A Conversation with John B. Fenn

John B. Fenn and M. Samy El-Shall

Department of Chemistry, Virginia Commonwealth University, Richmond, Virginia 23284-2006; email: jbfenn@vcu.edu

Annu. Rev. Anal. Chem. 2009. 2:1–11

The *Annual Review of Analytical Chemistry* is online at anchem.annualreviews.org

This article's doi:
10.1146/annurev-anchem-060908-155216

1936-1327/09/0719-0001$20.00

Video

Please visit **http://arjournals.annualreviews.org/doi/abs/10.1146/annurev-anchem-060908-155216** for video highlights from this interview.

Editor's Note

Every year, the Editorial Committee of each Annual Reviews series asks an internationally distinguished researcher to prepare a prefatory article for the volume being planned. It was an easy choice for us to seek a contribution from John B. Fenn (b. 1917), who since 1994 has been a professor of analytical chemistry at Virginia Commonwealth University in Richmond, Virginia. Few people have achieved the originality and the impact that John Fenn has shown in his development of new instruments for chemical analysis. Anyone using supersonic jet expansions or electrospray ionization owes a huge debt to this special individual, who was awarded the Nobel Prize in Chemistry in 2002. A full account of the development of electrospray ionization may be found in Fenn's 2002 article in the *Journal of Biomolecular Techniques* (see Related Resources at the end of the interview).

What follows is a transcript of an interview that Fenn's colleague Professor Samy El-Shall kindly conducted on our behalf. This interview captures the spirit and the imagination of this singular individual. It also shows that the path to discovery is seldom straight and narrow.

Richard N. Zare

A CONVERSATION WITH JOHN B. FENN (EDITED AND ABRIDGED)

Samy El-Shall: My name is Samy El-Shall. I'm a professor of chemistry at Virginia Commonwealth University. John Fenn has been my colleague at VCU for the past 15 years, and today we'll talk about his journeys through vacuum and mass spectrometry and the molecular beam. John, thank you very much for doing this interview.

John Fenn: My honor, but it remains to be seen whether it's my pleasure.

SE-S: I know you went to Berea College in Kentucky. I also know you were born in New York City. So how did you make the transition from New York City to Berea College, and from Berea College to Yale University?

JF: Well, my father and mother both graduated from college the same year—my dad from Rutgers and my mother from Columbia. She majored in home economics, and Dad majored in electrical engineering. This was right before the end of World War I. They were both hired by the Presbyterian Mission Board to teach at a school that the Mission Board ran in Sitka, Alaska. Dad was hired to teach mechanics and to also be the handyman to keep their generator things going, and Mother taught home economics. They met up there, and they fell in love and got married.

Mother lost her first baby, and the doctor said they'd never take her through another pregnancy because her pelvic structure was such that if she were going to have children, it would have to be by Caesarian section. She was determined to have children; she was one of ten children herself. Since they weren't doing Caesarian sections in Alaska then, she insisted that they move back to the States. They did, and we lived in Hackensack, New Jersey.

My father got a position as a superintendent of a waterproofing factory in Lodi, New Jersey, which was right near Hackensack. The man who was a big wheel in the Presbyterian Mission Board came from Englewood, New Jersey; he was a very wealthy man. He had a small conglomerate known as the Fred S. Bennett Corporation, and one of the small companies in this conglomerate was the Metacloth Company, in Lodi, New Jersey. The company made waterproofing duck by passing cotton duck through cuprammonium sulfate solution. Ammonium hydroxide makes this vivid blue cuprammonium, and that hydroxide is one of the solvents for cellulose. In fact, one of the early rayon processes was based on the solution of cellulose and cuprammonium solution, which upon being neutralized with acid would then reprecipitate the copper. That was known as the Bemberg rayon process. The Metacloth—that was its trade name—had this characteristic blue-green color, and it was very popular in the Tropics. Because of the copper, the white ants left it alone, and you could make tents and tarpaulins in the Tropics and the white ants wouldn't bother. In 1927 the Depression came, and the Metacloth company was sold to new owners. All of a sudden Dad was without a job.

My mother's sister was then teaching down in an institution in Kentucky known as Berea College. This Berea comprised four schools. There was a foundation junior high school, which went through ninth grade; there was a two-year teacher training school known as the normal school; there was the three-year tenth, eleventh, and twelfth grades, and the college. They charged no tuition; the place was run my student labor. Every student had a job. I went there in the eighth grade. I was in training school, which was associated with the normal school, to give the normal school students some teaching experience. I went through there—ninth, tenth, eleventh, and twelfth grades. When I finished the twelfth grade, I was only 15.

My mother and father thought it was too soon for me to go to college. So I stayed on an extra year at Berea and took piano lessons and a course in mechanical drawing. I applied for admission at several universities. I got admitted at Yale and at Northwestern University. Northwestern had

John Fenn

a better chemistry department. (I'd had wonderful chemistry teachers at Berea, so I majored in chemistry.) Furthermore, its fellowship was more generous. So, I decided to go to Northwestern.

But one of my next-door neighbors at that time was a German professor by the name of Charlie Paut. He had taught me to play chess. Charlie asked me what I was going to do next year. I said I was going to college. I said that I'd had offers of scholarships at Northwestern and Yale. He immediately said "Well, you will enjoy New Haven."

I said, "Well, I'm not going to New Haven. They're not paying as much money, and the chemistry department is not supposed to be as good as Northwestern's." But Charlie persuaded me otherwise. And it happened that the treasurer of Berea had a son who had been admitted to Yale as a freshman, and he was going to drive his son up from Berea and install him as a freshman at Yale. He offered me a free ride from the middle of Kentucky up to New Haven. So I took advantage of that and rode on up to New Haven. And that's how I went to Yale instead of Northwestern.

The chap who was head of the physical chemistry group was Herbie Harned, who had been at Brown and come to Yale. He also brought [Lars] Onsager. Along with Onsager came Gus Akerlof, who was an experimental physical chemist. Gus was my major adviser when I got to Yale. It makes you wonder if there is a Nobel virus. I was in the physical chemistry group at Yale, and while I was there, Lars Onsager got a Nobel Prize. Another guy by the name of Ray Davis got the Nobel Prize in physics for his work on showers of neutrinos from the Sun. Within a five-year period, there were four Nobel prizes that came out of Yale's chemistry department.

SE-S: Did you ever take classes with Onsager?

JF: Oh, yes. They were known as Norwegian 1 and Norwegian 2—beginning statistical mechanics and advanced statistical mechanics. Lars was hopelessly brilliant. All we'd have to say is "I don't see, Dr. Onsager, how you got from this step to that step." And he'd pick up a piece of chalk—I've seen him go right around the room deriving *anything* right from scratch. There was not a mean bone in his body. But people would not come and give seminars at Yale because they'd start out and Onsager would say, "I don't quite understand this" and would get up and take the chalk away from the speaker and derive it right there on the board. He was so brilliant that it was embarrassing.

SE-S: Your first job was in the chemical industry, right?

JF: Monsanto Chemical Company in Anniston, Alabama. It was their phosphate division. The west end of Tennessee has got all this phosphorus rock. They shipped elemental phosphorus by tank car from the TVA [Tennessee Valley Authority] down to Anniston, Alabama, and there they'd burn the phosphorus to P_2O_5. I grew to understand what a versatile compound phosphorus is because of the number of different kinds of salts you can make with it—baking salts, baking powders, and all those things.

While I was there, a young chap who had finished his degree at Princeton came down, and he and I got to know each other. We were both working in the same group. His name was Jim Mullen [James W. Mullen II]. He always said that someday he was going to start a company, and when he did he wanted me to join him. We both left [Monsanto] at the same time. He got a job at Bell Labs, and I got a job at a little company up in Wyandotte, Michigan, known as Sharples Chemicals. Sharples built its whole business on making amyl chloride. From the amyl chloride they'd make all kind of amines, alcohols, amyl alcohols, and whatnot. So they had a whole [lot of] five-carbon molecules. I spent almost two years on a pilot plant up there.

Jim kept in correspondence with me, and one day I got a letter from him. He said, "John, I've started my company. I want you to come down to Richmond." So I got on the train (this was right in the middle of the war), and rode all night down to Richmond, Virginia. He'd started this little company called Experiment Incorporated. At Bell Labs he'd been attached to a group that had charge of the telemetering section of a big project called Bumblebee. It was a Navy project whose purpose was to develop ramjet-powered antiaircraft missiles. U.S. intelligence had found that the Germans were working on the ramjet-powered antiaircraft missiles. This was just at the end of World War II.

SE-S: Can you tell me more about Project Bumblebee?

JF: It was going to be a ramjet-powered antiaircraft missile where you depend upon flight velocity in order to get high enough pressure so that when you're flying at Mach number two point three (I think it was)—you'd squirt fuel in and burn the fuel. Then you had gas that was hot enough at high enough pressure to blow out the back end so that you'd have a jet-propelled missile with no moving parts.

They called it Bumblebee because there's an old belief that according to all the laws of modern aerodynamics it's impossible for a bumblebee to fly. But the bumblebee doesn't know any aerodynamics, so it flies anyway.

Jim had been on a crew using these test vehicles. They would boost them up with rockets, and then when they got up to speed, the rockets would drop off. Every time they would start these things up and turn on the fire, the flame would go out. Jim watched this for a while, and he remembered in freshman chemistry using kerosene as fuel and that carbon bisulfide burned much more easily than hydrogen. He said, "Why don't you try carbon bisulfide?"

SE-S: So they tried it.

JF: They did. They had a prune juice bottle that they used to pour the carbon bisulfide into this fuel tank. The first test when they used carbon bisulfide as fuel, the fire burned, and they got net thrust. On the strength of that, he wangled a contract from the Navy to work on stabilizing flames in high-speed flows, and started this new company down in Richmond. Jim was a great believer in publishing results, so we published a number of papers on stabilizing flames in high-speed flows.

At about that time Princeton had been given land, a big tract of land that was owned by the Rockefeller Medical Foundation in New York. They had their plant and animal pathology laboratories in Princeton. They decided to discontinue that, and so they turned over this 200-acre tract of land right outside Princeton to the university. The Navy by that time had decided that jet propulsion was here to stay. What they needed was a source of supply of students that had some training and background in high-velocity flows, combustion, fluid flow, and heat transfer.

They set up something called Project SQUID, a Navy-authorized and -financed program to support basic and applied research in those fields of science related to jet propulsion. The first [director] that they had was Mark Mills. Mark was from California; he didn't like New Jersey. So after a couple of years, he left. We had to find somebody to run Project SQUID. Somebody put my name in the pot, and they offered me the job. The first thing the Navy did after I got there was put me in London—the Office of Naval Research branch office in London. They would send people over there for a year because this was right after the war, and the Navy felt it had an opportunity and obligation to help recover the European scientific and research community.

SE-S: You stayed in London for a year?

JF: As a matter of fact, we stayed two years. My three kids will tell you to this day that those two years in London were the high point of the Fenn family fortunes. Those were wonderful years. Then I went back to Princeton.

SE-S: How did you make the transition [back] to Yale?

JF: Princeton had an aeronautical engineering department. Art Ross, a very good friend of mine when I was a graduate student at Yale, had worked at GE [General Electric] for a while and then had gone back to Yale. Yale had decided to expand its engineering department. Art was advising the provost at Yale. He had seen the success of Project SQUID [at Princeton] and decided [Yale] wanted to do something like that. So they offered me a job. I was extremely fond of Art. On the other hand, Princeton was clearly a better place for the kind of things that I had done as a graduate student. So I decided not to go.

[But] Art was terribly disappointed. He went to see Charlie Taylor, the provost, who (as I found out later) had been counting on me. Art called me and told me how upset they were; he said, "You broke my heart" [laughter], so I agreed to go to Yale [after all].

I was in combustion by that time—combustion, flame theory, and stabilizing flames in high-speed flows.

SE-S: Was [Joseph O.] Hirschfelder working there?

JF: Von Kàrmàn and Hirschfelder were two different schools of thought on flame propagation. Joe Hirschfelder was a physical chemist, and so it was H-atom migration that was determining everything. And Von Kàrmàn was much more practical; he was an engineer. I can remember going to combustion symposium meetings, with the Von Kàrmàn group on one side. The big argument [was]: What was the propagation mechanism for flames? Hydrogen diffusion or thermal conductivity? Boy, there used to be knockdown, drag-out blows between the Hirschfelder group and the Von Kàrmàn group.

I'll never forget the AGARD [Allied Group for Aeronautical Research and Development] symposium in London. Von Kàrmàn got up and gave his latest spiel on flame propagation. A key component of it was the diffusion velocity of hydrogen atoms. In the question period, somebody asked Von Kàrmàn what were they using as the diffusion coefficient for hydrogen atoms. He didn't know. So he asked one of his colleagues. He said, "Where did we get the hydrogen atom diffusivity factor?" This guy said, "Well, we got them from Hirschfelder."

Joe Hirschfelder jumped to his feet and said, "You see! You see! They even copy our mistakes!" Because the number they had copied from the literature was wrong! But he was a great proponent of hydrogen atom diffusion [as the] mechanism, which governed the flame velocity.

SE-S: By that time had you started working more in molecular beam experiments?

JF: Well, by that time I had decided that the way to study flame reactions was the way that chemists study reactions. I had been fascinated by molecular beam experiments. You've heard the wonderful story about the Stern–Gerlach experiment? They decided to do molecular beam experiments, and so they used silver. They would pass it through a magnetic field, use a slit source, and then you were supposed to end up with two beams. They'd run this beam for a while and deposit the silver, then they could take the silver out and do a densitometric trace. The story goes that [Walther] Gerlach, who was [Otto] Stern's assistant when they did this experiment, ran it [and] took out the glass plate but there was no deposit. Gerlach came to Stern and said, "I'm sorry, Herr Professor Doctor Sir," and clicked his heels, I'm sure, "the experiment was a failure." Stern picked up this glass plate. Sure enough, there's nothing on it. But while he's looking at it, these two lines appear. It turns out Stern was a heavy smoker. He loved to smoke; he could only afford cheap cigarettes, and they had lots of sulfur in them. This silver pattern had been deposited but wasn't visible, but when Stern looked at the glass plate the cigarette smoke deposited sulfur on there, and so these two lines of silver sulfide appeared. Stern used to love to say after that, he got the Nobel Prize for that experiment because it proved the Bohr atom theory and because he smoked cheap cigars.

SE-S: Did your work at Yale basically involve free jet expansion?

JF: I learned a little about gas expansion because of the way the ramjet works. I was fascinated by Stern's work on using beams to study molecule collisions. It struck me that this was really the way to study chemical reactions: to take two beams, cross one reactant with another reactant, and bring about single collisions and then collect the products. There was enough known about the activation energies in the combustion reactions to know that there had to be a lot more energy in the collision than you would get, crossing two beams, from thermal sources.

So the question was: How are you going to get to higher velocities? There were all kinds of theories. I remember one big experiment run by Leonard Wharton, who cooked up this fancy idea that you would have these two electrodes with an extremely high potential difference between them so there would be a very intense field. An atom would be sucked into the core, and then you'd shut off the field, and it would coast on into the next one. He had this sequence of a thousand crossed electrodes. The idea was they were going to have this perfectly timed, and so it would accelerate, and they would be able to get up to 1 eV of kinetic energy of these atoms.

Well, we had already been doing some free jet expansions, and we knew from our molecular beam work that we could get hydrogen molecules up there by this seeding process. When you do a few calculations, you suddenly realize you can get heavy atoms up into several eV of energy with no problem at all and plenty of intensity. I put in a proposal to the Navy—Project SQUID, to get some vacuum pumps so that we could use these free jet expansions to produce beams that have kinetic energies of several eV.

Immanuel Estermann, who was assistant director of the Office of Naval Research at the time, came up to visit our lab to discuss this proposal. I was tremendously impressed, because he was a famous guy, and here he was coming up here. He spent the day, and he said, "John, I think this is important, but I think it's much too difficult to do with graduate students. I think it's important enough it that I'm going to sponsor it." So he gave us the money with which we bought these huge vacuum pumps.

Michel Boudart was at Princeton at the time; he was in the chemical engineering department. He had a young [student] by the name of Jacques Deckers, who he brought over from Belgium He had been doing beam work in Belgium. He knew about atomic beams. To make a long story short, I hired Jacques. I had this idea of using these free jets. I had no experience in pumping and vacuum systems, but Jacques had a lot. He's the one that proposed that we get these big, 32-inch diffusion pumps.

We started out making supersonic free jet expansion and taking the core out to make beams, and we were getting beams at any energy we wanted to. We were doing scattering experiments off

Samy El-Shall

surfaces that nobody had ever dreamed about doing before, and people began coming to see us. They would come and take one look at these big 32-inch diffusion pumps and say, "Well, that's very nice, but not for us." So we scared off the competition. We had the whole field to ourselves for several years.

We learned how to do velocity analysis of the molecules in the beam after this free jet expansion, we found out how the velocity depended upon the source pressure, and then we found out that we could put heavy molecules and seed it. And so we got all kinds of information on the coupling of accelerating heavy molecules and light molecules.

SE-S: But at the time you had no mass spectrometer?

JF: We didn't have a mass spectrometer then, no. Then we began to use mass spectrometer detectors. We wanted to get bigger molecules. One way of getting bigger molecules is to take this solution and disperse it and let the solvent evaporate. The first experiment we did was the sticking coefficient for silver atoms on a glass surface. We could make a beam of silver atoms and vary the energy all over the place and see what the sticking probability was.

SE-S: And at that time, Malcolm Dole at Northwestern was working on electrospray?

JF: Yes. I read Malcolm Dole's papers, and that's what got me very interested. He gave a paper at one of these meetings that I went to, and I was fascinated by it. Northwestern used to sponsor every year a molecular beam meeting. It was at one of those meetings where I met Malcolm Dole. We kept up quite a correspondence, and I got to know him very well.

He was able to make these big molecules by seeding them, and we could accelerate them and—

SE-S: I think he was working with polystyrene?

JF: Polystyrene; yes, that's right.

And we learned that you could look at the velocity distribution of these things and you could learn things about the rotational, vibrational transitions and the probability of them. We learned lots of things about the vibrational relaxation rates in gases, how fast vibration could turn into translation, and people got very interested. They'd come by our laboratory, take one look at these huge pumps, and say "Thanks very much, but not for us."

SE-S: [Let's talk] about your work in electrospray in the mid-70s, when you started getting biomolecules in the gas phase. I guess everybody was amazed at these results.

JF: People [said] "It's impossible for this man to get these results from this equipment."

SE-S: What was the reaction like from NIH?

JF: They supported it. They gave us the money.

But everybody said it was hopeless, that we'd never figure out what was going on.

SE-S: Jim Anderson was working with you?

JF: Yes. He does theory pretty much now, but he was the best experimentalist I ever had. First he was my graduate student, then I kept him on as a postdoc, then I got him on the faculty. Jim was really the most brilliant guy I ever had.

SE-S: You had very good people, such as Mike Labowsky?

JF: Mike Labowsky, [Masamichi] Yamashita. He was the one that did the first electrospray work, when we got big molecules, ions. We started getting these big biomolecules with a whole bunch of peaks. I remember walking into the lab one day with one of these spectra, and I said, "Each one of these peaks is the same molecule, only the difference is the number of charges. Each one of them in principle is a measure, an independent measure of the mass of the parent molecule. There ought to be some way that we can average those values."

I came back two days later. Jim Anderson had worked out the algorithm, taking advantage of the fact that the difference between the ions of one peak and the adjacent peak was simply one charge. He worked out the algorithm by which we could get an average molecular weight value for each of these peaks. Your accuracy goes way up.

SE-S: Very nice! And you continued at Yale through the 80s and then you had to retire?

JF: Yes, Yale had a mandatory retirement age, 70. And that made me angry because we were right in the middle of things, and so I fought tooth and nail, but the bureaucracy was pretty adamant. I got tired of fighting.

Vicki Wysocki [had] invited me down [to VCU] for a seminar. Later, at an AAMS [American Association for Mass Spectrometry] meeting, I said to Vicki, "Do you suppose your university would give me a laboratory? Because I've got things I'd like to do!" And so she went home and then she came back and said "Yeah—"

SE-S: Actually, I remember that very well. We had a faculty meeting—Larry Winters was chair at the time. It took just five minutes to agree that [you] would come here and have a lab. It was done in five minutes. That was '94. So it's been now about 15 years since you came to VCU. You started at Richmond [with Experiment Incorporated], and you came back to Richmond [to Virginia Commonwealth University]. So what are your reflections, now, on the whole journey through electrospray and, before that, the molecular beam? And where do you see the next steps in the development of electrospray?

JF: One thing leads to another. My latest thing, involving some experiments I did quite some time ago, but I want to go back and look at them, is bringing about what I call a condensation laser. How does a laser work? You remove the ground state, right? You get a population inversion. We were electrospraying off a thing into gas that was expanding into a vacuum system, and we found out that we got much bigger signal. It was CO_2 we were using. But it worked with all gases. It was the cooling that did it.

And what I realized was: It was the condensation. When you do these free jet expansions, you condense out the ground state, and so that means that there's nothing to soak up the electrons, and so they're emitted. They won't soak up photons anymore.

The radiation goes up! And more recently, Joe Bango [Joseph J. Bango, Jr.] has taken these—what are they called, holey fibers?—which are multiporous tubes. Which are made out of silica. And you heat them up and draw them out so you end up with a whole bundle of very small orifices. And then if you put gas in and expand them—

SE-S: Ah, so you use these as multiple expansions?

JF: Yes. We did our first experiments with single tubes. Joe Bango hasn't done anything beyond this, but what they showed is, when you have all these arrays of very small orifices, they all make condensation. And so I know just as sure as I'm sitting here that we can build lasers, which have tremendous output because you can get thousands of these small jets, each one of them in a small cross-sectional area, putting out photons to beat the band.

SE-S: CO_2 will be the gas?

JF: CO_2 or any gas; you want one that condenses. CO_2's convenient.

SE-S: So when you got the call about the Nobel Prize, can you tell us where you were?

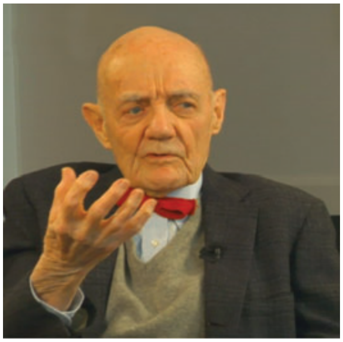
JF: I was asleep! They called at six-thirty in the morning, something like that. They said, "This is so-and-so from Stockholm calling. And we wanted you to know we have just released to the press the names of the people that are going to share the Nobel Prize this year, and you are sharing the Nobel Prize in Chemistry. And so you can expect your telephone to start ringing in about half an hour."

SE-S: And of course you went to Stockholm after that, and you got the award, and—

JF: I went to Stockholm and had a heart attack. At the banquet they always ask the oldest [prize recipient] to give a two-minute talk after dinner. You had to climb up this huge stairway, way up at the top here looking down on this great crowd. I got kind of winded going up those steps, I noticed.

SE-S: But you gave the two-minute talk.

JF: Yeah. But they were taking great care of me. They had me in the doctor's office the next day. They examined me and put me in the hospital. They handled you with velvet gloves over there. You had a personal limousine, and this keeper that takes you everywhere. She was the one,



when I mentioned that I felt a little dizzy, next thing I knew I was in the hospital. [Eventually] they decided I could go back on the plane; it'd be better to get me home before having any surgery.

SE-S: And then you became much younger after that? [Laughs]

JF: I became older. But no wiser, yet.

SE-S: Well, two years ago we had your 90th birthday here. I remember Dudley Herschbach's song [for the occasion]—a song about science.

JF: Dudley—he's one of my favorite people. Dudley's the guy—you know the little book I wrote on thermodynamics [*Engines, Energy, and Entropy*]? It's been translated into Chinese. And the Chinese [universities] have all raved about it. And they're right, in the sense of—compared to what else is available.

SE-S: How did you publish this book? You said Dudley Herschbach had helped—.

JF: Yale had what were known as residential college seminars. A bunch of students can get together, and if they find somebody in the faculty who would tell them about butterfly migration [or any topic], they can have a residential seminar. Well, I was in Calhoun College, one of Yale's residential colleges, and a bunch of students came to my lab one afternoon, because I was then in mechanical engineering, and said they would like a residential college seminar on repairing automobiles. They didn't know that my dad had taught auto mechanics down in Berea and that I knew that if we were going to do anything meaningful we had to have a shop and tools and engine blocks, and all that stuff.

I told them, "Look, that isn't going to fly, as such. But I'd be glad to try to teach you about the principles of heat engines and what is it that determines the number of miles per gallon you can hope to get, what does high-octane mean, and all that." So we had a residential college seminar, which went pretty well. You could only repeat them once, and then they had to become a regular course. I wasn't sure I wanted it to go into a regular course, but it so happened that a guy up in the geology department had decided there would be a market for paperback books on [such] subjects. I put *Engines, Energy, and Entropy* together and had it printed up.

(*Left to right*) Dudley Herschbach, John Fenn, and Richard Zare

SE-S: How did Dudley get involved?

JF: Dudley writes poems. Dudley had sent me a copy of his latest poem on a particular flower. So I said, "You sent me a one-page poem; I'm sending you a 50-page manuscript."

Well, it happened that a guy from a west coast printer had come [just then] to see Dudley about the possibility of his doing a book for them. [Dudley] had just gotten this thing [from me], and it apparently struck his fancy. So he gave them this manuscript that I'd put together on engines, energy, and entropy.

And I go back and look at it, and I think, "You know, this is a pretty good book. It ought to be the textbook for the first course in thermodynamics. Because none of these other books even tell you what a property is!"

SE-S: So, John, how do you keep yourself working every day? I see you here [at the university] every day, right?

JF: Well, I come down every day, but whether I accomplish anything or not is something else.

I've got correspondence to keep up with and I still have—I'm thinking about a proposal now—I'm getting more and more keen on this condensation laser business. But, you know, writing up a proposal and going through all the bookkeeping requirements and whatnot, I'm not sure I have the stomach for it anymore. . . .

But the thing is—the thing that bothers me is—courses ought to be fun. A course should get a student interested. I don't care whether we cover everything in the periodic table or not. I would never major in chemistry if I were starting today. They think they've got to cover everything. That's a lot of baloney.

There's no fun anymore! Look at the chemistry books. They get thicker and thicker and thicker. I wish we could somehow get it across that the purpose of education is to develop young people's minds, not fill them up with a lot of facts. Teach them how to think!

The fundamental fallacy is that [because] we know more chemistry than we did 50 years ago, the students have to learn more today than they did 50 years ago. That overlooks the fact that the student's brain today is the same as brains were 50 years ago and the same as they'll be 50 years from now! Just because more is known doesn't mean that the student has to learn more. The student ought to learn how to use his brain. Education is not training.

SE-S: I agree with you.

Thank you very much, John.

JF: My pleasure. I'm always glad to preach when I can get somebody to listen.

DISCLOSURE STATEMENT

The interviewer and interviewee are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

RELATED RESOURCES

Fenn JB. 1982. *Engines, Energy, and Entropy: A Thermodynamics Primer*. New York: W.H. Freeman. 293 pp.

Fenn JB. 1996. Research in retrospect: some biograffiti of a journeyman chemist. *Annu. Rev. Phys. Chem.* 47:1–41

Fenn JB. 2002. Electrospray ionization mass spectrometry: how it all began. *J. Biomol. Tech.* 13:101–18

Annual Review of
Analytical Chemistry

Volume 2, 2009

Contents

Errata

An online log of corrections to *Annual Review of Analytical Chemistry* articles may be found at http://anchem.annualreviews.org/errata.shtml