomenon, but there are lots of phenomena for which the macro description doesn't work for small systems. For example, the solid form of almost all pure substances is a crystal. But when you get to very small systems, you don't get crystals; you get polyhedra or icosahedra. An icosahedron is not a crystal. It's not infinitely repeatable. You can have 13 atoms, for example, with 1 in the center and 12 around it in the form of an icosahedron, or you can have a second icosahedral cell that gives you a total of 55 atoms, and you can build icosahedral shells out and out and out, but those aren't crystals.

A crystal doesn't have a center. At what size does any given substance take on its crystalline bulk form instead of its small polyhedral structure for clusters? If you have a crystal and you add 1 or 10 or 100 more atoms or molecules to it, it goes linearly with the amount of stuff. But when you have a polyhedron, you don't have monotonic behavior because you get closed shells, and you add things onto the outside of the closed shells, and the behavior is non-monotonic. When the structure depends sensitively on the number of things, it isn't a monotonic dependence anymore. When does the monotonic behavior break down? These are open questions.

Most small systems melt at temperatures well below the bulk temperature. At what size do they start to melt like the bulk? Martin



I wrote a letter to Mayor Richard J. Daley which started out, "Dear Mayor Daley, you live like a pig."

How could you say that?

It was so elegant. My interest in it only arose when I moved to Chicago in 1964. And it was a consequence of my dislike—my actual anger—at the level of pollution, air pollution especially, in Chicago. It was a filthy city! The air was gray, and it smelled bad. It was toxic. It was really awful. And I got very angry! I thought I was psychologically prepared for it, but when I got here and lived in it, I realized I wasn't. I actually wrote a letter to Mayor Richard J. Daley which started out, "Dear Mayor Daley, you live like a pig."

Did he write you back?

The letter got more rational after that. I said I had heard that there were things being done to try to address the pollution issue, but I didn't see any signs of it. I had the intuitive sense to send a copy of it to our alderman, who was Leon Despres. The reply I got was an invitation to me and to Alderman Despres to visit the city's air pollution facilities.

Were there really facilities for air pollution?

There was a laboratory. And it was so clean and neat and elegant, it was clear nothing was happening. I began to get very involved as a citizen at a time when there was an actively growing interest in addressing the environment. In the 1960s, there was a whole societal tipping point, and environment and pollution changed from being for nerds only to being politically acceptable. It culminated at the end of that decade in the Environmental Protection Act and the Clean Air Act. But I got involved with citizens' groups, even testifying at the city council. It was pretty interesting.

They said they didn't have enough staff in the environmental department to monitor air pollution sources, so we made a proposal to train volunteers. They wouldn't have any enforcement authority, but they could report to people who did. So we proposed this at a meeting at the city council, and they carefully scheduled so we were the very last people to speak that day. There were only three aldermen left, one who was chairing the meeting—a charming Irishman who later went to prison (he was actually a friend of Mayor Daley, but when he was convicted, Daley wouldn't stand up for him at all)—Mike Bilandic, who later became mayor, and Leon Despres. I made the proposal, and Bilandic said, "I think that's a really dumb proposal." But this Irishman from the north side winked and looked at me and said, "I think it's a pretty good proposal!" Whereupon, Bilandic said, "Hey, that's a good

Is there a similarity between gallium and tin?

Eh, both weird! The basic big question which I find challenging for any given phenomenon is, how do you approach the macro-micro question by asking where the macro breaks down—and why?

How long have you been working on the macro to micro problem?

A dozen years at least. I can look it up.

So hardly your whole scientific career—

Oh, I've been working on all sorts of things. For example, one area that I worked in for many years, though I'm not working on it now, though I certainly haven't lost interest in it, was the one area that I knew that I would never study! Absolutely!

What is it?

Thermodynamics! Because when I finished graduate school I knew absolutely for sure that subject was completely finished. There was nothing more to be done! And it was lovely, and it was elegant, but it was finished!

idea!" But it never happened. Anyway, it didn't need to, because the federal law changed!

About that time, I realized if you wanted to address the problem, you needed to do more than put precipitators on power plants. You had to somehow use energy more efficiently. So we began by looking at how energy was used. The first thing we did was a study of the energy used to produce the automobile, starting with the ore in the ground and ending with the final disposal. We weren't looking at the transportation aspect; we were looking at the manufacture and the disposal. We looked at each step and compared the actual energy and free energy with the ideal thermodynamic limit with the idea that where the difference was greatest, you had the greatest leverage for technological change. So that was the first study of what became life cycle analysis. And we went from looking at the automobile to looking at all sorts of things, comparing plastic and paper bags, for example, and people moved to Argonne and began doing it regularly there.

We published quite a number of papers on these analyses—actually the very first one we did, the automobile, I submitted to *Science*, and they wouldn't publish it, so it came out in the *Bulletin of Atomic Scientists*.

How did you get the information to the people who needed it?
Oh, we would talk to people in the industry.

They took your suggestions?

Yes, they were interested. One time somebody asked, "Why are you comparing the actual energy and free energy with the ideal thermodynamic limit, which is based on an infinitely slow process? Who would wait for the delivery of a car by a manufacturer who makes his cars reversibly?" And that raised a very interesting question in my mind. In traditional thermodynamics, you use thermodynamic potentials to determine the ultimate best performance that you can get. You use different potentials for different kinds of processes. But there's a whole class of thermodynamic potentials that are based on an infinitely slow process. And the question that was stimulated by that challenge was, is it possible, or under what conditions was it possible to define and construct and evaluate the analogue of the traditional thermodynamic potentials for a process that's constrained to operate in a finite time? This led to our asking questions about existence—we were proving existence theorems—and developing methods to construct these finite-time thermodynamic potentials. It has become a whole field of thermodynamics, finite-time thermodynamics.

I have spent many years working on thermodynamics.

Was there something else you thought you were going to work on instead?

I did my thesis on electronic structure calculations. I always was interested in both experiments and theory. One of my favorite experiments was colliding positive and negative ions at low relative velocities and watching them neutralize as the electron jumped from the negative ion

to the positive one. You can see it because the electron jumps, for example, from an oxygen negative ion to a sodium positive ion, and it goes to an excited state, so the sodium emits light. You can detect the light with photocells. We had two beams that went into a magnetic field, which bent them 180 degrees so they went in one direction and came out the opposite direction, arranged so they merged, and the relative velocity was very low. I always have loved that experiment!

So it slows them down?

No, they're going at almost the same speed. It's how fast they're moving relative to each other that matters. That [experiment] came at a time when I would go out with my graduate students and postdocs on Fridays for lunch. One of the places we liked to go was a Chinese restaurant on 63rd Street called Tai Sam Yon. So we named the apparatus Twin Accelerating Ion Sources Analyze Magnetically Yielding Optically-Excited Neutrals—TAISAMYON! And we put that in the paper when we published it!

Anyway, I've done a lot experimentally and theoretically in things like atomic collisions and atomic photoionization. And for a while I got contaminated by biology—I collaborated with Karl Freed and Tobin Sosnick on some protein work. We looked at things like how certain kinds of things, like exposed hydrogen bonds on the surface of the protein, act as an attractive site for pulling things together.

What comes first for you, theory or experiments?

When I was a graduate student, I was trying to do both, but my thesis was all theoretical because there was a problem [with] the apparatus for the experimental work I was trying to do. I foolishly was trying to make my apparatus out of glass instead of metal, and I was able to have the glassblower at Harvard make the apparatus. He showed it to me when he finished it, and all he had to do was anneal it overnight, but the annealing thermostat didn't work, and the apparatus melted. I came in the next morning to pick it up, and instead of being round it was sort of flat.

And you gave it up immediately?

Yes, exactly. I got so discouraged I decided I would just do a theoretical thesis. But actually I had a temporary instructorship after I finished my PhD at Harvard, and in that period, I actually did experimental spectroscopy in collaboration with a man named Bill Klemperer, a longtime friend since graduate school—and Stuart Rice! They were looking at the vapor of alkali halides like sodium chloride in infrared, and there was one discrepancy between what they were finding and a much, much earlier paper from the 1920s about UV spectroscopy, so I did the UV experiment and found it really was consistent with their infrared work, so we were collaborating even in the 1950s.

How is it to have a working relationship for such a long time?

We haven't written a paper together in awhile. But we will!

Why worry? If it's not an asteroid that makes us extinct, we can do it ourselves!

You didn't always intend to solve practical problems with your science, did you?

I was never averse to it. But I think it was the environmental challenge when I came to Chicago that really got me interested in that whole aspect of using science. One of the people I worked with was David Currie from the Law School. [Once] somebody in the administration decided there should be a public debate about controlling air pollution. So they asked [law professor] George Anastaplo and me to be on one side, and on the other side, there was Harold Demsetz, Ronald Coase, and Milton Friedman. It was a formal debate—resolved that Commonwealth Edison should be required to put precipitators and air pollution controls on their power plants. We had the affirmative, and they had the negative. We knew how they would argue, but they didn't know how we would argue. That we knew. But there was another advantage we had that we didn't realize—they were so overconfident, they didn't do any homework! They just thought they knew what the answers were. We were able to show that the costs of controls were much less than the cost of the damages, and we won the debate. You can imagine how I felt, winning a debate from Milton Friedman and Ronald Coase! Harold Demsetz was the first speaker, and the first thing he said was, "We do not yet have enough air pollution." Demsetz was a fantastic teacher in the sense that he was just great at getting students to challenge their unexamined accepted ideas.

After that debate, three of us decided to give a course together on environmental management—David Currie, Dan Janzen, an ecologist who left for Michigan at the end of that year, and myself. We didn't know where it should go in the curriculum, but we thought maybe Urban Studies. I called up the chair of Urban Studies, and he said, "Hey, that's a great idea! I'll give you a course number." I think we gave the course three times before David Currie went to Springfield to head the state pollution control board, and George Tolley from economics took over from him. George and I gave that course for many years. We got tired of it and we stopped, but he and I have been giving a course for the past six or seven years now on energy and energy policy together. We started with about 35 students, and this past fall we had 135 students. We try to get as many students as we can from the natural sciences because the majority comes from economics. We make them work in interdisciplinary teams, and each team has to write a professional quality term paper. A surprising number of students in the course have decided to go into public policy work. It's been a lot of fun.

How did you become interested in science?

As a small kid, I was given a microscope, and I really liked that. And one Christmas I remember waking up at about four in the morning and looking at the pile of presents and finding this box. I could smooth the paper and read that it was a chemistry set. I think I must have been about 11. As a teenager, I read a lot of books about magic in a bottle and crucibles,¹ so I knew chemistry was about explosions and the structure of the atom. I loved explosions. I had a lab in the basement. At that time, anybody, including a teenage kid, could go to a chemical supply house and buy anything. So we would buy sodium. You could throw a chunk of sodium in the water in the gutter and watch it go zzzzt . . . BOOM!! I also learned about physics from my mother's high school physics book, which was just terrible. Physics was about ladders leaning against walls. So physics was boring and chemistry was interesting—I knew that from what I read! And it wasn't until I got to college that physics was also interesting. I decided then I wanted to work on the border of physics and chemistry.

If you could work on anything right now, what would you be working on?

I would certainly have students working on the problem of size-dependent properties [or] the way that electron-electron interactions determine the structure of an atom. The pictures out in the hall from my office essentially demonstrate that atoms with two valence electrons, like magnesium or barium, are much like molecular states—you can assign vibrational and rotational quantum numbers to the electrons collectively. In many ways viewing the electrons as molecule-like in these two-electron shells is a better approximation than imagining them swimming independently. This led to a question that we never got around to answering: suppose you have four electrons like the carbon atom. Is the structure of the ground state of a carbon atom really a tetrahedral shape? Linus Pauling said that to get a tetrahedron, you have to promote the atom. I've come to disbelieve that. I think the electron-electron correlation and the repulsion of the electrons is enough to make the carbon look tetrahedral in its ground state. But that's only my guess. You'd find out by looking carefully in the right way examining the structure of a well-represented carbon atom. That's one of the things I'd do.

Are you worried about what will happen to environmental protections under the current administration?

I'm terrified. If this president and his head of the EPA carry through with what they've threatened, it could lead to global disaster. It could cause environmental consequences that would lead to many phenomena that would destroy our civilization. If sea levels were to rise a foot-and-a-half, we'd lose coastal cities, and the climate extremes could become so severe that we'd have far more storms than we're having now—

Interview with R. Stephen Berry continues on p. 10

¹Milton M. Silverstein, *Magic in a Bottle* (New York: Macmillan, 1942); Bernard Jaffe, *Crucibles: The Lives and Achievements of the Great Chemists* (New York: Simon & Schuster, 1930).

Robert Gomer, chemist, longtime teacher and cherished colleague, 1924-2016

By Greg Borzo



Professor Emeritus Robert Gomer, a chemical physicist who pioneered techniques for studying molecules and taught at the University of Chicago for nearly a half-century, died December 12 of complications related to Parkinson's disease. He was 92.

In his research, Gomer studied the emission, diffusion and absorption of atoms and molecules on ultraclean surfaces. That work started, Gomer once wrote, "after hearing a seminar account of E.W. Muller's field emission microscope. I built one and became active in developing this instrument as a tool for surface studies."

Gomer was a pioneer in the modern discipline of surface physics and chemistry, said Steven Sibener, the Carl William Eisendrath Distinguished Service Professor in Chemistry and the James Franck Institute. "His lifelong work on the surface diffusion of atoms and adsorbates on metallic surfaces represent research that has withstood the test of time, and provided foundational information for the generations of researchers that followed in his areas of interest."

Gomer first came to the University in 1950 as an instructor in the Department of Chemistry and the James Franck Institute. He served as director of the James Franck Institute from 1977 to 1983 and was named the Carl W. Eisendrath Distinguished Service Professor in 1984.

"Chicago offered a sense of belonging and a sense of being a part, however modestly, of a great adventure," Gomer once wrote. Richard Gomer said his father will be fondly remembered for his collegiality.

"He viewed his colleagues and students as family," said Richard Gomer, a professor of biology at Texas A&M University. "He loved having lunch with chemists and physicists daily at the Quad Club. It was a real meeting of the minds, but one that would often end with a game of billiards."

After Gomer became a professor emeritus in 1996, he and his wife, Anne, organized regular interdisciplinary talks for faculty in their Hyde Park home. "This is the institution that became, about five years ago, the Robert and Ann Gomer lecture series, which I chair and which meets about six times a year," said David Bevington, the Phyllis Fay Horton Distinguished Service Professor Emeritus. "Bob was a true inspiration to us all and will be greatly missed."

Gomer approached science and research "as a contest with nature," said Lanny Schmidt, Gomer's former student and now a professor of chemical engineering and materials science at the University of Minnesota. "He personalized every problem in a way that made it fun to do research. The tedious tasks were still part of the overall contest."

"One time we were working on an intricate spot-welding problem," Schmidt added. "Upon completion Bob said, 'Now we've got Mother Nature right where we want her!'"

Richard Gomer recalls that his father was a tough grader who wasn't afraid to give out Ds and Fs. "His only reason for doing this was to set high standards, as he always did for himself, as well as for others," he said. "This inspired many people to pursue research. He spent a minimal amount of time in his office; he was constantly working in the lab."

Born in Vienna, Austria in 1924, Gomer went to England in 1938 as a refugee child, came to the United States in 1940 and went to Po-

Gomer was an outspoken opponent of the proliferation of nuclear weapons.

mona College. After serving in the U.S. Army, he received his PhD in chemistry at the University of Rochester in 1949.

Gomer was an outspoken opponent of the proliferation of nuclear weapons. He was a regular contributor to and chaired the editorial board of the *Bulletin of Atomic Scientists*, a journal founded by Manhattan Project physicists that covers policy issues related to the dangers of nuclear weapons.

In 1966, Gomer was one of four scientists who wrote a classified report for the Department of Defense about the potential use of nuclear weapons in the Vietnam War. "Tactical Nuclear Weapons in Southeast Asia" concluded that such strikes would be catastrophic for U.S. global interests. "It was our purpose to show that using nuclear weapons would be an immoral folly and set an awful precedent," Gomer said.

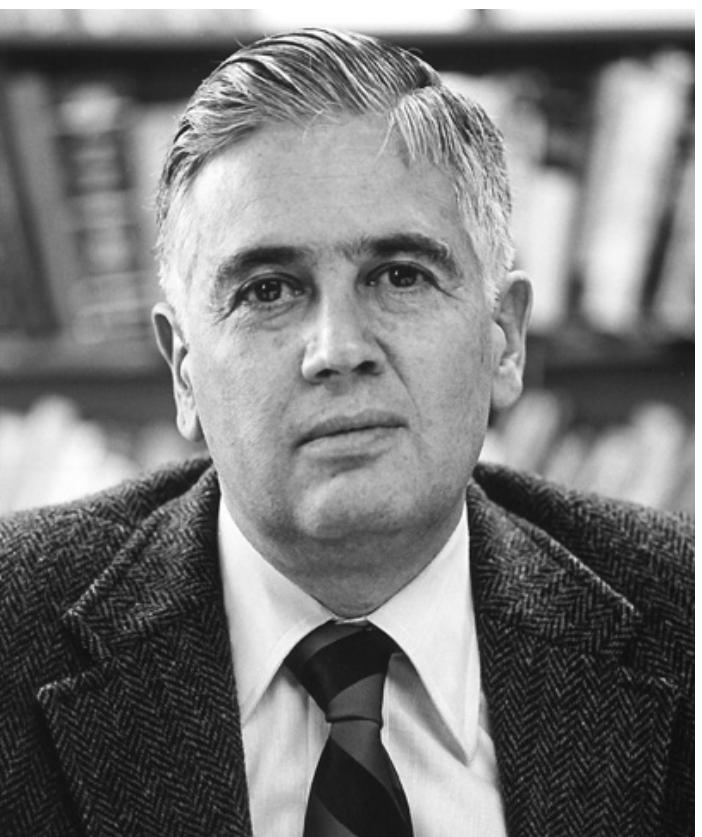
Gomer was elected a member of the National Academy of Sciences in 1981. He was an Atomic Energy Commission Postdoctoral Fellow at Harvard University and an Alfred P. Sloan Research Fellow at UChicago. He also was a Guggenheim fellow at the University of Paris, and a Fulbright fellow at the Technical University of Vienna.

He won several awards, including the Bourke Lecturer from the Faraday Society, the Kendall Award in Colloid or Surface Science from the American Chemical Society, the Senior U.S. Scientist Award from the A. von Humboldt Society, and the Davison-Germer Prize in Surface Physics from the American Physical Society.

He wrote *Field Emission and Field Ionization* (1961) and edited several scientific journals, including *Applied Physics*.

"We shared many conversations on the nature of atomic-level dynamics at interfaces," Sibener said. "This topic and associated scientific advances remain at the forefront of science today, contributing many crucial ideas to what is now popularly referred to as the field of nanoscience."

Gomer is survived by his wife, Anne, son Richard; daughter, Maria Luczkow; and grandchildren Katie, Anna and Julia.



An award-winning journalist and author, Greg Borzo worked at the American Medical Association, Field Museum and is now a News Officer at the University of Chicago. He is also a tour guide at the Chicago History Museum, and his books include The Chicago "L," Chicago Cable Cars; and Chicago's Fabulous Fountains.

This story reprinted courtesy of the UChicago News Office.



Simple Rules for Synthesis

Scott Snyder explores collections of complex natural products, envisions ‘Star Trek synthesis’

Professor Scott Snyder, formerly of Columbia University and The Scripps Research Institute, joined the Department of Chemistry at the University of Chicago in Autumn 2015. An avid educator, Snyder has co-authored several books, including the textbook used in Chicago’s undergraduate organic chemistry sequence, a handbook on natural product synthesis, and a primer for new faculty called *Teach Better, Save Time, and Have More Fun*. His gift for taut description also extends to his research aims: “We want to learn how to synthesize complex structures, come up with new tools to do so, and figure out what the biomedical or biochemical potential of those structures are.” Yet while many chemists may share these goals, Snyder’s focus on developing reagents and broadly applicable strategies for the synthesis of complex natural products makes his approach particularly effectual.

“We make complex natural products,” he explains, noting that over half of the most widely sold pharmaceuticals either comes directly from nature or is derived from nature. Some natural product-based drugs include aspirin, a derivative of willow tree leaves, the breast cancer drug taxol, which comes from the Pacific yew tree, and the antibiotic vancomycin, which comes from soil bacterium *Amycolatopsis orientalis*. “As creative as we are as chemists, we never would have come up with a structure like [vancomycin],” says Snyder. “What natural products can teach us, apart from whether or not they become medicinal agents, is how to target biomolecules.” Vancomycin, for instance, targets specific peptide chains on bacteria through a series of five hydrogen bonds, thereby interrupting cell wall synthesis in Gram-positive bacteria. “Often, when I look at molecules we’re thinking of synthesizing, I have questions like, ‘Why are the atoms in this particular order?’ ‘Is there a reason why

We want to learn how to synthesize complex structures, come up with new tools to do so, and figure out what the biomedical or biochemical potential of those structures are.

this pattern exists?’ ‘Does this allow for a selective interaction with a biomolecule?’”

While Snyder has focused on molecules with biomedical utility, his efforts are not disease-driven. “For me, it’s molecule first and foremost,” he says. “I want to make sure that whatever we are engaged in, we will have opportunities to develop new reagents and new tools to put molecules together more efficiently.” Recalling his early interest in halogenated marine natural products, Snyder explains that his approach was motivated by efforts to develop tools that could be modified for more general use. “[Could] we come up with appropriate chemical reagents to very simply put in the same atoms as what nature often does with exquisitely tailored enzymes? Although we have a lot of reagents to do chlorinations or brominations of simple alkenes, we could not do similar processes for a number of more complex, naturally-relevant substrates in a laboratory setting. We used that as inspiration to come up with tools to solve this problem. One, known as BDSB (for $\text{Et}_2\text{SBr} \bullet \text{SbBrCl}_5$) has been commercialized and sold by Sigma-Aldrich in a number of countries throughout the world.”

Through such an approach, Snyder has gained useful insights on the limitations of the field. “[While] we may have come up with twenty ways to do a particular reaction, when you’re working with a more complex molecule, maybe all twenty of those methods fail. That indicates that we may need another tool or approach to solve this particular problem. One of the advantages of working with a more complex molecule is that it may afford a clear sense of where there are limitations in existing tools. The mere fact that there are twenty tools also might indicate that there’s no such thing as a general reaction. We think we may know the rules of reactivity, but a lot of times

we actually do not. I have never had a synthetic design ultimately translated in the laboratory from start to finish based on what was originally conceived.”

Whether a consequence or an effective means of addressing this unpredictability, Snyder prefers studying collections of structures. “Typically a plant . . . doesn’t make just a [single] molecule. It will make several related structures, because [it] is still refining what is helping it have organismal survival,” he notes. “For anything that we do, I’m looking for solutions I can then apply to other targets as well. We recently published a paper where we developed a cascade reaction—i.e., a process where, after one reaction happens, it sets the stage for the next and the next—to rapidly assemble a number of critical bonds. Based on that success, we have begun to think about other molecules that have proven difficult to make where this approach may be a successful way of handling certain elements of those molecules as well.”

Recently, Snyder and his lab have focused their attention on alkaloid natural products, including some used in traditional Chinese medicine. “In some cases, we know that these molecules have come from a medicine, but little is known about what it actually does, whether it is the active component, one of several active components, or inactive,” he says. “I believe all natural products exist for a reason. Nature invests a fair amount of ATP energy to produce these structures for that organism’s own purpose. Often they can interact favorably with human biology, and for us it is a question of figuring out what a molecule might be able to do.”

Another long-term interest has been compounds found in red wine, including resveratrol, a molecule that has received popular attention for its potential role in the so-called “French paradox”—the otherwise inexplicable longevity of the French despite a diet traditionally high in fat and cholesterol. “Nature uses resveratrol as a seed to make several hundred structures as a front-line defense against a fungal infection,” he explains. “Once a fungal infection comes along, between two and ten resveratrol [building blocks] will come together to make a whole collection of new structures as a front line defense [that] can allow for organismal survival or at least slow things down so that other resources can be brought to bear to allow for certain plants to survive.”

Though particular molecules have drawn his attention, Snyder’s work broadly aims at the methodological craft of synthetic chemistry. “We do not want several uncontrolled pathways that lead to several dozen structures at once; we want to understand how to make one, and then from there make the next, and then from there make the next. If you had a molecule that had six different places that are chemically similar, could we pick them out one at a time in any order that we might want?” He calls the ideal accomplishment “Star Trek synthesis—you go to this machine, you say, ‘ice cream sundae,’ and boom—it shows up. For organic chemistry of the twenty-first century, I would argue that is one of the field’s main goals—to find the way to tailor a molecule and put in anything we might want in any order we might desire.” (ICH)



In Memoriam Mildred Dresselhaus

Dominick Reuter, courtesy MIT News Office

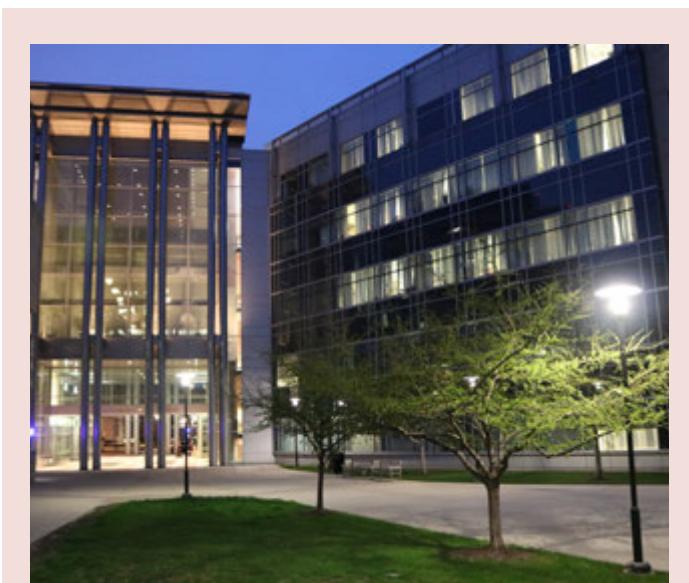
Mildred Dresselhaus (PhD 1958), first female full professor at MIT, first female president of the American Association for the Advancement of Science, former president of the American Physical Society, and recipient of numerous awards, including the National Medal of Science, Kavli Prize, Enrico Fermi Award, and Presidential Medal of Freedom, passed away on February 20 at the age of 86.

The daughter of Polish Jewish immigrants, Dresselhaus grew up in the Bronx and attended Hunter College, graduating with a BA in 1951. Initially intending to become a teacher, Dresselhaus was encouraged by Rosalyn Yalow, later a Nobel Prize recipient, to pursue physics.

After completing her MA at Radcliffe College, Dresselhaus arrived at the University of Chicago for doctoral research under Enrico Fermi. Fol-

lowing a postdoc at Cornell, she joined the Lincoln Laboratory at MIT in 1960, the Department of Electrical Engineering in 1967, and the Department of Physics in 1983.

In addition to being the first female professor to receive tenure at MIT, Dresselhaus was also the first woman to attain the prestigious rank of Institute Professor. Conscious of gender disparities in the sciences, Dresselhaus was a strong advocate of women in science, co-organizing the first Women's Forum at MIT in 1971 and encouraging and mentoring other female scientists throughout her half-century career in teaching and research. Called the "queen of carbon science" for her work on graphite, fullerenes, the electronic structure of semi-metals, and nanomaterials, Dresselhaus co-authored 8 books and some 1,700 papers. (ICH)



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to
chemistsclub@gmail.com.

We hope to see you there!



Interview with R. Stephen Berry,
continued from page 4

and we just had tornadoes yesterday! Look: no snow in January or February here in Chicago! The blindness of the present administration to this phenomenon and their absolute refusal to recognize the facts we observe—even if you're skeptical of the models—could be suicidal.

What can we do?

Take a bigger view—the whole human species, the whole of life on earth is a tiny transient in the bigger scheme of things. So why worry? If it's not an asteroid that makes us extinct, we can do it ourselves!

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the chemists club

Spring 2017

Dear friends,

Welcome to the spring edition of the *Chemists Club!* This issue features several members of our department who have made breakthrough contributions to science and served the community in many ways. You will surely enjoy reading the interview with Steve Berry, the James Franck Distinguished Service Professor Emeritus, who describes his long involvement with environmental and energy law in the course of his inquiry into thermodynamics and other aspects of physical chemistry. Steve is an inspiration—and, at the age of 86, he continues to co-teach his Energy and Energy Policy course in the University's Big Problems curriculum. We remember our dear colleague, Professor Robert Gomer, who, in addition to making groundbreaking strides in surface physics and chemistry, was a staunch and vociferous opponent of nuclear weapons. For many years, Bob and his wife, Anne, have helped to cultivate a vibrant intellectual community among emeritus faculty by hosting a bimonthly lecture series in their Hyde Park home. This series continues under the name of the Robert and Anne Gomer lecture series. We feature the research of one of our newer colleagues, Professor Scott Snyder. Scott is not only one of the premier synthetic chemists in the world, but he is also an outstanding teacher and a co-author of one of the most popular textbooks for organic chemistry. And we also remember distinguished alumna Mildred Dresselhaus, whose dazzling career studying carbon was matched by her advocacy and mentorship of other women in science.

With the many recent hires, the department has been abuzz with activity, with grants and awards pouring in, and publications and lectures going out. We also had an exceptional bounty of matriculating graduate students last Fall, nearly twice the size of our typical class. I invite you to come and see firsthand all that is happening at Chicago. We will be holding a reception on June 1 in the Gordon Center Atrium as part of Alumni Weekend. We sincerely hope you will join us then for refreshments, short talks on current research, lab tours, and mingling with current students and faculty. The success we have enjoyed as a department is in no small part due to the role played by our alumni, and we hope we will have the opportunity to express our gratitude and share our work in person.

Best regards,

Viresh Rawal
Professor and Chair