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INTERVIEW

Interleaved excerpts from interviews of Dudley Herschbach (DH) by John Rigden (JR) on May 21–22, 2003 and Bretislav Friedrich (BF) on March 5–9, 2012*

Up to High School

BF: You have been fond of invoking the postulate ‘ontogeny recapitulates phylogeny’. Could you talk a little bit about your early ontogeny – and perhaps also mention when and how you encountered the evolutionary postulate for the first time?

DH: I’m quite sure I heard it as a freshman in high school in a biology course. It was a new idea to me then. That the development from the fetus on went through a phylogeny struck me very much. Once you encounter an idea like that, it becomes part of your reference framework and you recognize other things that seem to correspond to it. As a freshman, I also met Shakespeare’s seven ages of man, a progression akin to phylogeny. (I know you read Shakespeare and Milton and listened to *Car Talk*; that was how you mastered English!) I’ve several times quoted ‘ontogeny recapitulates phylogeny’, particularly in my first major review of molecular beam work on reaction dynamics in 1966. It seemed an apt way to view the development of such a new field. Starting out, of necessity we had to expect in essence to repeat stages traversed decades before by Otto Stern. Of course, we went off in a different direction, but naturally had common roots. Thinking in terms of a historical progression, and that you’re part of passing-on from generation to generation, is something that I was very aware of from an early age.

Students often make comments revealing that they think, ‘We’re envious of you and your generation. You had it so easy, the fruit was hanging on the trees, you just walked by and it fell on your lap.’ That’s because the way they encounter science in courses and textbooks makes it look like progress marched on as Olympian figures made one discovery after another. Science doesn’t look like that to them in their experience of it, and they don’t realize that it didn’t look that to people of my vintage either when we were students. I remember Bright Wilson telling me how it was when he was a graduate student with Linus Pauling. He and the other members of Pauling’s group had no idea that the work they were doing was totally reshaping the way chemists thought, by focusing on molecular structure and electronic structure. They didn’t have the historical perspective to appreciate it. Of course that’s true for each generation.

I like to point out to students two things they should recognize. First, the lucky pioneers, striding through the orchards with fruit landing in their laps, also often stepped into potholes or quicksand because there was no clear path. So they had the privilege of making what later seemed bonehead mistakes. Today’s students don’t have to fall in the same ditches. Second, today’s students inherit a legacy of tools – conceptual, theoretical, as well as instrumental tools – that changes everything. As if being given new eyes and hands. Students now can see and do things their predecessors couldn’t even imagine.

Sometimes I elaborate on that point, usually citing as a favorite example magnetic resonance imaging. I’d asked Ed Purcell about it, and he said he’d never imagined NMR could be used for imaging. Why not? Well, I think it likely was completely out of the question for the pioneers who pursued NMR. They wanted to get a magnetic field that was very uniform over the whole sample, in order that the spin-flips occur at a sharp, well-defined frequency. For imaging, however, you want the field to be extremely nonuniform, in a sense the worse possible field, because you want to get a different frequency at every geographical point in the sample in order to create an image. From the original perspective of NMR, that’s ridiculous. Moreover, a huge number of frequencies need to be recorded and processed to obtain an image. I think that requirement, probably unconsciously, kept people from thinking of imaging, until computers became

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powerful enough. As brilliant as Purcell was, he didn't have the new mind-set that the computer had fostered.

BF: I was wondering if you could give us a tour of the reading that influenced you the most, intellectually and emotionally, during your formative years, say until you were, say, 20?

DH: Well, I did a heck of a lot of reading. Going to college, starting at 18, actually interfered with my reading because of the academic assignments. However, as I've said many times, the History of Western Civilization course that all freshmen were required to take was the most important course I ever had. It required more reading than any other course, and enabled me to understand all kinds of institutions, traditions, and cultural things that had been completely mysterious to me before. Actually, both before and since, history and especially biography have been a major part of my reading. Probably also the most influential too. But I enjoy reading of all sorts. I cherish the memory of learning to read, stretched out on the kitchen floor, trying to figure out what was in these little balloons in the comic strips.

BF: At what age?

DH: According to my mother, I was four. She had bought a set of encyclopedias, designed for kids. There were a dozen or so volumes, each 300 pages or so. I remember looking things up in those books; they were certainly important in my early education. Before I went to school, I had read entirely the first two volumes, which had a lot about dinosaurs, geology, and ancient civilizations. I didn't go to school until age six. In those days, it wasn't customary for kids to go to kindergarten, at least for people in our area. I missed a lot of first grade and part of the second too, because I had terrible earaches. That didn't matter much, since I could read very well way before I went to school. On my own, I just kept reading, reading, reading. Once you can do something well, you like to keep doing it.

During most of my grammar and high school years, I regularly visited a charming little town library. The librarian, Mrs. Vogel (an apt name for her because she was a birdlike lady), took a liking to me and she would set aside books she thought I would like to read. Somewhere I've described how she once saved for me an anthology of Russian literature. It was about two inches thick, printed on thin onion paper, so had about 2000 pages or so. Ordinarily, books were due back in two weeks. I was so naïve, I thought that the ordinary rule applied, so read the whole anthology in two weeks. From that I came away with a powerful impression that Russian literature is unbearably gloomy, so have avoided reading much more of it!

BF: Any reading favorites?

DH: I remember there was a series of adventure books called *Indian Brother* that I read and reread, probably when I was 10 or 12. The first volume started out on Mount Katahdin, in Maine, which I never got to till decades later. The tale was strongly moralistic, and that appealed to me. I can't remember the details now, but the hero made friends with an Indian boy and did many good deeds. I was a very earnest kid. For example I read the Bible, because the Bible was supposed to be very important and I wanted to find out why. I read it not once but twice, the whole Bible.

BF: Both Testaments?

DH: Yes. A lot of it of course struck me as very weird stuff, very. To start with, the Book of Genesis really puzzled me. It made so obvious that God was rather nasty. He forbids Adam and Eve to eat of the Tree of Knowledge. Yet he'd just created them. Even I as a kid could see that was unfair. Kids are naturally eager to do something if their parents say they shouldn't; that's sure to make the kids all the more interested in doing it. Surely God knew that? Then the punishment was so unjust: Eve was sentenced to the pain of childbirth. That's terribly ugly and nasty. The God of the Old Testament is not attractive at all.

Our house burned down when I was nine and the encyclopedia set I mentioned was lost. After that, we didn't have much in the way of books at home. Mostly they were either *Reader's Digest* books, which my mother liked, and Ellery Queen mysteries, which my dad liked. But my mother always gave me a book for my birthday or Christmas, so I had a dozen or so, classics such as *Robinson Crusoe, Treasure Island, King Arthur, Twenty Thousand Leagues Under the Sea*. So for several years most of my reading was in books from the town library, probably averaging two or so a week. As mentioned already, for me the Western Civ course I had as a freshman in college was a revelation. Harold Johnston, my freshman advisor, told me years later that when he first saw me he thought, 'Now there's a real hick.' He had grown up in a small town in Georgia, so he could right away recognize a country bumpkin.

BF: Ontogeny recapitulates phylogeny.

DH: It's ironic that I wound up at Harvard. In the minds of people from the part of society that I grew up in, my roots, Harvard epitomizes snooty, aristocratic, people. Of course, that image is completely untrue of Harvard today, but it was promulgated with good reason back in the 20s and 30s, when my parents and their friends formed their impression of Harvard.

BF: In your boyhood, you worked on the family farm, as you've mentioned on various occasions. I was wondering about several aspects of it. One was whether you had a certain evolution of ideas about what you wanted to do, but also I'd find it interesting to hear how you actually worked with your siblings and your parents at the farm. Was it, for instance, teamwork? Or how organized was it?

DH: Our family only lived on the farm about five or six years and it was not a source of income. That period was between the time our house burned down and when we moved into the nearby town of Campbell. I was between 9 and 14 or 15 then, the eldest [born June 18, 1932] of six kids, three boys and three girls. Those who were old enough helped with the farm chores, particularly my two brothers. We had just one cow, a few pigs, quite a few chickens and rabbits, a sizable vegetable garden, a substantial field of potatoes, maybe two acres, and a nice barn. As the eldest, my chief chores were milking the cow, tending the pigs and chickens. My sister, Dorothy Dell, two years younger, helped our mother in the house a great deal. Our mother had serious heart trouble, resulting from damage inflicted by an epidemic of rheumatic fever when she was a youngster.

My dad, like his father, built houses. He originally wanted to be an architect, and for two years, just after high school, he studied at a school learning design and drafting. He started a construction company but only a few years later, in the Depression, it went bankrupt. Back then, builders had to offer people second mortgages when selling a house. Since a second mortgage takes backseat to the bank's first mortgage, when a lot of people went down in the Depression, a lot of construction companies did too. My arrival about then came at an inopportune time – a not unfamiliar phenomenon. After the bankruptcy, he continued building, but only one house at a time. Usually he had only one hired hand to help more or less full-time, and once we got old enough, about 10 or so, one or more of his sons would work with him part-time.

BF: Was it common for parents to pay their children if they helped with running the household or the family company?

DH: We didn't get an allowance for doing regular chores, but sometimes were paid a dollar for an afternoon's work on a building job. In an episode I well remember, I unintentionally broke the windshield of my dad's pickup by throwing a rock. In order to get money to replace the windshield, I was paid for three or four full days of work. It involved a very big pile of boards that had been taken off old barns, knocking out all the nails and trimming up the boards. Although I was paid, it was really to make amends for my crime of throwing the rock.

But at age ten I did become relatively wealthy, in my eyes even a plutocrat. That was 1942, and the pay for picking a 50 lb box of prunes jumped to 25¢ from 9¢ the previous year, because the war had greatly boosted the price of prunes, which went into soldier's rations. I was excused from helping my dad with his house-building to be able to work in nearby orchards during most of the summer. At my birth an aunt, as I was told many times, predicted, because I had big hands – talk about ontogeny recapitulating phylogeny – that I would make my living with my hands. Her prediction was remembered that summer! Most people don't know about prunes or how they're harvested. They say, 'Oh, you mean plums?' No, prunes are different. You shake the tree and then crawl around the tree to pick them off the ground. The tree has a sunny side and a shady side. My technique was to pick as fast as I could under the sunny side, so I could pick even faster on the shady side. I was very agile and very ambitious. Typically, I'd pick 40 boxes of prunes, 50 lb boxes, a day. That's a ton of prunes a day!

So, for three or four weeks, I earned $10 a day, as a ten-year-old kid. Most of the grown men among our neighbors at that time weren't making more than $10 a day. From then on I bought all my own school clothes. Also that first summer bought a nice used bike for $26. I was concerned about my wealth. It probably was more than $200, a huge amount of money for a youngster to have back then. (Ten years later, at Stanford the yearly tuition was only $600.) I'd heard a lot about the Depression. It seemed just as mysterious and scary as God, but to me the Depression appeared much more important because people spoke about it so much. So I asked my parents what to do in case another Depression came: I didn't want to lose my fortune. They recommended I put it in postal savings. At that time, you could go to the post office, hand in your money, and get a gorgeous certificate. The certificates were large, brightly colored, adorned with elegant artwork – and paid 2%. (Nowadays, that sounds like a lot!) Postal savings were considered to be secure. My parents felt that the postal savings would survive as long as any kind of government did. Some years later I cashed in my postal savings, when I learned of ways to get higher interest. I'd accumulated a fair number of those big, gaudy certificates. I felt very lucky and special to be, as I thought then, already well on the way to being financially independent.

BF: So for how long did this go on, that you were financially independent?

DH: My orchard earnings continued for the next five or six summers, after which I found other jobs. In the orchard work, when prune-picking was over, I harvested apricots and walnuts for another few weeks, although that paid less well. Apricots you picked from the trees, then cut them, took out the pits, and spread the cots on large trays that were stacked into little houses where they were treated with sulfur. Walnut picking came last, in the three weeks or so before school started. That was interesting, because the kids who picked walnuts got ugly stains from the shells on their hands. Those stains could not be washed off and lasted for about a month. At school, some kids looked down on those with stained hands, so I personally experienced early on something of what discrimination was like. I think that influenced my political outlook much later in life. As a kid, I noticed but didn't feel wounded personally by others making fun of or looking askance or saying unkind things about my dirty hands. Actually, I felt proud of those hands, because they got dirty doing what my aunt had predicted.

BF: But you were also a sort of champion at this work.

DH: Yeah, that's how I felt.

BF: And as we know, it's a very rare thing to be good at anything.

DH: Yeah. It's very good for kids to feel they're good at something at an early age.

BF: You mentioned to me once that you were very impressed by observing your father doing the work that he was doing very meticulously and very well, and that he put an extra brace here and there although the customer would not see it.

DH: Exactly. As mentioned, when I worked with him he was building one house at a time. He would draw the plans and do everything in the construction: initial excavation, pouring concrete for the foundation, framing the walls and roof, plastering, plumbing, wiring, bricklaying, roofing, kitchen cabinets.... He did it all. Often he'd say: 'This is the way a good mechanic would do it.' By mechanic he meant a craftsman. He took great pride in his work. It was a very important lesson he wanted to transmit to his sons: that how he felt about his work was more important than how anyone else felt about it. Later, I came to recognize that his integrity and pride in his work was indeed very deeply engrained in me and my brothers. And also to recognize how precious a legacy it was.

BF: Yes, I can be your witness in this respect. This brings me to a more general question. In earlier times it was customary to speak of moral and intellectual education; although extinct, I'd like to resurrect this categorization for a moment, and ask you about the moral precepts that you received in your childhood and youth that shaped your moral perceptions and attitudes the most. So, this was apparently one...

DH: This was a very important one. The conventional moral education provided by Sunday school and church and the Boy Scouts was also significant. Our family regularly went to church but only up until I was about eight or so; I remember that because we always had ice cream afterwards. Later, in high school years, I went on my own and sang in the choir. During those years I was emotionally devout, although reading the Bible had made me much less intellectually devout. Yet I thought that the yearning people had for approval and protection by God was important and fundamental. That yearning was reinforced by the Depression. For most of the people that our family knew, the Depression had shaped their whole lives. Where we lived at the time I was picking prunes, many of our neighbors had come from Oklahoma to escape the great Dust Bowl tragedy.

BF: As described by John Steinbeck.

DH: It was *The Grapes of Wrath*. I knew exactly what Steinbeck was writing about. A lot of my best friends at school were from Oklahoma, so their mind-set affected me, and lingers still. Many of those people were very religious. The family of one of my best friends were Holy Rollers. I'd go with him to their church and see his parents and other adults rolling on the ground, crying out as if they were inhabited by Satan. That was scary to see; I felt very sorry for them but the rolling also struck me as futile and demeaning.

BF: Emulating one of the saints, I think St. Benedict, who apparently used to roll in thorns and nettles.

DH: Uh-huh. At any rate, the traditional virtues preached at church didn't mean as much to me as the attitudes and behavior I saw in my own relatives and their friends. Your question about moral education reminds me of a gathering of the clan that my mother many times organized at New Year's. There was no TV, so lots of family storytelling. A lot of the stories, although not overtly religious, described acts that were admirable in a moral way. The storytellers weren't trying to teach moral lessons; it was just that these people were basically good and also proud of it; they wanted especially kids to know that.

The old-fashioned word 'character' comes to mind. Just this Saturday [March 3, 2012], at the celebration of the life of Norman Ramsey, many people referred to his character. Last Wednesday, at the symposium marking the centennial of the birth of Ed Purcell, that was said of him too. Likewise for my PhD mentor, Bright Wilson, and my undergraduate mentor, Harold Johnston. All who knew them felt great respect for their character. It had strong appeal for me, partly because my background made me value highly the attitudes they conveyed, of total integrity and admirable standards in personal as well as scientific matters. So I've felt grateful all along to have had such role models. Again, there's the ontogeny replicating phylogeny. From your parents, your teachers, your mentors, all these people who nurture you, you get moral as well as intellectual insights that you feel the privilege and responsibility of passing on as best you can to the next generation. You are part of this human stream, and you want to measure up to what your forebears have done. And to what you see in your progeny, because if they act in an admirable way, you're very proud. You feel, 'Oh, this is how I hoped it would be.'

BF: May I ask, how did World War Two affect your childhood or youth? When Pearl Harbor happened you were nine.

DH: In our neighborhood a lot of young men went off to war. Patriotic energy surged, everyone wanted to contribute in some way to the war effort. My dad went off to distant sites to work on building military barracks and plants for production of war supplies. Victory gardens sprouted everywhere. All farming activities of course expanded – among them my prune-picking. When I was old enough to join the Boy Scouts (you had to be 12 then), I took part in several drives to collect paper. And another drive to collect thistle-like plants that produced fluffy, cotton-like stuff used to make something involved in military gear, although we never learned just what it was. I remember helping to harvest truckloads of the stuff, because a lot of the plants grew along a creek near our house.

FDR might as well have been a saint, at least in our neighborhood. People who had suffered so much in the Depression regarded him as their savior. His famous fireside chats had the character of evangelical sermons. In chairs encircling the radio, people listened with rapt attention. All seemed aware that they were living in an era of far-reaching historic transitions. That awareness extended to high school, which I entered in the Fall of '46. Many of the teachers were returning from the war. They quietly conveyed to the kids something of the enormity of the war. Of course, we kids knew some of the boys, not so much older than we were, who had not survived the war. Maybe I only imagine it now, yet looking back it seems to me that students right after the war sensed an implicit but pervasive message: 'You are so lucky to be able to enjoy hard-won peacetime, but you've got to be serious about your opportunities.'

BF: Could you say more about your Japanese friend in high school?

DH: The very first day, waiting outside the door for the first class, I met Mino Yamate, who became my best friend. He was Japanese-American. His family were US citizens and had long lived in California, but had been sent to an internment camp in Colorado during the war. Everyone knows the story now. It resulted from fear that because these people were of Japanese ancestry they might aid a possible invasion of California. Of course that was a foolish idea but politically effective. Many Japanese-Americans actually volunteered to join the army and rendered distinguished service fighting for the US in the war. Mino and his family were admirable people, very industrious and able. His older brother Henry was a lawyer, and Mino became a doctor. My dad gladly helped them build their house in Campbell. Fine people, in every way. For many years I kept in touch with Mino and his family.

BF: When you reached graduation from high school, America was at war again.

DH: Cold War, yes.

BF: In Cold War, certainly, but also in some hot wars, in particular the Korean War. Were you in danger of being drafted at any point?

DH: The Korean War came when I was already in college. I played freshman football, and the dean of students came to talk to the freshman football team because we were all eligible to be drafted. He said, 'I've done a good thing. I've persuaded the ROTC (if you were in ROTC you weren't drafted) to take in any of our football players, so you won't have to be concerned about being drafted.' I refused. I thought that was wrong. The dean called me in personally, one on one, to try to persuade me that I should –

BF: – enter ROTC.

DH: Yes. I didn't think that was right. I had known, as I say, older boys who went off to war and didn't come back, and, as you know, I wasn't from a privileged part of society. There were kids like that at Stanford, and there were kids like me at Stanford too, who came on scholarship. I just couldn't accept something like that ROTC dodge. However, although fully eligible, I didn't get drafted while I was in college. I could have been drafted as a graduate student too, but Eisenhower put in a policy that favored drafting men younger than 20. I squeaked by in not getting drafted, but if I had been I would have gone.

JR: Can you identify any specific events or influences as a child that sparked your interest in science?

DH: Oh, yes. But first I should give a little background. My parents had both lived in California quite a while. In fact, my dad was born in San Jose, as I was, and so was his mother, so I am a third-generation native of San Jose. My parents both were high school graduates, but we didn't even know anyone who'd gone to college and I certainly didn't expect to. The key event that sparked my interest in science occurred just after I turned 11, on a visit to my grandmother's house in San Jose (she lived a couple of miles from us). There I saw an issue of *National Geographic* magazine with gorgeous star maps. The star maps were in an article by Donald H. Menzel of the Harvard College Observatory. It was certainly the first time I'd ever heard of Harvard, but it surely didn't even register then. When she saw how intrigued I was with those star maps, my grandmother gave me that *National Geographic* and I soon began making my own little pin-prick copies of the maps. There was a locust tree in our back yard that became my observatory. I'd sneak out at night, climb up in the tree, peek at my pin-prick copies with a quick flick of a flashlight and pick out the constellations. Many years later a friend, who had heard this story, somehow found a copy of that magazine (July, 1943) and kindly gave it to me; I still like to look at those maps from time to time!

The grammar school I was going to then, and it's unbelievable if you know what Silicon Valley is like now, was in the orchards. We were bussed more than 10 miles to get to school. The school had about 80 kids in a four-room building, so two classes per room. It had a little library, really just a bookcase, maybe four feet high and four feet wide with five shelves. One of my teachers pointed out there was a book on the planets there, so I read that avidly. I might have wanted to become an astronomer, but I had the impression there couldn't be jobs for more than four or five astronomers in the world.

But the star maps really were what got me interested in science. When I went on to high school, I eagerly took science and math courses just because I wanted to learn something more, with no notion of preparing for college. But I was a good football player, and my coaches said 'Of course you should go to college.' Then my teachers, since I was a very good student, began saying that too. The school, Campbell Union High, was small – fewer than 100 kids in my class. Many were bussed for 25 or 30 miles. Most of the kids were like me, farm kids and few expected to go on to college. At any rate, I was advised to take what was then called a college prep course, along with wood and metal shop courses called vocational training. That's why I signed up for chemistry in my junior year. The teacher, John Meischke, was terrific. He had a Master's degree from Berkeley, really knew his stuff, and was a fabulous teacher. I can tell you some tales about him if you'd like, but I should first back up a bit and mention the way it was in 1946, when I entered Campbell High School. It was right after World War II and many of the teachers had just come back from the war. They didn't talk a lot about the war or sermonize, but somehow made the kids aware that our generation too would have serious work to do.

For instance, the first class I had at Campbell High remains a vivid memory. Soon after we sat down, in walks our teacher, Mr Drummond, an impressively large fellow. The first thing he said was, 'I don't know much about Algebra.' Then he went on to say, 'But I can tell you one thing. In this class, if you calculate by the right method but get the wrong result, you'll get no credit. I served in the Artillery Corps, and if we calculated by the right method but wound up shelling our own troops, we got no credit.' That established an attitude, maintained pretty consistently throughout Campbell High, that the standards were high. The teachers expected the kids to take responsibility for learning what they were supposed to.

Within a couple of weeks, there were several kids in the class who had a better grip on Algebra than Mr Drummond. But that was no sweat for him. As a former major in the Artillery Corps, he considered his job was to make sure the privates and corporals did things up to regulation. So he encouraged students who understood things to explain them to him and the other students. Well, of course, that was great for the kids that understood things; they learned it all the better. I suspect it was not bad for the other kids either, because they listened to peers. It worked pretty much that way all through high school. Chemistry was an exception because, the teacher, John Meischke, was really very, very good.

He, too, encouraged the kids very much to think for themselves. He never lectured us very long, maybe 15 minutes at the start, and then we immediately, every day, went into the lab. The room had chairs in front and the lab was most of the room, so he'd have a little discussion with 20 kids or so and then send us right into the lab. Meischke would prowl around, asking questions of us individually while we were working. So you had to be prepared. Also, he gave us written tests every week; very challenging tests.

JR: Was he a veteran?

DH: No. He had some health problem that exempted him from being drafted. I remember a typical episode in his class. One spring day, we were going to be working with acids and bases, and he was telling us to be careful. He always wore a lab coat and safety glasses. I was probably dozing a little in the second or third row, and suddenly a girl in the front row screamed. Then we saw Meischke had slipped into the sleeve of his lab coat a skeletal hand, then just gradually let it creep out. He didn't say anything. Everybody got the message. Another time I remember that he said, 'Excuse me a moment', then walked to the back of the room. He came back a minute later and wrote the time on the board. He didn't say anything. Then 15 or 20 minutes later we all smelled this ester, this strong fragrance, coming in. That touched off discussion about diffusion. That's the way he did things. He made a great impression.

Physicists might be interested to know what my high school physics was like. Our teacher, Mr Noddin, was famous in Campbell as a former basketball coach. There were stories about his tantrums during games. He would jump up and down on his hat, and chew a towel, or so we were told. On the first day of his class he explained that his only contact with physics was holding the pole for the survey team that laid out the baseline for Mount Tamalpais. He would write on the board a series of words with dashes between them and say, 'When you know those words, you know unit one.' Every demonstration he'd try was a fiasco. The kids would have to rush up and save him when he started pumping water through the vacuum pumps and things like that. But by then, as seniors, we were very self-reliant. His exams were all fill-in-the-blanks. He'd write on the board: 'Newton's Second Law states blank, blank.' He'd supply the prepositions and we were to fill in the other words. Although his course would seem to be about as bad as you could get, the students were interested and we learned physics decently. I came to appreciate that when I went to Stanford. I took, in my first year, chemistry and then later physics.

Undergraduate at Stanford

JR: All right, you have described a good, early environment with good teachers, a good school. So how did you decide to go to Stanford? How did that happen?

DH: Well, I was recruited pretty heavily as a football player. Berkeley, in particular, had a coach, Pappy Waldorf. I remember meeting with him as well as his end coach, Eggs Manske (playful names!). When Pappy learned I was interested in chemistry, he sent me to see Glenn Seaborg, who was on the athletic committee. This would have been in the spring of 1950. Stanford also recruited me. I remember going up there on a weekend and witnessing a game announced as with the 'Stanford of the East;' Harvard University was coming out to play. Stanford had an unusually weak team that year. They won only one game. They beat Harvard 44 to 0. My high school team would have beaten that Harvard team. The return engagement was cancelled, so Harvard and Stanford haven't played since.

I was a good football player. I started as a freshman, already, in high school. I played right end, and you played both ways then; I loved to play both ways. I made All-County, All-Pop Warner, and things like that. Actually, I got telegrams from quite a few colleges that I hadn't even applied to, congratulating me on being admitted. I was offered a football scholarship to Stanford; but I was also offered an academic scholarship, which was actually better. That's probably not true anymore, I'm afraid, but it was then.

In high school I also played basketball and tennis. Taking up tennis was a result of Mino Yamate's influence as he played first singles on the team. I played first doubles with Harold Taylor. In our senior year we were co-captains of the football team; he was center, I was right end. In tennis, we were called the 1st Armored Division. We were big guys with big serves and just blew away our opponents. We were undefeated for three years.

But what I started to say before I digressed was that I was terribly interested in football as an intellectual thing. I had shoeboxes full of 3×5 cards with all sorts of systems of plays, and I read lots of books by and about famous coaches and football players. You probably had the same feeling about academic work that I had in high school. I felt that I really had total mastery of all the academic subjects I'd studied. I had almost no inkling of what was ahead of me when I went to college because I didn't have any way of knowing how much more there was; that I was just in the foothills of a great mountain range. When I got to Stanford as a freshman, the football team arrived a couple weeks early. So the first thing I remember was seeing all these guys out for football, and some of them were very good. But within three weeks some of the best talent wasn't there anymore. For one reason or another, college wasn't the place for them.

So one thing I learned from football is that the people who are the most naturally gifted in any field may still not be the people who really are successful in the sense of really contributing and doing something noteworthy, because it takes a combination of things for that. Of course, you see that in science in particular. I remember that, during orientation week, before the start of classes, we freshmen dutifully went to a talk by a dean, held in a big auditorium. The dean was saying how important ideas were, and I was thinking, 'That's a bunch of malarkey.' My parents, although they were always very encouraging and happy with whatever I wanted to do, and it was the same with all their six kids (I should have said I was the oldest of six), they were still dubious about my going to college, as the first kid, of course, to do so. Although we didn't know anyone that'd gone to college, they'd heard stories of people who did and became arrogant and too proud to work with their own hands, and there were a lot of funny professors there with egghead ideas. The impression was that what you learn in college really didn't have much to do with the real world, so you didn't take it all that seriously; it was just a social kind of thing. College was for an upper class who felt it was necessary for their kids, but not real people. That was really their attitude. Of course, I learned they were partly right. But very soon discovered there was much more to college.

Anyway, I enjoyed playing football enormously, and indirectly it helped my academic performance as a freshman in a major way. I was so weary after football practice every afternoon that I couldn't really study very long in the evenings. But I also had a weird schedule, as it happened. I had six 8:00 AM classes, MWF and TuThSat, and nothing then until noon, when I had five 12:00 classes MTuWThF, and then I had three 1:00 classes MWF. So I had 9:00-12:00 free every day. Well, it was obvious that was about the only weekday time I was going to have to study. Also, I soon discovered that near the freshman dorm there was the Hoover Tower. I wandered in there and I found the spacious reading room nearly empty, so I started studying there.

Well nothing could have been better. For four years I studied at the same desk in the Hoover Tower, in a beautiful wood-paneled and serenely quiet room. So I had those three hours a day. My roommates, of course, hardly saw me studying – this jock sacked out not long after dinner. One of my freshman roommates was from Lowell High School in San Francisco, which was regarded as among the academically strongest in the state. I was very surprised when I got A's in all my courses, even an A$^+$ in History of Western Civilization. None of my three roommates got any A's, even the fellow from Lowell, who had been much more confident than I was. So all of us were astonished at my academic performance, which I think benefited a lot from the study regime imposed by football plus my unorthodox schedule.

To do so well surprised me, because I had been told by my high school teachers, 'Well, you got all A's in high school, but it's going to be all B's because it's tougher in college.' But it wasn't tougher. For instance, I loved history, to start with, and History of Western Civilization, which all freshmen had to take, was the most rewarding course I ever took, partly because it was taught in sections entirely, it wasn't lectures. They had a wonderful syllabus and a special library with the resources cited in the syllabus, and we went there to read. They told us every week to be prepared for our three discussion sessions. It just opened my eyes to so many things I had observed that seemed strange and mysterious; I had no idea where the immense variety of institutions and traditions came from. So I saw all of that in this course.

It was a great course and I really seriously began thinking about majoring in history. But I loved science, and I knew I could read history already. It was clear to me that unless I went further into science I soon would not be able to stay connected with it and feel I at least understood it. My dad had urged me to be a doctor. He said, 'Don't be a lawyer; they're all crooks. But doctors are okay.' I remember asking lots of my classmates who were planning to be doctors why they were doing it, and so many of them would say, 'Because, well, it's a good racket.' I didn't like that. I came to feel that's probably a good thing if you're going to be a doctor, because you can't be too softhearted. I wasn't cut out to be a doctor. I think because I may have put down chemistry in some preliminary query, I was assigned as an advisor Harold Johnston, who was an assistant professor then in chemistry. He had a huge influence on my later career. He was my freshman advisor and then all the way through college.

JR: You came out of a high school with having been influenced by a chemistry teacher, and then you got assigned, or did you ask for, a chemistry advisor?

DH: Well, as I say, I think probably in some preliminary thing, where they want to know a probable or possible major, I may have put down chemistry because of Meischke. It was the most intellectually interesting experience I'd had in high school, other than the football plays that I'd worked out. I suspect that's how I came to be assigned to Harold Johnston. I can remember still, going with several other advisees on the Sunday before classes started, to meet Harold Johnston. We met in a little room by his office. He was then a very shy guy, but he told us what a university was all about.

Unlike the dean, he didn't give us some business about how important ideas were. He said, 'The University has three missions. It preserves knowledge', well, that was obvious, with libraries; 'It transmits knowledge', and I knew about that; that's teaching, 'and it creates knowledge.' Well that was a completely new idea. I thought a university was just sort of a higher-level high school, and then he told us about research. He even told us something about his own research. I'm sure that's the first time I'd ever heard of research. I remember his telling us about his field of chemical kinetics. You had usually to contend with a network of reactions, lots of things going on, and there were these intermediates that you couldn't observe directly. I remember thinking, 'Gosh, it's wonderful that guys like him try to figure all of this out. It sounds practically hopeless.' And it was pretty hard in those days to ever know what you're really doing. It is amazing what chemists figured out, regardless.

JR: By the methods they had.

DH: The methods they had, which all dealt with substances in bulk. You could vary the temperature, you could vary concentrations, and then you'd have special things like isotopes. More and more they were beginning to make use of spectra to follow molecules. In fact, that was one of the key things Johnston had introduced, following fast reactions spectroscopically.

JR: You said in something I read that you took ten courses a semester.

DH: That's right. Here's how that happened. First of all, I should explain that I played spring practice in football and then quit, for two reasons. One was that the rules changed and allowed unlimited substitutions. So in spring practice, unlike the fall, suddenly there were four times as many coaches and players were assigned to either offense (my case) or defense. It showed immediately that the game didn't belong to the players anymore. People forget, but it used to be that substitution in football was just as limited as in soccer. You could send in a punter, but if the coach was seen making motions that looked like they were signaling a play, you were penalized. The whole philosophy was that the game was played on the field by the players, totally different than today. I didn't like playing only one way.

Also, I had found by then, the spring term of my freshman year, so many exciting academic experiences and some vision of more. I've mentioned the history course. The chemistry course I had was a good, solid course, but I was amazed to find, as I alluded to earlier, that my high school course had prepared me so well I really was totally on top of it from day one, no problem. But from Dr Johnston I had learned about research. My English Class was lively and satisfying. Finally, five days a week I had an excellent German course. The teacher, Mrs. Josephson, was such a sweet lady. Everyone in the class, about 15, all loved her. So I had found the academic experience that year very congenial, as it has been ever since.

The summer after my sophomore year Harold Johnston invited me to work in his lab. That proved a key episode in my career. I heard the grad students talking about something called quantum mechanics, and they often referred to probability. So I decided, 'Well, gee, I should study probability theory.' So I looked in the course catalog for next fall, my junior year, and there was somebody named [George] Polya teaching a course on probability theory. Of course, I had no idea he was one of the great authorities in the world on probability theory and also a famous teacher. Well, I totally fell in love with Polya. I took everything Polya taught after that, and when I discovered that he had a sidekick, Gabor Szego, also Hungarian, of course I took everything Szego taught. Pretty soon I was taking every math course I could fit in. Polya has a book still in print in Dover called *How to Solve It*. He was really an eminent mathematician. I remember years later writing a letter to him congratulating him on his election to the National Academy, and he wrote back and thanked me and said, 'Well, of course, I was elected into the French Academy in 1925,' which is a much more distinguished thing.

One thing I got from Polya is the word *heuristic*. I'd never heard it before. Somebody pointed out to me that heuristic appears in many of my own papers; I do find non-rigorous but insightful theory very appealing in chemistry and physics as well as in mathematics.

Polya impressed me so much. He always started with some concrete example, usually a rather colorful, interesting one, and then he drew generalizations from it. He would say so many things I've quoted over and over to my students. For example, one thing he said, 'When you solve the problem in mathematics (and it applies more generally, too) look around. You'll find you've solved others. Because doing this is like looking for mushrooms: if you find a mushroom in the woods, you can be sure there are other mushrooms right around, because it always takes special conditions to grow mushrooms and it's never a point sort of scenario.' He was very interested in the strategy for approaching problems. He had a two-volume series called *Mathematics and Plausible Reasoning,* published by Princeton, and much of that is devoted to Euler, an incredibly prolific mathematician. But, unlike most, Euler usually gave the qualitative reasoning that led him to postulate a theorem before he went on to prove it.

Polya would dissect the reasoning and bring out the heuristic aspects. He also taught a course beyond probability theory. I think he had a couple semesters of probability theory and then one called *Higher Mathematics from a Lower Point of View,* and the next term *Lower Mathematics from a Higher Point of View*. He had a wonderful sense of humor. Whenever I got back to Stanford (until he died in 1999) I would go see Polya. He was such a joy. He had a huge impact.

So inadvertently, by my senior year, I'd fulfilled all the requirements for mathematics, and I'd taken a lot of physics, too.

JR: Was physics a second major and chemistry a third?

DH: Yes, I'd fulfilled the major in chemistry, too. For some reason, Stanford would only allow you to get one major. I decided I'd get my undergraduate degree in math because the teaching was so excellent. It was inspiring, whereas it wasn't quite so uniformly so, elsewhere, especially in chemistry. Physics was also good, especially courses in Mathematical Physics. I had a course in mechanics from Daniel Webster, which may be a name you don't know. I can remember now him telling us how he'd built his X-ray apparatus by going to the dump in San Francisco and picking up old coils of wire and things. It was wonderful. I loved the physics department.

I should mention also a course in kinetic theory of gases and statistical physics that I took from Walter Meyerhof, because that's when I first heard about Otto Stern and molecular beams. In a brief digression, Meyerhof mentioned, in perhaps three minutes, Stern's first experiment confirming [Maxwell's] molecular velocity distribution. [In] February [2002], I was in Frankfurt to take part in dedicating a new experimental physics center named to honor Stern and Gerlach for their famous experiment done there in 1921–22. I learned then that Walter Meyerhof was, like Stern, among the many who emigrated from Germany in 1933 when Hitler came to power. Meyerhof was from a very distinguished old-line Frankfurt family. In fact, his father was a Nobel Prize winner in medicine [Otto Meyerhof]. If Meyerhof had not been forced to emigrate and come to Stanford, I might not have heard about Otto Stern, at least probably not at such an opportune moment.

JR: Your introduction to chemical kinetics was really through Johnston.

DH: That's right.

JR: You can probably date your career from that early experience with that professor?

DH: Oh, absolutely. Yes, yes. It seemed to me a fundamental thing to try to understand how reactions occur at the molecular level. Immediately that interested me. In fact, that's why I came to Harvard. I wanted to work with Bright Wilson, because I felt I needed to have more of an understanding of the mechanics of molecules before I could really try to understand chemistry at the level of what molecules are really doing, making and breaking bonds, instead of in this sort of gross macroscopic way that chemists were limited to before. As soon as I heard about Otto Stern, I thought, 'That's the way to study chemical kinetics.' Using molecular beams, you can really find out whether or not a reaction occurs as an elementary step.

Otherwise, it's very hard to tell what is really happening when many reaction steps are occurring at once. That was the problem chemists faced in trying to unravel elementary steps in reactions. Resolving elementary reaction steps is much like establishing the elemental composition of substances. Just as we want to know what elements are in a material, we want to find out what molecular processes are happening in chemical transformations. Ordinarily, you don't have any way to separate out the processes when they occur in bulk. With beams, you could say, 'Gee whiz, I can intersect two beams and see whether products emerge.' I remember telling Harold Johnston about this. He laughed and said, 'But there's not enough intensity.' I did a few simple calculations, because you just use gas kinetic theory, the same thing I was learning in Meyerhof's course, and you could see, 'Well, it should be possible if you had an unusually large reaction yield.' Then I learned about Michael Polanyi's early work with alkali atoms reacting with halogens. Those did have a large yield, and alkali atoms and alkali halides can be detected by means Stern had used.

JR: Still at Stanford. The way you talk and the way your career has developed, you really think like a physicist, I would say, more than a chemist.

DH: Sometimes I do, I'm sure.

JR: No, you focus on a simple system, you want to do calculations, you want to be guided in a way by calculations that give you—

DH: Yes, I like simplicity. I like to understand things.

JR: Well, but that's physics. Did you ever consider physics?

DH: No I didn't, I think because of my roots. I always liked physics. I always have. I think it's such a beautiful subject. But chemistry appealed to me because it's sort of rough, and wild and woolly, and so broad. I've often said to young people, 'Well, if you don't know what to do, but like science and math, chemistry is good. You can do anything. You can just solve equations or run computers or you can raise mice and you'll be a respectable chemist somewhere.' 'But physicists', as Harold Johnston once remarked to me, 'tend to run in packs.' They all say, 'There's this problem right now that matters or this problem or that.' But chemists are more odds-and-ends every which way.

Actually, I think part of the reason that chemistry interests me is philosophical. I remember Meischke's course back in high school was the first thing I'd encountered where I didn't see right away what was the hang of the subject, what it was all about. It took several weeks before I began getting a feeling for chemistry, whereas everything else seemed pretty obvious from the beginning what the basic notion was. It took me many years to appreciate the special epistemology of chemistry, and it's quite different than physics. I say it this way: chemistry is like an impressionistic painting. If you stand too close to such a painting, it appears to be just meaningless dabs of paint. If you stand too far away, it's an equally meaningless blur. But at the right distance you see wonderful things come into focus.

A physicist tends to stand too close to chemistry, looking to reduce things to first principles. The old-time biologist wanted to stand too far away, to avoid getting swamped in too much molecular detail. Of course, many modern biologists have become chemists, for all practical purposes. But the chemist's intermediate domain, where you see the impressionistic beauty emerge, is fascinating. That appeals to me. In physics, you really want to get things down to the absolute foundation, but in chemistry you can't do that for the most part so you have to operate in this way where you blend intuition and rigor. The chemical physicist is trying to put as much rigor in as he can, but for problems that are really interesting to true chemists they still work more or less the way they did in the 19th century and even earlier. You have to bridge that cultural gulf. It's the blend of intuition and rigor that I think appeals to me in mathematics, too. Of course, there are parts of physics that are like that. A lot of solid-state physics is that way. A lot of cosmology too.

BF: When you were a student at Stanford you took a record number of courses in mathematics, physics and chemistry.

DH: Yeah, I probably did.

BF: You would actually have been able to major in all three, but you chose mathematics as your major because Stanford did not allow multiple majors. How difficult was it for you to do the homework for so many courses? It must have been easy because otherwise you would not have been able to fit it in.

DH: First of all, I have to say – this has often occurred to me – I wish you and I could have got together ten or fifteen years earlier than we did, because you know, by the time you and I met in 1982 I was past my prime. By then I was fifty. That used to be old; now I'm almost eighty. I was certainly brighter in my student days, in mathematics in particular, and as everybody knows, mathematics is like music: if you have some feeling for it, it gets easier and easier. And I was ridiculously efficient in those days. For example, as a student I would do any assigned homework immediately, and I mean immediately. In the next class I'd be working out my homework while the guy was talking, because I really studied. I kept ahead. The key thing was never to get behind, so I knew what the professor was going to say, essentially. I knew what the syllabus was and so forth and if there was something to read I always had read it. Well, students don't realize how much easier that makes things. I also had, when I was in college, a photographic memory essentially, and Stanford had a quarter system where the terms went only eleven weeks. The last week was finals, so for ten weeks I could remember practically everything. In Harvard's system, where it goes fifteen weeks or so, I probably wouldn't have done as well.

At Stanford, I had one place I went to study, a certain desk in the Hoover Library. I kept my books and notes there. About 30 years later I heard a lecture by the famous behavioral psychologist [Burrhus Frederic] Skinner about how to write a book every three years. Go to the same place every day, turn on a red light – like conditioning Pavlov's dog – and write a hundred words, between revising old ones and new ones. Just a hundred each day. He put a slide up showing that he'd indeed produced a book every three years. He offered two other pieces of advice: keep a notebook to jot down ideas you can mine for your book; and after your work is published, read it to yourself with appropriate musical accompaniment. Of course, someone in the audience – it might even have been me – asked which music? He said, 'Oh, for my work it's always Beethoven or Mahler.' You write very well, so might do that. I haven't tried it, but I do enjoy reading sometimes a paper that I'd written years before. I've always found writing to be hard work. The computer/word processor is a huge help, because I usually revise every sentence several times before finishing it.

Grad student at Harvard

JR: When did you know you wanted to get a PhD? You got a Master's. You didn't need to get a Master's. You could've gone directly to a PhD, could you not?

DH: Well, in those days it was more fashionable to get Master's. I got two Master's, actually, because in my senior year I applied to Harvard and other places. I already knew that I wanted to go work with Bright Wilson. I had read a lot of Wilson's papers and I was so impressed with them. There's another thing I should mention that was important in my intellectual development. That summer with Harold Johnston after my sophomore year was followed by another summer with him, and then I went to Los Alamos after that in a summer internship. But that summer, that first summer, really was the first research I had a chance to do, and that meant I read a lot of research papers.

At first I was so impressed with everything I read, especially in the *Journal of Chemical Physics*. I could not imagine myself writing papers like that. But the problem I was working on with Harold Johnston was such that I had to read a lot of different papers. After a while I began noticing suddenly, as if scales fell from my eyes, 'Hey, these guys don't seem quite to appreciate what these other guys have done and so they don't really have quite the right perspective.' Then I realized that the authors were more or less ordinary people doing these things. The more papers I read the more I noticed that some were not as good as they should be, and others were better than I would've imagined. Hal Johnston was very, very good as a model because he had been an English major through junior year in college. And then, as he told me, he decided to go into chemistry, partly for patriotic reasons at that time. At any case, when I got to read papers by Bright Wilson and George Kistiakowsky and other outstanding scientists, I began to understand why they were so highly regarded. So I felt eager to work with such a guy in grad school. I don't know if I would have even thought of going to graduate school if it hadn't been for that transforming first summer, in which I was mixed in with Hal's graduate students. After that it seemed the natural thing to do.

My senior year I took the graduate record exam. I remember noticing that all the problems in physical chemistry were multiple choice. It was obvious there was only one choice with the right units, so you didn't have to know anything, just figure out the units. But there were other problems where you need to know colors of solutions and such. Maybe those were why I didn't do well on that exam, and I didn't get an NSF Fellowship. People were shocked when I didn't. I took the exam the next year and somehow did well and got an NSF. Anyhow, because I didn't get an NSF, I stayed at Stanford for one more year and got a Master's in chemistry, working with Hal Johnston.

Then I went to Harvard the following year. I wanted to get a PhD in chemical physics, but that program was administered by a committee, not a department, so did not offer Master's degrees. As I already had a Master's in chemistry, and was going to be taking mostly physics courses at Harvard, I decided to get a Master's in physics. Grad students then tended to get a Master's degree, saying that if drafted to serve in the Korean war, it might enhance an early obituary.

In fact, that first year at Harvard, 1955–56, I took four courses each term, and I also started research in Bright Wilson's group. The courses were terrific. The course in electromagnetism, taught by Roy Glauber was a super performance. He never brought any lecture notes to class, but worked out all the mathematics on the blackboard. Another fine course was Solid State Physics, taught by Nico Bloembergen. We students relished his wonderfully insightful explanations. I was delighted to have a course from Norman Ramsey on molecular beams, a seminar course using the proof sheets for his newly published book, which became a classic. I also audited a course on group theory by John Van Vleck, and one on quantum measurement theory by Julian Schwinger. I already had a graduate level quantum mechanics course at Stanford, taken by only about 10 students, so I was astonished that more than a hundred people – most auditors – attended Schwinger's more advanced lectures. He would arrive in class with a large stack of notes, plunk them on the desk but never even glance at them, as he unreeled a dazzling lecture. Once asked to describe his style, my response was: 'Schwinger was an awesome virtuoso, playing original cadenzas of breathtaking beauty.' I also audited an undergraduate course, I think it was called 'Waves and Particles,' given by Ed Purcell. It too had many auditors there to enjoy Purcell's lucid lectures, often enhanced by strikingly simple demonstrations.

On the other side of Oxford Street, I took another unique course from Bright Wilson, based on his just published book, *Molecular Vibrations*. But the most unique of all was a course given by Peter Debye, who was at Harvard that year as a visitor. I didn't know about that in advance. His course was titled, Introduction to Chemical Physics, but it could have been 'My Life Work.' I was one of only three students taking the course for credit, but there were at least 40 auditors. Debye was fabulous, just fabulous, rightfully legendary as a wonderful lecturer. He didn't assign any homework, although his lectures led at least his three enrolled students to look further into many things.

At the end of each semester, we had one-on-one oral exams from Debye. To this day, I much regret that I failed to make any notes at the time and cannot remember what Debye asked me in the exams. One striking aspect of his approach I must mention. He always presented things – it was all theory in the whole course – in what might seem backwards. He wouldn't plunge in and derive something. He would tell you the result, why it was important, what role it had in history, and what came from it. After that you felt so familiar and comfortable with it that the derivation seemed easy and inevitable. But I can also remember coming out of his lecture, which seemed so lucid, and trying to explain something that had excited me in the lecture to someone else. It was not easy to do at all. So after that I really paid attention to just how he did it. I've tried to emulate him in many ways since. Like Polya, he had a big influence on me, and so did Bright Wilson. I have to talk a lot about Bright Wilson.

JR: Yes, we'll come back to Wilson. In your Master's thesis, you did some things with internal rotation at Stanford.

DH: That's right.

JR: Now, was that the point at which you came to recognize Wilson?

DH: Yes, yes. That and his work on molecular dynamics generally led me to read Wilson's papers. Of course I was very happy to be accepted to join his group and discover internal rotation was a major focus of their current work. I thought of it as 'almost a kind of chemical reaction, changing from a methyl group from one potential well to another.' It was particularly interesting to get involved with that.

JR: I think I know where you're coming to. Let's switch to Wilson. Just talk a bit about what you took away from your association, and then one other question. You knew him as a student; you were his student. And then you knew him as a colleague. How did that compare? Was Wilson different in one case than in the other?

DH: In the most important respects, he wasn't the least bit different. When I first met him, I remember jotting down a note in my little coop book. As you know, I wanted to do my PhD with him. When applying to Harvard, I'd written to him asking that and he responded, saying, 'You don't have to decide before you come.' On meeting him, Wilson impressed me as deeply interested in science, not just in publishing papers. That's the note I jotted down, because I'd already encountered enough people that were more concerned about their social standing and in publishing papers.

But Wilson wasn't like that at all. Wilson had luminous integrity. I think everybody fortunate enough to be associated with him would tell you this right away. I remember Frank Westheimer at the memorial service for Wilson said, among other fine things, that at department meetings or anywhere else when you were with Wilson, everybody behaved a little better. You just could not be as petty as you might otherwise have been in the presence of Bright Wilson. He was that kind of guy. It was his character that made him so special. And he was a brilliant guy. That made an impression too. But it was character as a person that I think counted most. That applies to Harold Johnston, too. A year or two ago, a guy I like a lot said to me, 'Dudley, I don't envy you the science you've done or the awards you've received, but I do envy you the students you've had.' I said, 'Well, that's very perceptive. That's right, I've had fabulous students and I'm very fortunate. But, you know, you should envy me for my mentors too. To have had Harold Johnston and Bright Wilson, who were so admirable as people, and had the highest scientific standards, was a tremendous blessing.'

JR: Were you at all familiar with anyone in Wilson's group who didn't get along with him? I don't know anyone.

DH: I can't imagine that. No, I think he was just tremendously respected by all of his students and all of his colleagues. Frank Westheimer was also such a person. Everybody just felt they had to try to emulate mentors of such outstanding character

First trip abroad

BF: Could you say a little bit about your first trip abroad? What did it mean to you culturally and professionally?

DH: I went to the Faraday Society meeting in 1962, and afterwards to Germany. Both parts of the trip were very memorable. The Faraday Society meeting was held at Trinity College, Cambridge, where Newton had been a Fellow. On the way, I first visited Oxford, at the invitation of David Buckingham. He was then Junior Censor of Christ's Church College at Oxford. That required him to discipline wayward undergraduates, but also provided him with a suite of four spacious rooms and maid service. David took me on a brisk tour of Oxfordian wonders, architectural and historical. The second day of my visit was devoted to intense discussion and calculations, all prompted by

David wondering whether red shifts in stellar spectra might be due in part to Raman scattering by galactic gas. We emerged with a negative answer, but had much fun getting to it. That evening, at dinner in the Senior Common Room, I witnessed David perform a traditional ritual, in which he shinnied up a pole to kiss a beam overhead.

When I arrived at Trinity College, I was again amazed, just as at Oxford, by the profusion of medieval buildings. The meeting had some magic moments. George Porter (later to become Lord Porter) gave a brilliant talk on his study of iodine atom recombination. He kept referring to the third body that catalyzed the recombination of the atoms as a chaperone. Fred Kaufman rose with a seemingly innocent question. He wondered why Porter used the term 'chaperone'; didn't that mean somebody who kept couples apart? Porter, with a huge smile, replied: 'Ah, but on this side of the water, a chaperone is someone who promotes a stable union.' The contrast between Fred's rich Viennese accent and George's elegant Queen's English was delicious. Ever after, whenever I talked about termolecular reactions, I always mentioned this charming episode.

Another episode that I've often described came at the concluding dinner. Nevill Mott was the speaker. He said he was a theorist, but was pleased to have the opportunity to give a talk to a group of primarily experimentalists. Especially here at Trinity College, which had a rich history of outstanding experimentalists. Pointing to a series of portraits in the hall, he noted that Newton did many experiments, particularly in pursuit of alchemy, J.J. Thomson discovered the electron, and Ernest Rutherford discovered the nucleus. Then Mott swung around, swung his hand high as he pointed to a massive portrait behind him, and exclaimed: 'And here is the greatest experimentalist of them all!' The portrait was of Henry VIII, who was a patron of Trinity College.

At meetings 25 or more years later, I've heard the 1962 Faraday Discussion hailed as the harbinger of reaction dynamics as a field because it included talks by John Polanyi on his lovely infrared chemiluminescence method and by me on our early crossed molecular beam experiments. Actually, our talks aroused little interest at the meeting. However, John and I had intense discussions with each other and enjoyed meeting many leading kineticists, especially Eugene Nikitin. After my talk, John told me it was fortunate that I could not see the reaction of Sir Cyril Hinshelwood, seated on the rostrum as chair of the session. When showing slides to introduce my coworkers, I had included one of Isaac Newton, then gone on to christen the velocity vector diagrams we used for kinematic analysis as 'Newton diagrams.' Apparently this offended Sir Cyril, who had scowled more and more furiously during my talk.

I went over to Germany to visit Peter Toennies at Bonn. He'd done his PhD with Ned Greene at Brown and I'd met him when he came by Harvard on his way to take up a Fulbright Fellowship in Germany.

BF: Just for a year...

DH: Yes, but then he wound up there his whole career, and did much extraordinary work with molecular beams. Peter is delightfully vigorous as well as astute. I've always much enjoyed discussions with him, as he brings up searching questions. On this visit, I was surprised that Peter no longer had the gorgeous big, red, fluffy beard he'd previously sported. We went on a hike that led to a little tavern up in the hills overlooking the Rhine. Years later, Peter reminded me that during our conversation there, I'd answered 'very few, maybe three or so' when he'd asked how many labs were likely to eventually pursue molecular beam studies of chemical reactions.

BF: And you went for a steamboat ride on the Rhine, I think. Is that right?

DH: Yes. When I left Bonn it was Easter Sunday, and I took the Rhine steamer up to Rüdesheim. That day changed my life. As we went up the river, more and more people got on, they drank more and more, and had a more and more happy time. I reflected on how I had become a stick in the mud, working so much in the lab, and vowed to experience more of life. In Rüdesheim, I found a rosé wine that I took a great liking to; it seemed to capture both the sparkling, cool sunshine of the day and the serenity of the old castles along the Rhine. For many years after, I sought and often found that particular rosé from Rüdesheim. You must excuse my nostalgia, easily awakened by your question.

BF: That was exactly the idea.

Own research

BF: You concluded your article *Einstein as a student* by invoking a comment about Shakespeare, namely that writing his plays 'must have been easy or it was impossible.' Then you juxtapose this with Einstein's case, saying that 'for Einstein, producing his golden eggs would have been impossible if it had not been difficult.' And my question is how difficult or easy has it been for you to achieve what you did in research?

DH: Of course I made up that comment about Einstein's eggs, to offer a symmetrical contrast with Shakespeare. Einstein himself urged a colleague that 'you must strive to do things the most difficult that you can do at all.' Scholars of Einstein have indeed found his work was not easy for him. He'd thought ten years about special relativity before it finally crystallized for him.

In retrospect, a lot of my research looks easy, more so than when doing it. Much turned out to be less difficult than anticipated. But some things proved much more difficult than expected. Quite a few of those were given up or remain in limbo. For instance, early on I thought I had a good idea for observing the key biochemical process of ATP to ADP conversion rather directly, using NMR of phosphorous nuclei, but the experimental sensitivity was woefully inadequate. Forty years later, others accomplished that via a totally different means. Usually theoretical work, at least of the sort I've done, has been less subject to frustrating hang-ups. There are plenty of exceptions, however. At the outset of developing dimensional scaling for electronic structure, 30 years ago, I thought there was good prospect for hatching an iterative method, akin to Kenneth Wilson's renormalization approach technique. That has still not been achieved.

More typical are frustrating roadblocks encountered in completing a project, when what had been considered a suitable and reliable approach misbehaves. For example, one of my favorite theoretical papers develops an unconventional way to factorize the classical partition function for a molecule. That results in a factor for each atom that corresponds to free translational motion of the atom in an effective volume defined by the average vibrational amplitudes of the atom and the geometrical configuration of the neighboring atoms. At the final stage, checking results by two methods, I found a discrepancy of the square root of two. That bedeviled me for two weeks, before recognizing that the problem arose from a redundant coordinate and could be easily cured.

In our early crossed molecular beam experiments on reactions of alkali atoms we met a serious roadblock. We'd had a quick success with reactions of potassium and methyl iodide and analogous molecules. But when we tried reactions with molecules other than alkyl halides, such as carbon tetrachloride or halogen molecules, they poisoned the surface ionization detector, inducing spurious and irreproducible responses that precluded measurement of reactive scattering. As described in my Nobel Lecture, that stymied us for nearly two years. A simple cure emerged in 1963, from work undertaken with an entirely different motivation in the lab of J.W. Trishka, at Syracuse University. The cure merely required pretreating the surface ionization filaments with oxygen or methane. The efficacy and reliability of this technique was confirmed in our lab by using a deflecting magnet to distinguish between the alkali halide reaction product and nonreactively scattered alkali atoms. The deflecting magnet was essentially like that used by Otto Stern and Walther Gerlach in their famous experiment 40 years earlier. Another case of ontogeny borrowing from phylogeny.

The frustration by poisoning of the detector had set in shortly before Yuan Lee arrived at Berkeley as a grad student and wanted to join my group. Of course I told him about the poisoning problem. Also said that, since we could not be sure we could get much further with reactions, we might wind up doing spectroscopy. I actually had designed our apparatus so that it could easily be converted for spectroscopy in case reactive scattering experiments didn't pan out. As you know, Bill Klemperer undertook molecular beam electric resonance spectroscopy, and developed it into a fabulously versatile and fruitful method. Fortunately, Yuan did join my group a few years later and led the construction of the apparatus that took us beyond the alkali age, thereby immensely expanding the scope and sophistication of the field.

BF: So how close were you actually to bifurcating into spectroscopy?

DH: Not all that close, because I just felt there had to be a way we could lick the poisoning problem. Later, George Kwei told me that the graduate students had talked among themselves and concluded 'Well, OK, Dudley's still determined to get through this, let's keep going on this for another six months or so.' By then the cure had emerged, but a year or a year and a half, in the life of a graduate student is a long time. For me too because I felt responsible for getting them into this. Meanwhile, we did a thorough job on the alkyl iodide reactions, with K, Rb, or Cs and including alkyl groups with up to seven carbons. Also built more apparatus such as the deflecting magnets and analogous electric fields that proved very useful. Although we were impatient to study other reactants, the alkyl iodides were very interesting. Our $K + CH_3I$ experiment provided the first data on the angular distribution of products in a chemical reaction. As the KI emerged mostly backwards from the direction of the incoming K atom, we dubbed that a 'rebound' mechanism. It was found for all the variants of alkali atom + alkyl iodide reactions and later for many others. Velocity analysis of the products done a few years later showed the rebound involves repulsive energy release comparable to that in photodissociation of the target molecule. So the rebound is pretty dramatic, particularly when the incident atom is as heavy as a Cs atom.

BF: Almost like the Rutherford scattering…

DH: Not quite as dramatic as that, but often when talking about it I refer to a cereal known as Rice Crispies. The cereal box says 'shot from guns', because it emits popping sounds as you chew the cereal.

As soon as the detector poisoning was cured, we did reactions of alkali atoms with the diatomic halogens, Cl_2, Br_2, I_2, and found the alkali halide product distribution peaked strongly forward with respect to the attacking alkali atom. That resembles the stripping mechanism familiar in nuclear physics. Ironically, those experimental results came only a few days after Don Bunker published in *Scientific American* a nice review of computer simulations that led him to conclude that all chemical reactions would be rebound. It turned out the simulations were misleading because of a generic flaw in the way the three-body potential energy surfaces required for the collision trajectory calculations were constructed. That was done by summing two-body interactions, and thereby typically introduced little bumps in the three-body surface. Those bumps, at the collision energies involved, tended to reverse the trajectories, so favored rebound rather than stripping product distributions.

BF: You called the bumps warts.

DH: Yes, little warts reverse the trajectories and hence the perspective of viewers. (That can happen in human chemistry too!) Later, Godfrey and Karplus much improved the computer simulations and obtained stripping for the $K + Br_2$ reaction. They constructed their surface in a way that guaranteed it would be smooth, by solving a secular equation. As you well know, in such a calculation various matrix elements push this way and that, but the resulting surface is free of warts. The poisoning and wart episodes came in the same time frame, so illustrate how fledgling research is likely to be bumpy. It was part of the excitement. Every little thing we did back then was fresh, giving brand new results, often unexpected. Within a few years, both experiments and theory had become much more civilized, but at the beginning both were rather primitive.

For instance, consider the product angular distributions. At the very beginning, nobody knew whether to expect a preferred direction. The crossed beam experiments found, in succession, classes of reactions that exhibited backward, forward, sideways, and symmetrically forward and backward peaked distributions. And quite elementary theory related those classes to the electronic structure. In retrospect it looks like child's play, and it really was. So, returning to your question: it was easy because we loved so much the possibility of doing it that the problems along the way didn't seem all that hard. To other people, it looked as if it should be difficult. Even for the unusually facile alkali atom reactions, the product yield in the early crossed beam experiments was ridiculously small, as viewed by chemists. It was less than a monolayer per month. The surface ionization detector made that yield quite adequate: it turned alkali species into ions with 100% efficiency and ignored anything else. What a blessing! Going beyond the alkali age was genuinely difficult, but made much less so by the experience gained with the rudimentary apparatus that sufficed for the wonderfully congenial alkali atom reactions.

BF: I wonder whether you could say something about surprises in research, good and bad, or plums versus rocks in your vocabulary.

DH: Well, the plums of course are things that turn out to work or give you results as good or better than you were hoping for. The rocks are the opposite. I haven't tried to make a list of plums and rocks. Of course, we remember the plums much more than the rocks. As I've just been saying, at least in our molecular beam odyssey, plums far outnumbered rocks. Although we tend to suppress rocks, I think the ideal experiment actually delivers at least some pebbles. It gives a response from Nature enough like you were anticipating that you can recognize it, but significantly different. If the response is exactly what you expected, that's disappointing because then you haven't really learnt anything beyond what you knew in designing the experiment. (Of course, sometimes such a confirmation is welcome!) But when pebbles or sizable rocks appear, you are challenged anew. It helps to have your nose rubbed in your lack of understanding.

Among plums, the best convey or provoke new insights, more gently than do pebbles. I'll mention as a nice example some results from collisional excitation of chemiluminescence. Enzo Aquilanti, when he was here as a post-doc, worked with Roger Anderson on these experiments. We had an apparatus designed to accelerate alkali atoms up to a range of tens of eV or so, a suitable range to excite electronic emission. For instance, take potassium atoms hitting oxygen molecules. In scanning the collision energy, we saw emission from excited K atoms appear at the expected threshold energy, denote it E*, and were startled but pleased to see it climb very steeply in intensity. But more startled to see, just a couple of eV above threshold, the emission intensity drop steeply downwards. That was a surprise, but qualifies as a plum rather than a pebble, because the drop occurred just where the threshold for producing an ion-pair, $K^+ + O_2^-$ was reached; denote that $E_\pm$. That made it obvious what was happening. We called it an 'internal reflection' mechanism. The incident K atom likes to transfer its valence electron to the O_2 molecule, which is electrophilic. That transfer evidently happens well below the $E_\pm$ threshold where the ion-pair can separate. At energies below $E_\pm$ but above E*, the trajectory of the nascent ion-pair must travel out the attractive Coulombic asymptote, but does not have enough energy to escape and emerge as separated ions. So the trajectory has to reverse, bringing the ion-pair close together again. This internal reflection can happen many times, more and more often as the available energy climbs nearer to $E_\pm$, but below that the electron eventually hops back onto the K atom, often into the excited state that yields emission. This neatly explains why the emission increases so strongly as $E_\pm$ is approached from below, then drops abruptly above, where the ion-pair can finally escape.

My favorite plum is one I cheerfully said I wanted to have on my tombstone, when John Rigden had brought up this issue...

JR: So what's on your tombstone?

DH: The tombstone could have the contour maps for the $H + Cl_2$ and $K + CH_3I$ reactions. I've called them 'kissing cousins' because of their 'first-born' roles in John Polanyi's work and in ours. The congruence between the maps shows that the 19th century notation we still use to write down chemical reactions is very misleading. The maps say the reaction dynamics of these cousins is actually the same, even though one involves just covalent bonds, the other transition from covalent to ionic. So this gives us a new perspective: we need a notation that helps us to understand and characterize dynamics. Then we could recognize cousins among nominally very different reactions. The contour maps for the cousins have appeared in several review articles. The one I'm showing you now came from a talk I gave at the 90th birthday party for Pauling, in 1992. (It's in the book *The Chemical Bond,* edited by Ahmed Zewail.) I focused my talk on things we'd learned from Pauling. First came a picture depicting the development of physical chemistry in the 20th century. This was from my Nobel Prize talk and emphasized cultural changes. The broad foundational era of thermodynamics concerned with macroscopic phenomena remained dominant until the 1920s. Then came the era of molecular structure that prepared the way for the era of molecular level dynamics. I went on to illustrate links to three of Pauling's favorite themes in electronic structure: electronegativity, hybridization, and resonance. Electronegativity is not rigorously defined but very valuable in heuristic chemical thinking. It is manifested in a simple way in many of the reactions we studied. Favorite examples were reactions of hydrogen atoms, halogen atoms, oxygen atoms, or methyl radicals attacking I-Cl. In each case, the attacking group bonds predominantly with the I atom, although that bond is much less strong than the bond to Cl would be. The large electronegativity difference between iodine and chlorine provides a neat explanation of that and other features of the dynamics. That difference makes the uppermost molecular orbitals, which are antibonding, predominantly I atom orbitals, as pointed out in the 1930s by Robert Mulliken in his interpretation of fine structure in spectra of I-Cl. Hybridization, another concept very familiar to chemists in the context of bonding, had a key role in creating spatial orientation of molecules for collision experiments.

JR: That's right. You said your goal was in all of this crossed-beam dynamics to understand how chemical kinetics is governed by electronic structure. Have you achieved that goal, do you think?

DH: Yes, I think that's basically what these experiments did demonstrate. Although the interpretations of observed dynamics in terms of electronic structure were chiefly qualitative, such insights are fundamental. Of course, now electronic structure calculations have become much more incisive, largely due to computers getting more powerful. So for three atom systems without too many electrons, such as $F + H_2$, pretty accurate potential surfaces can be calculated. Likewise, full-scale quantum scattering can be calculated using such surfaces. So in such cases the computational chemists can predict what the dynamics must be and how it stems from the electronic structure. None of these things can be done as tidily or for as many systems as we'd like, but all the pieces are there,

BF: 'The splinters from Kisty's axe sprouted into a bountiful garden' is the most graceful description of George Kistiakowsky's lost nerve and his contribution to reaction dynamics. How would you characterize Kistiakowsky's contributions to chemical physics that preceded and followed his failed attempt at a crossed beam experiment?

DH: First, let me say, the reason I was pleased to come up with those words is that I just had profound admiration for Kistiakowsky as a scientist, as a personality, and a fine citizen. He was another one of the giants in my personal pantheon. Respect for him of that order is shared by many other people, John Polanyi for example. You've got to see things in their historical context. Harold Johnston, my undergraduate mentor, who was a very distinguished chemical kineticist himself, had immense respect for what Kisty did in the days when you didn't have the kind of tools we have today. One of Kistiakowsky's students, [Herbert] Gutowsky, for example, went into NMR and did great things with that. A lot of his students, again, went on, after apprenticeship in what was a tough game of chemical kinetics, to other fields. Kisty, years after his beam episode, went into shock tubes in a big way. That was a very productive and exciting domain, because it opened the way to kinetics at very high temperature and pressure conditions.

BF: Wasn't Ned Greene his student as well?

DH: Yes, Ned Greene did his PhD with Kisty, and then he of course did key work in the early days of chemical work with molecular beams as well. But Kisty was a great experimentalist. If he'd not done anything in kinetics, he still would deserve admiration for remarkable work he did in thermodynamics. He measured the heats of hydrogenation, which for technical reasons was a hard thing to do, and he measured them extremely accurately. That was the foundation for lots of other important work. And furthermore, he was so versatile. He did some of the first really good high-resolution spectroscopy on polyatomic molecules like formaldehyde. Bruce Mahan was another outstanding student of Kisty's. Although he didn't have suitable equipment for it, Kisty and his student Bill Slichter, deserve praise for the beam experiment alluded to in the 'axe' episode. It was historically important as it drew attention to work of his friend Kantrowitz proposing supersonic beams.

BF: The great Kantrowitz-Grey paper. Arthur Kantrowitz was a true rocket scientist, right? The proverbial rocket scientist.

DH: Right. Slichter and Kistiakowsky attempted very sensibly to verify the predicted behavior of the supersonic beam, using an ammonia beam, to take advantage of trapping on liquid-nitrogen cooled surfaces. But they had pitifully insufficient pumping capacity. According to gossip, Kisty was so angry that he smashed the apparatus with an axe. It was plausible, because he certainly had a temper. When I asked him, he said yes, he did. Now I'm not sure that could be literally true. Kisty was a man of good humor – he might have thought, 'Yeah, that's a good story, I don't want to mess it up'.

Our early beam experiments obviously benefited greatly from the ease of pumping alkali atoms and the target molecules we used by simply installing everything within a big liquid nitrogen trap, which is basically what our original apparatus was. Together with the blessed surface ionization detector, bequeathed by Ellison Taylor and Sheldon Datz, as well as Otto Stern and Irving Langmuir, that trapping made our experiments easy compared with what Kisty tried. As you know, supersonic beams have enormously enhanced both spectroscopic and scattering experiments.

Even a failed experiment, if a worthy effort, has a certain nobility. Especially, if well-conceived but premature because the available technology was lacking. Success is not the only thing. Once baptized, a good idea or quest can live on. It can inspire others. Bright Wilson, my PhD mentor, often pointed out that scientific research is really a kind of gamble. When to quit? When to give up? Nobody can tell you. After learning, thinking, and calculating about all that seems relevant, your judgment comes down to intuition. The 1986 Nobel science laureates took part in an earnest discussion of intuition. It was on an unrehearsed TV program, in response to the question: 'What is the role of intuition?' Rita Levi-Montalcini said, with her beautiful Italian accent, 'Well, I think we don't speak of intuition in science unless it's successful.' John Polanyi responded, with his elegant Queen's English, 'Well, I'm the bad boy here. I don't think there's such a thing as intuition. There's just insight.' Often, I find conversations in different accents are enchanting, especially when the opinions expressed conform to Neil Bohr's dictum that 'A profound truth is one for which its opposite is also profound.'

BF: Seven years before Kantrowitz's great paper, Kistiakowsky was constructing a detonator for the plutonium bomb, right?

DH: Yes. He assembled the explosive trigger for the first test bomb, the Trinity bomb, with his own hands. He was a great authority on explosives. Kisty also wrote one of the first books on photochemistry. As he explained to me once, people thought that photochemistry was going to be the messiah of chemical kinetics. But then found that electronically excited atoms and molecules undergo a vastly different variety of reactions than under normal thermal conditions. Kisty had a sparkling personality, very keen and quick-witted. You know about his service as Eisenhower's science adviser.

BF: Yes, exactly, that was my next question. So it was him who inserted into Eisenhower's farewell speech as President the phrase a 'military-industrial complex', is that right?

DH: Yes. Kisty got on very well with Eisenhower. He liked to tell about teasing the President by pointing out that in his career in the military he lived in a communistic society. I can almost see Kisty here now: tall, with a twinkle in his eye, very engaging, explaining how there's only one law in politics – not two as in thermodynamics. In politics, he said, the only law is 'you scratch my back and I'll scratch yours.'

As you may know, Kisty was in the last boat of the White Russians escaping across the Black Sea, and he hadn't even graduated from college. He went directly to [Max] Bodenstein in Berlin and said 'I want to work with you.' Bodenstein was a *Geheimrat*-type professor, typically visited his lab wearing white spats and carrying a cane. On his first visit to see Kisty, his new student, Bodenstein admired the nice glass vacuum apparatus Kisty had set up; that was essential for all gas kinetics experiments in those days. He asked Kisty if he had made it, and Kisty admitted he'd the glassblower do it. Bodenstein promptly smashed it to bits with his cane, saying 'No student of mine will have the glassblower build his apparatus', and walked out. That led Kisty to become a legendary glassblower. I'd heard from Hal Johnston before I ever met Kisty that only he and Bodenstein could make an extraordinary greaseless glass stopcock. It involved a thin glass bellows that enabled pushing a tapered piece into a conical hole that sealed so accurately it would hold a high vacuum. I learned to glassblow in Hal's lab and made Bourdon gauges, if you know what they are, but they're nowhere near as difficult.

BF: Bodenstein was a successor to Nernst. It was considered the heyday of physical chemistry in Berlin, because at the university there was Bodenstein, at the Technical University there was Max Volmer, and then of course there was Fritz Haber's Kaiser Wilhelm Institute with its all-star team that included Herbert Freundlich, Michael Polanyi, Eugene Wigner, Henry Eyring, Karl Friedrich Bonhoeffer, Hartmut Kallmann, the Farkas brothers, to name just a few.

DH: Certainly an awesome bunch, but Kistiakowsky would have stood among them. Here's what I had in mind with the phrase 'splinters sprouted a bountiful garden.' There's a poem by Marianne Moore titled 'Imaginary Gardens with Real Toads.' When I came across her poem, I thought science is like that. A lot of other things are too. Actually, the metaphor is really a calumny on toads, because toads are very nice creatures. They eat a lot of insects. They do no harm at all in a garden.

BF: They change into princesses, sometimes. I wonder what Kistiakowsky would think about the explosive growth of the 'military-industrial complex', that only happened after his insertion of the phrase into Eisenhower's speech.

DH: Yes, he certainly would have been sad to see that.

On research in general

BF: A 'very easy question', how to tell what's good research in science? When you open an article, a journal, how do you tell?

DH: Yes, well, at first glance it may not be easy or even possible to tell. I think I mentioned to you the story of how I looked up, out of curiosity, the very issue of Philosophical Magazine of 1913 that has Niels Bohr's famous little paper on the hydrogen atom. In that issue there's a much longer paper by an author whose name I and everyone else has forgotten. He was explaining the periodic table of the chemical elements on the basis of the J.J. Thompson raisin muffin model. I have to wonder whether most readers of that issue when it arrived were impressed with the long article treating the whole periodic table. But likely frowned at the brief paper by an unfamiliar author, actually a young Danish soccer player, who seemed to have plucked things out of mid-air to get the correct value of the Rydberg constant. I've read somewhere that Arnold Sommerfeld instantly recognized the great importance of the Bohr paper, but he was surely an exception.

Science is a human enterprise. I think we assess a scientific contribution in much the same way we respond to a work of art. Whether a performance, a painting, a book, or architecture, we value art highly if it changes how we feel or think about something fundamental in our life experience. Such a response of course depends very much on the beholder's own background. The most esteemed artistic creations have wide and enduring appeal across human cultures and generations.

Judging what's good science usually requires more specialized knowledge than assessing art (although sometimes the opposite may hold). Good research connects with something fundamental, often much broader than the particular project or problem under study. Generally it seeks insight or a fresh perspective on such fundamental issues and suggests further avenues to explore or provides new tools, conceptual or instrumental. A sure criterion for an important paper in your field: Does reading it change in a basic way how you think about the topic? It's yet more important if it changes what you will do next. So, as with art, the assessment comes down to the impact on other people. That depends a lot on historical context and contingencies. The impact of significant advances in some cases is quickly appreciated. In other cases what initially was considered just an oddity is only later recognized as a major advance. The recent Nobel Prize in chemistry for quasicrystals is a striking example. Linus Pauling, certainly an outstanding crystallographer, was extremely critical. He devoted considerable effort to analyzing data and concluded quasicrystals were a fluke, not a real phenomenon.

BF: A question from a historian of science, Dieter Hoffman: has your study of the history of science inspired your research, perhaps even in a technical way? Dieter knows, of course, that you've talked a lot about the inspiration you got from the work of Otto Stern, and I think there are quite a few other examples that you've discussed, but the emphasis of this question is more on technical aspects.

DH: Right. The inspiration, aside from technical aspects, is tremendously important, to my mind. At the end of my Einstein essay, I cited Steven Weinberg's very nice little piece in *Nature* with four bits of advice for graduate students. One of them is to think about what you're working on in a historical context. Psychologically that's a powerful thing, to feel you're part of a progression, and whatever you do other people will build on. That is very important, I think, not only for scientists but for people in the wider world, who we hope may recognize that science is a great adventure of our species.

I've often learned from history things of direct technical value. It's most apparent in our molecular beam experiments. But I also learned much that I made direct use of from reading papers by Hans Bethe, John Wheeler, Eugene Wigner and others about the development of nuclear physics. Models we applied to stripping reactions and to reactions proceeding via 'sticky' collision complexes had direct antecedents in nuclear physics. We used very similar mathematical formulations to those that had been developed for neutron scattering and for the liquid drop model of nuclear fission. Our work on vector correlations in chemical reactions stems directly from that of L.C. Biedenharn on nuclear reactions. Looking into historical perspectives has likewise enhanced or sharpened many of the technical tools I've wielded in quantum mechanics, thermodynamics, statistical mechanics, spectroscopy and collision dynamics. Many of these tools, often used in my teaching as well as research, were actually developed before I was born or soon after. At my now venerable age, they should fully qualify as belonging to the history of science!

Yet, I happily emphasize again the homage our molecular beam work owes to Otto Stern, I.I. Rabi, and Norman Ramsey; most of the techniques we used originated in their labs.

BF: Peter Toennies, during his time in Bonn, implemented in his molecular beam work the Rabi scheme with a scattering center instead of the C-field.

DH: Yes, that's a lovely example. We also used directly the Rabi scheme in its electric resonance incarnation, in collaboration with Bill Klemperer, to resolve the quantum states of reaction products. For example, CsF from the $Cs + SF_6$ reaction was analysed that way. We observed directly the distribution of CsF over its vibrational, rotational, and space-quantized states.

Honors

JR: Let's talk about honors next.

DH: We can dispose of that pretty quickly.

JR: You've gotten a lot of honors.

DH: Yes, I certainly have. Goodness.

JR: Is there one that you prize the most?

DH: Well, of course, I have to mention the Nobel Prize, because that's so sobering. When it came I thought, 'Oh, my gosh.' People seem to value it so highly. I've always told my students that the best prizes are given by Nature. The prizes really should go to atoms, molecules, and ideas, but we don't know how to do that, so give them to people instead. As you know, we have the privilege of working on things that we get excited about and the joy of working with other people who share the excitement, our students and colleagues. I can't think of any prize that can compare with that. I think of Nature as an angel up there. She sees these eager, curious mortals down below fumbling around. Now and then she says, 'Well there's one who seems very earnest and hardworking, so I'll drop him a plum.' By plum I mean a happy insight. I've gotten my share of plums.

JR: You've got a lot of plums.

DH: Yes, I've got a lot of plums. As an old prune picker, I appreciate that.

JR: How did the Nobel Prize change your life?

DH: Of course, I've been asked that same question many times and I say, 'Well, nobody wanted to interview me before that.' Many people invite you to give talks, serve on boards or committees, foster programs, endorse petitions or book jackets, write letters of recommendation and such. It adds up to a lot. Much you have to turn down, as either not worthwhile or feasible, but many opportunities are things you feel glad to be able to do. I especially welcome opportunities to talk about science education to all sorts of audiences and take part in science fairs and other activities with high school and middle school kids. You can't help but be aware that the Nobel Prize changes the way people act toward you. I've never liked that. Beforehand, I'd noticed it and wondered, for instance 'What is it like for Purcell?' Knowing and loving Purcell, I didn't think he wanted that. Of course, there are times when it's quite delicious, even absurd. One of the most satisfying things was getting letters and calls from former students and friends who were so happy about it. The press conference and reception in the Chemistry Department was the same way. People were really excited, so much so that I automatically behaved more calmly. A few days later, I bumped into a good friend, a psychology professor at Harvard. I asked him, 'Have you psychologists figured out this phenomenon? Something like this makes people get so unreasonably happy.' He said, 'No, we psychologists don't study happiness, only unhappiness.'

JR: Did you anticipate the prize?

DH: No.

JR: No, really?

DH: No, I absolutely didn't. Actually, several people had thought fit, even people I didn't know, to send me letters years before saying, 'I've nominated you for the Nobel Prize.' I'd always written back, 'I'm grateful, and pleased that you think so highly of my work.' But I really didn't think that the sort of thing we were doing, as much as I loved it, was likely to bring a Nobel Prize. So I did not expect it at all. When it did happen it was awkward in one way. I felt sorry for a couple of other people who were outstanding in the field and clearly were hoping to get a Nobel Prize. Knowing that, I actually phoned them. One fellow said that his colleagues all seemed to be avoiding him and he thought it was because they'd expected he would have gotten the prize. As you know, in a given year no more than three people can share the prize.

The way I learned about it was odd. I was just getting ready for my class and my secretary called, saying 'Somebody wants to speak to you about the Nobel Peace Prize.' I thought that was strange. A few times I'd gotten phone calls from reporters after the Nobel Chemistry Prize was announced. If it went to somebody I knew, sometimes the reporter would say they'd suggested getting 'a quote from Herschbach.' For example, that happened with Roald Hoffmann. So, I thought this call was something like that. The Associated Press identified himself and said, 'I'd like to get your comment on the Nobel Prize in Chemistry.' I replied, 'Sure, who received it this year?' because I didn't know. So that's how I learned. The pleasure was all the more intense because it was unexpected and because the prize included Yuan Lee and John Polanyi, two fellows I admired immensely.

When we were in Stockholm we had a very pleasant time with our families. I remember talking with John about our regret that George Kistiakowsky, who had died a few years before, didn't have the pleasure of receiving a Nobel Prize. Kisty certainly deserved a Nobel, and would've been delighted with the Stockholm festivities. I felt the same way about Bright Wilson and Harold Johnston. So you're very aware how capricious awards like that are. You can't take them too seriously. But you do feel a special responsibility because you've been anointed to represent science.

JR: I have two questions. Talk a little bit more. Do you really think that your Nobel Prize has influenced people's assessment of your proposals to NSF?

DH: Well I don't know how to understand otherwise a case like the one where five reviewers said excellent and one good but contradicted himself saying the proposed experiment was both too easy and too hard. When it comes to papers and grant proposals, I'm almost absurdly particular about what I write. I really sweat to try to make it good. When I feel ready to write a paper or proposal I usually think I have understood the problem pretty well. But nearly always I learn so much more from the discipline and effort of trying to describe it well. It's true that I often don't put everything I could in a research proposal, partly because I'm always trying to think of the reader and not overburden or oversell. But I try to say enough to make the case. So the big difference in my 'before and after' experience with proposals is hard to understand. Maybe I'm kidding myself, but I do think a major cause is the view that, 'Well, this guy should be able to get support elsewhere.' As mentioned above, when you get something like a Nobel Prize you feel all the more responsibility to be a good citizen on behalf of science. That takes energy and time. When it turns out harder to get funding to continue research, naturally you feel that's something of a penalty. I've heard from a couple other US laureates that they also had more trouble getting funding after. Yet people imagine it's a snap. Actually, in Europe and Asia it's the other way around.

BF: In order to continue on a more cheerful note: I wonder whether you could retell 'the rabbit and the Swedish royalty' story. It has many charms, but also what could be called a socially soothing quality.

DH: My father was along as a guest on our visit to Stockholm. In fact, he was in seventh heaven, because it turned out that the Swedish rabbit-breeders association was having their annual meeting that same week in Stockholm. In his elder years, he'd made a profession of what had long been a hobby. From his 'Romeo Rabbity' in Watsonville, home to 550 bunnies, he shipped prize-winning specimens all over the world. We happened to have seen a postcard showing the royal family – their kids were pretty young then – and it featured the family pet, a pink Mini Lop. My father actually originated the Mini Lop breed. He had bred the cute little bunnies down from the original Belgian Lop, a big, ungainly critter with very long, droopy ears. He marketed them as house pets, in several color patterns; most popular were harlequin, spotted, and pink.

Among the traditional rites on the day of the awards (always December 10), are a concert during which the King hands out the prizes, followed by a huge banquet (1500 guests) and then dancing. During the dance session, each of the laureates and their guests have an audience with the King and Queen. Of course, when we were ushered in for that, right away I introduced my dad, and mentioned that he'd created the pink Mini Lop breed. Well, the King clearly was dubious about that and said something in Swedish to the Queen. She was extremely gracious, turned to my dad and asked him a few questions about the rabbit. He confirmed various of its properties, whereupon the Queen turned to the King and said in English, 'You see?' The King responded in Swedish, and the Queen then asked my dad a few more questions. He answered gladly, also mentioning some properties she hadn't asked about but confirmed it had. Again, she turned to the King and said, 'You see?' Again he replied in Swedish. After two more cycles, delivering a total of four 'You see?' messages, the King, looking glum, did not say another word.

Research funding

BF: A question about research funding posed by Helmut Schwarz. What would you choose as a scientist: freedom without means, so to speak, without very much funding; or a lot of funding with strings attached?

DH: Of course, it's a diabolical choice. Clearly, we need freedom to do our best in scientific research, but need at least a minimal level of funding to be able to do it at all. Michael Faraday had to write a letter begging to be excused from the work he was funded for, as we would describe it today, which was to improve optical glass. It was very poor in his day, and people wanted better telescopes. He was given funds for that research and worked on it, but it was not congenial for him and he wasn't making much progress. He had clear ideas about what he wanted to try to do with electromagnetism. So he asked to be excused from working on glass so he could look into electromagnetism. Fortunately, he was granted freedom, and made extraordinary discoveries that had far-reaching impact.

Today it seems very unlikely that any funding agency would agree to such an extreme switch in direction as granted to Faraday. A narrow view of 'accountability' is prevalent. Research proposals to NSF are expected to spell out what is going to be done for three years. In my experience, that's utterly unrealistic. If you can see that far ahead, you're not doing research. Somewhere I read that Eisenhower said: 'For war, planning is essential; but when in battle, plans are worthless.' That pretty much applies to frontier research too. What I'd like to see is implicit in Helmut's question. It would be a funding policy that allows some fraction of the budget, which should increase each year, to explore unanticipated ideas. In enterprising research, as you dig into it, often better ideas emerge. Then you need enough flexibility to change direction appreciably when the new ideas seem likely to yield more significant results. Otherwise, for the sake of fulfilling the original agenda, you have to defer working on such new ideas. Postponement is not only discouraging but in practice may not be feasible. It likely would take more than a year to get fresh funding to go in the new direction and meanwhile the grad students or postdocs best suited to undertake it might no longer be available. Particularly in US government agencies, funding policy has evolved to resemble buying goods like furniture rather than fostering discoveries. The funding is much too narrowly directed at defined projects rather than supporting scientists eager to pursue their best ideas as they develop.

BF: One of the defining features, by the way, of the Kaiser Wilhelm or Max Planck Society is to fund people and not projects.

DH: Yes, that's very wise. Bell Labs in its heyday was much like that. For example, Bell invested in solid state physics at a time when it was a very nascent field, and by no means obvious that it was going to revolutionize everything connected with communications. The Bell view was, 'This is fundamental. We'll get the best people here and see what they can do.' As described in a fine book by Jeremy Bernstein, *Three Degrees Above Zero,* that policy proved extraordinarily fruitful. It produced a cornucopia of discoveries and inventions. Perhaps the most profound in impact was the transistor. It came from free-ranging speculations, not from a directed project to improve communications and manipulation of data. At the press conference announcing the discovery of the transistor, the inventors were asked what it was good for. They replied, 'Oh, this is only a proof of principle. We don't know what it's good for.'

BF: It was a solution in search of a problem, as one of the derogations has it.

DH: Yes, and you've written a nice piece about such solutions.

BF: I was intrigued by what was said about the gorilla glass, for the multitouch screens. It was developed by a company which had no particular purpose in developing it. It was very similar in a way to science. They did it because it was interesting and challenging and because they could do it. That's why they did it. And it was just lying there and 'waiting patiently', or impatiently in this case, to be used for smartphones and tablets and stuff.

DH: I've been asked 'what's it good for?' quite often. A classic response was given by Ben Franklin in 1783. He was in Paris, in a crowd of 50,000 people (a large fraction of the population then!) watching the first manned flight in a hot air balloon. It only travelled a couple of kilometers or so. Some skeptical fellow asked Franklin, 'What do you think this might be good for?' His answer was 'What's the use of a new-born babe?' That's often attributed to Faraday, but he wasn't born till 1791, a year after Franklin had died. Actually, historians have determined that Faraday, when asked about his experiments in electromagnetism, answered: 'Dr Franklin responded to such a question by asking what good is a new-born babe. I would just add that we can endeavor to raise it to be useful.'

That pair of answers seem to me 'good for' refuting the question! When research discovers something fundamental, it likely will find unanticipated applications eventually. There are myriad examples. Gorilla glass is a striking one. Charlie Townes said he never expected the laser would be used for eye surgery, or for playing music or checking out groceries. Ed Purcell said he had no idea that nuclear magnetic resonance would be used for imaging people's bodies and for watching what happens in their brains when responding to questions. A scientific discovery really is like bringing into the world a new-born babe. How it grows up through adolescence to adulthood is not reliably predictable.

BF: It will undergo ontogeny as well.

DH: Yes! Here's what I've often said to reporters asking the 'good for' question. Nature speaks to us abundantly, but in many alien languages. She does not offer explanations; it's up to us to ask probing questions and generate our own understanding. In frontier research we try to discover or add to knowledge of the vocabulary and grammar of some strange dialect. To the extent we succeed, we gain the ability to decipher many messages that Nature has left for us, blithely or coyly. No matter how much effort and money we might devote to solve a practical problem in science or technology, failure is inevitable unless we can read the answers that Nature is willing to give us. That is why basic, 'curiosity-driven' research is an essential and practical investment, and why its most important yield is ideas and understanding. We are born blind and deaf to much of Nature's language, and it takes persistent groping and guessing to learn something of it.

JR: Over my life, the number of prizes has escalated. I actually wonder whether science has been helped or hurt by this. There's an industry of prizes now.

DH: I'm asked to write many nominations or supporting letters for prizes. Leo Szilard wrote an essay about that, foreseeing a limit that shut down science because everybody's busy writing recommendations. You could generalize this danger to not just prizes but research proposals. It's really frightening now. In a field like chemistry, you almost have to have a grant for each student, and that requires dealing with several granting agencies and a lot of reviewing. As a good citizen, you have to review many proposals from other labs too. Ed Purcell told me he never applied for a grant in his career, and he didn't think he could have. You have to know Ed to understand what he meant by that second remark. His work was all supported by what we would call a block grant to the physics department, from the Office of Naval Research. The ONR did not ask for proposals, and specified the purpose of the grant in a single sentence: 'For the investigation of the properties of atomic nuclei.' Obviously in that golden era it was appreciated that guys like Purcell were going to do the best science they could. You just give them money and they'll do something special.

The attitude has changed completely now. Rather than supporting people, it's viewed as buying projects. That has many drawbacks. One of the worst is the impact on young faculty. For them, it's life or death to get a grant to students doing research. Young faculty have often shown me very discouraging reviewer reports on their rejected proposals. More often than not, the reviewers are at fault. The reviewing process doesn't make amends for that. Moreover, the process is very slow; for proposals to NSF that are approved, it now usually takes a year or more between submission and arrival of funds. If a proposal is rejected, the applicant is not notified till a year after submission, then has to rush in a new or revised proposal in hopes of getting funding that if it comes at all won't arrive for another year. There's also a bias against truly novel proposals. NSF requires five or six reviewer reports. If a proposal is 'far-out' it's likely that one or two of the reviewers will fail to fully understand it, so the chance gets pretty small of getting nearly unanimous 'excellent' scores, as needed for approval. Less novel proposals, closer to the mainstream, are more likely to be approved because the reviewers are doing similar work themselves. In his autobiography, Luis Alvarez strongly criticizes the current peer reviewing system, saying it's a disaster.

JR: Yes, it is.

DH: Today most graduate students and postdoctoral fellows in science serve as hired hands on a project defined by a research grant. That has at least three bad consequences. (1) It limits their freedom to explore too far away from the defined project. (2) It is, I think, a major reason why the time to complete a science PhD in American Universities has expanded to a norm of six or seven years. For those hopeful of faculty positions, a postdoctoral stint of two or three years is expected. The age of people getting their first independent NIH grant is now 42! This stretching out of the apprenticeship has occurred, I think, largely because a veteran grad student or postdoc is much more valuable, in producing papers to support renewal of a grant, than a neophyte would be. (3) The funding system has also degraded the quality of a PhD. Grad students now tend to take fewer advanced courses outside their research specialty, and faculty teach fewer small advanced courses. That's because the pressure to feed grant proposals discourages students from devoting time to such courses and faculty from teaching them.

If support of grad students, and preferably also postdocs, on grants to individual professors were abolished, the same money could be put into expanding greatly the number of fellowships that students can win for themselves and into block training grants to university science departments. In applying for the training grants, the departments should have to describe a PhD program structured to foster breath, not just narrow specialization. The huge burden on individual faculty members to raise funds to support students would be removed, so proposals would involve chiefly apparatus and auxiliary items. Students would be able to join a research group without concern about status of funding for specific projects. They would no longer be hired hands. I'm pretty sure such a liberating system would alleviate the drawbacks I mentioned. I was a grad student before it became usual to support grad students on research grants. In 1955, when I came to Harvard, 33 of the 35 chemistry or chemical physics grad students entering had their own fellowships, some from NSF, many from private corporations. That certified students as national resources rather than hired hands. It profoundly influenced students' outlook and approach to grad study and the time to complete the PhD was usually close to four years, often less. I'd like to see funding agencies adopt a policy that students awarded a fellowship or training grant who complete the PhD in four years be rewarded with a postdoctoral stipend for a year to work at a lab of their choice. I've talked with many faculty members concerned about the drawbacks of the current funding system. Unfortunately, most fear that reforms of the sort I've suggested might reduce the overall investment in university science.

Own teaching

JR: In addition to your research, which speaks loudly, you've been a prominent teacher on this campus, well regarded. You've probably advised students and undergraduates.

DH: Yes, sure.

JR: So let me start by asking this. You said earlier today that at Stanford, as an undergraduate, Western Civilization may have been the most important course you took. When you advise students, chemistry majors probably, do you advise them to take humanities courses and broadly educate themselves?

DH: Yes. I often point out that people think science is essentially a technical subject, but actually frontier science is better considered as architectural. An architect has to understand a lot of technical things, but it's not the essence of architecture. An architect, at any historical period, has to consider the materials and construction methods available. Those are available to anybody, but the architect sees how to put them together in a way that opens up new possibilities and serves the purpose of the project particularly well. When we encounter that, we recognize it as good architecture. Frontier science is like that.

A really fine architect has to have a broad view of human culture, human psychology, and much else that isn't directly connected with bricks and mortar. Again, I think the same goes in science. I tell students that they will find that in science, as in many other fields, what you will be able to accomplish depends very much on how you interact with other people and how you communicate with them. Many resources that you can draw on are really cultural more than technical. What is called liberal arts education is valuable in science. And vice versa. Like Rabi, I think science ought to be a central part of a liberal arts education which aims to cultivate the habit of self-generated questioning and critical thinking. 'Why should I believe this? What is the evidence?' Those should be habitual questions.

If you study history or psychology or philosophy or whatever, and there you practice critical thinking of a different sort than in physical chemistry, so much the better. It's enlarging your scope and capacity to do the essential basic thing, and it's also making you more aware of human culture. Rabi has a fine statement I've quoted several times. It's in your book. Speaking about humanities and science he says the beauty of them is not in the subject matter alone. He laments that scientists don't communicate effectively with nonscientists, and too often 'we teach science as if it's about the geography of a universe uninhabited by mankind.'

For me it's gratifying to help students see that science is very much a human enterprise. Textbooks often make it look as if it proceeds by brilliant ideas and discoveries by Olympian figures. But I like to emphasize that science enjoys a huge advantage over other human enterprises. The goal, understanding Nature, waits patiently to be discovered. That's why ordinary human talent, given sustained effort and freedom in the pursuit, can achieve marvelous advances. So we all can play a role, whatever it's going to be, and be part of an unfolding saga. We receive a grand legacy from previous generations, try to add what we can to it, and pass it on. Our roles change in different periods of our lives, but even way past our prime we can enjoy gaining new insights. I get much pleasure in seeing the lovely things students of mine have gone on to do.

Liberal education

BF: A question by Michael Henchman: The US is increasingly unable to solve its urgent problems. Values are changing. Morality is being eclipsed by 'what can I get away with?' Financial wrongdoing, corruption and greed abound. This state of affairs can only change through education. Last week the president of Swarthmore announced that liberal arts colleges are threatened because their education is no longer considered relevant. What role do you see for education in our society today?

DH: I completely agree with Michael that education is crucial to contend with these urgent problems. In my view, a liberal arts education is more important than ever. As I said above, science likewise aims to instill the *habit of self-generated questioning and thinking,* of actively scrutinizing evidence and puzzling out answers. As emphasized by Thomas Jefferson and many others, to have a truly democratic society, we must strive to provide the electorate with such an education. In my freshman courses, at the first meeting I explained that my aim was to teach in a liberal arts mode; I called it 'liberal science.' Always I mentioned a definition attributed to James Conant Bryant, a Harvard President: 'Education is what's left after all you've learned has been forgotten.' I suggested it could be restated as 'Education is what's left that you are unable to forget.' If nothing is left, you might have been trained, ritualistically, to do well in exams, but to have something that can't be forgotten you need to take ownership of ideas and habits of thinking. I'd ask how many had taken ownership of a foreign language, noting that exemplifies how to approach the study of science as well as the empowerment it offers.

Then I'd ask how many were serious runners. Quite a few would raise a hand, rather tentatively, unsure of what this guy was after. So I'd tell them that if you run, say, half an hour a day for three months, that much hard pumping of your blood opens up lots of capillaries, your oxygen uptake will double and pulse drop much lower. It's called revascularization. That changes your flesh, not temporarily, but forever! (I used to run a lot; when I started my pulse soon dropped from 72 to 60; after not running for many years because of cranky knees, my pulse is still 60, spot on.) This course, and your college education, aims to change your brain forever! As with running, to do that you need vigorous effort. But a liberal arts education is much more fun. It invites study of a variety of subjects. Each has a somewhat different language and mode of thinking. Exploring such distinct subcultures strengthens your capacity for critical analysis, the unifying liberal art. Quite a few students have told me, decades later, that this sermonette in 'Chem Zen' changed how they approached their college education.

BF: Another question from Michael Henchman: a generation ago graduate programs in chemistry (one could probably say also in physics and math) attracted American students. Today, the programs are filled with students from abroad. Why are American students not attracted to chemistry, or to science?

DH: I think a major reason is that today the time to complete a chemistry PhD in American universities has grown to six or seven years on average. That is unappealing to native-born students, whereas students from abroad are willing to accept such a long apprenticeship. It's really become indentured servitude, not so different than that common in the 18th century. Many American-born students still major in chemistry, physics, or math; but most then go on to medical school, law or business school. Those programs have fixed times-to-degree. Law and business school programs are also much shorter, and even medical school with further training now takes less long than a typical chemistry PhD. Of course, the long and uncertain duration is particularly unappealing for young women. Shortening the time to the PhD is surely very important. As described above, and in my article on *Einstein as a Student,* I think a major factor in extending it so much comes from the way academic science in the US has come to be funded. The basic reform that I advocated there ought to markedly shorten the time. More simple is the proposal by Freeman Dyson to award students the PhD on the day they enter graduate school. Some students I know have in fact done such outstanding research as undergraduates that they could submit a fine doctoral thesis on their first day in grad school. I hope that will happen and thereby shake up what has become too much like a feudal system.

JR: Another thing you can say is that a liberal education should bring a person to be at home in the world they live, in the culture they live in, and if you accept that, then in 2003 science should be an important core part of that liberal arts education. Is it here at Harvard?

DH: It's nowhere near what it should be. Many students come already having been conditioned to think that science is just for a special geek subspecies. They get that message in many ways. That happens in other countries too. Last October I went to Korea to give three lectures. I wound up giving five because my hosts were so concerned that so few young Koreans are going into science. I also heard there talks by a Japanese fellow and an English fellow, and both described the same phenomenon. They had data showing that, just as in the US, native-born students aren't going into science, undergraduate or graduate, as much as they used to. It's worrisome. We recognize some reasons, but basically it's a paradox. In bookstores, never before has there been anything like the wide range of wonderful science materials now available, accessible to any literate reader. Similarly, there are excellent TV programs, on NOVA and elsewhere, as well as myriad websites. There was nothing like that when I was a kid.

For colleagues discouraged about our future scientists, I urge they go see the International Science and Engineering Fair, where I was last week. Or the Science Talent Search (formerly sponsored by Westinghouse, now by Intel). Both of these exhibit remarkable things done by high school students. Both are conducted by Science Service, a small non-profit outfit in Washington. I was recruited by Glenn Seaborg to serve on the Board of Trustees. It was launched in 1921 to publish *Science News,* which they're still doing. It's a superb magazine presenting science in the National Geographic style. When you talk with students at these fairs, it's pretty inspiring to see the high quality of their projects and their enthusiasm. At ISEF there are 1000 students, all winners at qualifying fairs and 900 volunteer judges. Leon Lederman and several other Nobel Laureates regularly attend ISEF and take part in a special program responding to questions from the students, and another meeting with the judges.

I've long thought it's peculiar that a PhD is considered prerequisite to teach at a university, yet the thesis presents only research. We should encourage inclusion of an unorthodox chapter, perhaps just a few pages, that deals with communication in a teaching mode. Whether or not the candidate goes to an academic career, that mode will be very important, maybe even more than technical expertise. The chapter would describe the research in a way accessible to someone well removed from the field, preferably a nonscientist. I usually recommend a grandmother, as I especially admire them: most are kind but not tolerant of pretentious jargon. The chapter could also enable candidates to discuss teaching experiences, innovative proposals or experiments such as work done on websites or apps.

JR: Have you asked your students to do that?

DH: I have suggested it but not insisted. Some have responded in that direction, but there are two practical impediments. They know perfectly well that such chapters would 'not count.' Also, most often the candidate is toiling down to the wire to get the thesis in before some deadline. So in most cases it's not realistic to ask for such 'extras.' That would not be so if it became customary. To encourage it, I think the best approach would be to provide sizable prizes for such chapters. Each department would then need a committee to select the prizewinners, so it would affect the attitude of the faculty as well as the students.

BF: What do you think could or should be the role of the history of science in teaching science?

DH: History is important in teaching science for at least three reasons. First, what I think of as a spiritual message. History makes you aware that you are part of a grand saga. You have an inspiring legacy from the past, you work to enhance it and pass it on to people who will build on it further. That transcends everyday distractions or frustrations. It's a view you want your students to have. Second, it is intriguing to learn what our predecessors experienced, their feats and foibles. Here's a favorite quote, circa about 1850, from James Clerk Maxwell: 'In Science, it is when we take some interest in the great discoverers and their lives that it becomes *endurable,* and only when we begin to trace the development of ideas that it becomes *fascinating*.' (I added the italics!) Telling historical stories can open compelling vistas in liberal science. Third, it often becomes easier to understand some method, technique, or concept if you look into how it evolved. Usually, then key aspects stand out more clearly.

In Chem Zen, I often told historical stories. I'll mention just a couple. Both illustrate how very elementary concepts, rather boring as presented in typical textbooks, had starring roles in epic discoveries. 'What's the matter in the stars' tells about how Cecilia Payne, a young English girl, born in 1900, came to Harvard in the fall of 1923 and by the spring of 1925 turned in her PhD thesis, published as a book titled *Stellar Atmospheres*. She had solved a problem that had long stymied astronomers. For about 50 years, spectra of stars had been collected. These showed myriad lines. From the wavelengths of the spectral lines, the chemical elements present were readily identified. But the intensities of the lines varied wildly from star to star; it looked as if the chemical composition might also do so. Quantum mechanics would later greatly aid interpretation of spectra, but had not yet been discovered. Cecilia used chiefly just two basic concepts: energy levels of atoms occurred in ladder-like patterns and the population of the levels varied with temperature in an exponential way, governed by the Boltzmann factor. She applied those two concepts in ways simple even for freshman chemistry. With ample data at hand, she showed that all stars had essentially the same chemical composition, just had different temperatures. Her thesis has been hailed as 'the most brilliant ever done in Astronomy.'

In 1955, after much further superb work, Cecilia was one of the first women appointed to the Harvard faculty. My wife and I enjoy visiting her portrait, which we commissioned and gave to Harvard; it now hangs in the Faculty Room of University Hall. It shows her as a young woman pointing to the heavens.

Another story, titled 'How Aristotle and Galileo were stumped by the water pump', spins out the dramatic history behind the ideal gas law, $PV = nRT$. It starts with the question why the simple, hand-operated suction pump (still used in much of the world) can't pump water more than 34 feet. The correct answer was not found until 1638, by Evangelista Torricelli ("Little Tower"). He was one of Galileo's students, so one moral of the story is that today's students will likely solve problems that stumped their professors. Torricelli invented the barometer, to verify his answer; in turn that led to the vacuum pump and a long succession of further discoveries and inventions. The story also provided nice questions for my students to consider. Among them: What if Hercules were asked, as a 13th labor, to weigh the earth's atmosphere: how could he have done it, and what should he have got as the answer? How much pressure do you exert on the earth's surface when standing up and when lying down, compared with that of the atmosphere? How might have Torricelli's family name helped him to solve the water pump problem?

BF: You once compiled a Chem Zen reading list, which I found very useful and still find very useful. Would you comment on that?

DH: The Chem Zen reading list was entirely optional, unrelated to the official subject matter of the course, but rather focused on historical and cultural perspectives. It was intended to provide a "liberal science" smorgasbord. Actually, the list emerged in response to a request from students. The first year, a small group delighted me by asking for suggestions for reading to do when they had become alumni! After that, at the end of each season, as a benedictory gesture, I'd hand out the list. In 2002, the last time I taught Chem Zen, the list had 17 items. Here I provide an expanded menu more compactly, by listing just first authors (with abbreviated titles where needed), since specifics can now be readily found on the Web.

Autobiographies: Luis Alvarez; Francis Crick; Carl Djerassi; Francois Jacob; Eric Kandel; Arthur Kornberg; Rita Levi Montalcini; Cecilia Payne-Gaposchkin; Rudolf Peierls; Emilio Segre; Charles Townes; Stanislaw Ulam; Victor Weisskopf; Edward Wilson.

Biographies, Subject (Author): John Bardeen (Lillian Hoddeson); Marie Curie (Eve Curie; Susan Quinn); Albert Einstein (Walter Isaacson); Benjamin Franklin (Walter Isaacson); Rosalind Franklin (Brenda Maddox); Lord Kelvin (David Lindley); Dmitrii Mendeleev (Michael Gordin); Walther Nernst (Diana Barkan); John von Neumann (Norman Macrae); Louis Pasteur (Patrice Debre); Linus Pauling (Thomas Hager); Michael Polanyi (Mary Jo Nye); Ernest Rutherford (John Campbell).

History, Authors (Title): I. B. Cohen (Science & Founding Fathers); Horace Judson (8th Day of Creation); Richard Rhodes (Atomic Bomb); Jennet Conant (Tuxedo Park; 109 East Palace); Sam Kean (Disappearing Spoon); Michael Riordan (Crystal Fire); Robert Scully (Demon & Quantum).

Scientific Culture, Authors (Title): Jeremy Bernstein (Experiencing Science; Life It Brings);

Jacob Bronowski (Science & Human Values); Gerald Holton (Thematic Origins); Peter Medawar (Advice to a Young Scientist; Limits of Science); Philip & Phylis Morrison (Ring of Truth); Lisa Randall (Heaven's Door); Lewis Thomas (Youngest Science).

Other Topics, Authors (Title): Betty Edwards (Drawing on Right Side of Brain); James Gleick (Chaos); S. Hayakawa (Language in Thought & Action); Steven Levy (Artificial Life); Eli Maor (e: Story of a Number); David Mumford (Indra's Pearls); Oliver Sacks (Musicophilia, Mind's Eye); Tina Seeling (Epicurean Laboratory); Neil Shubin (Your Inner Fish); Leo Szilard (Voice of the Dolphins); Robert Williams Wood (How to Tell the Birds from the Flowers).

Peer review, science of elections, Zipf's law in research, Homo Computus, 'Culture Wars'

JR: You talked about, in a very gentle way, the funding issue and the difficulties that you have encountered since your Nobel Prize. What you didn't say, or even imply, which you may not believe, but there are people who are concerned with the peer review process.

DH: Oh, yes. I'm among them, yes.

JR: Are you concerned that in an era when the number of resources is limited that people will review things in their own self-interest rather than in the interest of science. Do you have a sense of this at all?

DH: Well, yes. It seems to me that even if reviewers are perfectly well meaning, it's almost unavoidable. Now NSF, which is the prime funder for chemical physics, requires five or more reviews for a grant proposal. If a proposal, particularly from a young scientist, asks support for adventurous work that's really novel, not being done by anybody else, it's very unlikely there will be five reviewers who really can appreciate it and rank it highly. But if the proposal is for mainstream work, worthwhile but not novel, related to that the reviewers themselves are doing, the likelihood of a favorable assessment is higher. The reviewers hope to continue getting support for similar work themselves. So there's a built-in bias. Sometimes, I've joked, but it's serious, that NSF should have a special pot of funding for which a proposal is not eligible unless three of the five reviewers say, 'It won't work.' We should invest some amount to encourage such work. Under the conditions prevalent today, I don't think that molecular beam work would have received NSF support back when beams got started in chemistry.

What happened was NSF then had a fellow, Bill Cramer, who had actually done some ion beam work. He became the research monitor at NSF for molecular beams. Back then there were only one or two reviewers per proposal, and also there were many fewer proposals. When you sent a proposal to NSF, if approved, the money would actually arrive only four months after submission. Now it's a long time before they even send it out, even by fast lane. There are a lot of hoops to go through before even the reviewers are selected. With five reviewers, it winds up that you rarely get any funding earlier than a year. The upshot is you've got to have enough funding to keep your lab going, with some margin to borrow from to explore more adventurous ideas that turn up.

JR: Are you a member of the Science Board?

DH: No, I've not been.

JR: Have you ever been?

DH: No.

JR: But you surely have influence.

DH: I've talked with people who are members.

JR: And you've expressed these kinds of concerns?

DH: Oh, yes. But I haven't devoted myself to campaigning. Actually, the past couple of years I've campaigned about something else. It too is likely tilting at windmills. I'm much concerned about elections, especially our presidential elections. Years ago I read an article in *Scientific American* that acquainted me somewhat with the theory of elections. The 2000 election was so dismaying that I wrote to the *New York Times*. They didn't publish my letter, but as a result I got acquainted with a political scientist, Stephen Brams, at New York University. Colleagues in political science here told me, 'He's the expert on election theory.' He's written several books and many articles about it. Shortly before the 2000 election, Brams had sent a letter to the *New York Times* predicting what would likely happen, but his letter also was not published. Some months after, we managed to publish an editorial in *Science* about election theory.

Here's a quick sketch. It resembles the three-body problem in physics. Our election system, plurality voting, allows us to vote for only one candidate, no matter how many candidates there are. If there are more than two, plurality voting is about the worst possible way to do it, if we want the winning candidate to be at least acceptable to a majority of the voters. No doubt you recall some three-way races in which a candidate, that polls showed was the least appealing to the electorate, won a plurality because the other two candidates each got less than a third of the votes. By 'least appealing' I mean that candidate would lose, often by a wide margin, in a two-way race against either of the other candidates. Nowadays the early primaries are very important in presidential campaigns. There the plurality problem is of course worse still. For instance, there may be five or more candidates. Then the least appealing candidate (in the sense I just specified) can win with an even smaller plurality. There is a built-in bias in favor of a candidate at either the left or right end of the political spectrum. Such a candidate will collect all the votes in that part of the spectrum, whereas the rest are spread among the several other candidates. Our plurality system forces any third party to be a spoiler. In 2000, we had a spectacular instance in Florida, where [Ralph] Nader got 94,000 votes and Bush and Gore differed by only 530, so the election was decided by a minor third party, to the benefit of the major party candidate less congenial with aims of the minor party. That happened in 1992 also, when Ross Perot's candidacy decided the election in favor of Bill Clinton. Beyond all the other complications of politics, our election system is capricious. I'm convinced that the plurality system would enable someone like Hitler to be elected more easily here than he was in Germany in the 1930s, if we were suffering a similarly dire economic situation.

Despite the efforts of Brams and others, it's very hard to get people to even think about our election system. I've tried to get NOVA to do a program. There's much fascinating material. Many systems other than simple plurality have been tried in practice and analyzed theoretically. Lewis Carroll wrote a lot about election theory, after becoming upset about how Oxford Fellows and British MPs were chosen. Over 50 years ago Kenneth Arrow listed criteria that seemed sensible for elections in a democratic society to fulfill, then demonstrated no system would fulfill all of them. But some systems are much worse than others. Simple plurality is about the worst possible. Brams has made a strong case that, from practical as well as theoretical viewpoints, the best way to deal with it is so-called approval voting. It simply permits voters to endorse as many candidates as they would approve of holding the office.

Because of a misleading ballot format, in 2000 approval voting occurred inadvertently in Palm Beach County. There all the ballots marked for two candidates were thrown out. My letter to the *New York Times* was to point out the irony. Actually, it would not take a Constitutional Amendment to implement approval voting. The Constitution does not prescribe the plurality system or any system, just leaves that up to the states. What really should be done is to get some state to adopt approval voting for primaries, on a trial basis. Then people even in other states would become acquainted with it. Both major parties would benefit if approval voting were used. They'd be protected from derailing by minor parties, either the right or left, that can change the whole outcome, as happened in 1992 and 2000. A third party with appreciable support would no longer be a spoiler. The approval votes of its adherents could decide the election in favor of which major party candidate appealed most to the minor party's concerns. It's sad that our nation, which historically has been a mighty force for democracy, actually uses about the worst possible election system.

JR: Do you think you're ever going to be able to give up DNA and chemistry and devote more time to something like this? Public policy, science policy?

DH: Well, I'm probably not temperamentally suited to such a role. Maybe I will try it if I become aware that my colleagues find me so tiresome they don't want me around at all. But right now I just can't give up doing science.

JR: Okay. You've worked with both chemists and physicists. You've worked with both sides. How do you compare them?

DH: Oh, I like them both.

JR: I know that. But they're different.

DH: I'm intrigued by the cultural difference. I like to mention the wonderful synthesis that Kishi did, constructing the palytoxin molecule which required getting correct 72 two-fold structural choices. Kishi once remarked to me that he never was comfortable trying to solve a quadratic equation. Often I point out this sort of thing to students. When they arrive at Harvard, many presume that to be a scientist you've got to be good at any kind of science or math. But you don't actually. I say it's like being a musician. You don't need to play well all the instruments in the orchestra. Once I heard Yo-Yo Ma say, in public, 'I can't carry a tune. I can't sing. Fortunately, I can play the cello!'

The comparison with musicians is also useful in talking to students or the general public about another aspect of science that is generally misconstrued. Even if blessed with talent, musicians have to work hard to master their instrument, the literature and culture and other things required to perform well. Science is like that but much less hard in a major respect: the scientist can and likely will play most of the notes wrong, even off key, then finally get one right and be appropriately applauded. That's a huge advantage that science has over most human enterprises. Their academic experience conditions students to think that science is hard. But from a wider perspective, actually doing science is congenial and rewarding. What you want to find out, call it truth or understanding, waits patiently for you; it doesn't change. That's the huge advantage. In business, sports, war, politics, you may make a seemingly smart move, but a little early or a little late or conditions change so it turns out to be a fiasco instead of a triumph. That explains why more or less ordinary human intelligence can accomplish so much in science. You've probably heard the response Fermi gave when someone asked whether he thought his fellow Nobel Laureates in physics had anything in common. Fermi thought a while and said, 'No, I can't think of anything they had in common. Not even intelligence.'

I heard a fine talk by Charlie Townes about scientific creativity. He pointed out that there are scientists and other people who are far more productive than average, although they may differ only a little in their IQs. He discussed that in terms of Zipf's Law. That's an empirical relation discovered in the 1930s by George Zipf, a professor of linguistics at Harvard. He found that if you ranked the different words in a given text by how often they were used, their frequency was approximately inversely proportional to the rank. Thus, the relative frequencies of the highest-ranking English words (the, of, and, to...) are approximately 1,1/2, 1/3, 1/4, ..., respectively. Actually, I came across a paper written in 1927 by Ed Condon that evidently anticipated Zipf's analysis of word frequencies. Zipf went on to find similar correlations for many languages and for much other data, ranging from sizes of populations and economic activities to the length of speeches in plays. A similar phenomenon widely observed in physics is '1/f noise', also known as 'pink noise', wherein the noise power is inversely proportional to the frequency. Complexity theory indicates that pink noise or Zipf's Law behavior typically arises for events or processes that require the contribution of many independent variables. If these many variables are each distributed as a normal bell curve, and you want the distribution of the sum of all the variables, you'll wind up getting Zipf's Law. You can show that by rolling dice. A heuristic view, natural to a chemist, considers the daunting task of optimizing the yield of a multistep chemical synthesis. To achieve that, you have to get a very good yield in each step, of course hard to do. It's much more probable that your yield in one step is far from optimum, and then still more probable that your yield is poor in two steps, etc. That's how the inverse correlation of Zipf's Law arises.

Townes pointed to Zipf's Law as an intrinsic aspect of scientific productivity. It applies because many factors must be favorable to get exceptional performance. Accordingly, high-ranking achievements rarely emerge unless sufficient support is forthcoming for the inevitably far more numerous efforts that yield lesser results. This perspective is important for issues of research funding. As Townes emphasized, among the factors required to be favorable for strong performance is the acceptance of long-range prospects, diversity in approaches and institutions, tolerance of failures, and encouragement of trial and error because it is not possible to plan what scientific research is going to be successful. These are large-scale implications of Zipf's Law. It should also sharpen awareness that fostering careers depends on a lot of different things. It depends not just on your intellectual acuity, it depends on your education, it depends on the temperature of your intellectual environment, it depends on your personality and how you interact with people. All kinds of things come into it.

JR: Stamina.

DH: Stamina, yes, and health. I remember Bright Wilson remarking that very productive people were also unusually, exceptionally strong physically. Also, Henry Eyring until well past 60 used to run foot races with his graduate students.

JR: How do you think theoretical chemistry and physics have evolved over your active career?

DH: Well, theoretical chemistry and chemical physics have become far more physics like. Of course a great deal of it is due to the power of computers. We have a young theoretical chemist here who has 40 computers hooked up and running parallel calculations.

All his students are calculating diligently. I joke that we may be evolving a new species, *Homo Computus,* because so many people now spend a very large fraction of their waking hours hooked up with their computer. We all do that much more than ever before. But the tools now available are powerful. *Mathematica* probably has at least the equivalent of 10 or 15 years of intense study of advanced mathematics built into it. Anybody who learns the lingo, the way Wolfram set it up, have access to all that.

Often I point out to students that it's natural for the younger generation to look at the older generation and say, 'Oh, those guys were so lucky. They just walked through the orchard' shook the trees lightly and the fruit fell in their laps.' And I say, 'Well, you should recognize two things. One, those pioneers also had the privilege of making lots of blunders which are so easy to make when no path is clearly marked yet. But more important, you benefit from a legacy. There are all kinds of instruments and concepts and theory that were not available to your predecessors.' Again, it's like the architect aspect that I referred to earlier. New building materials and building methods enable you to do completely new things. Frank Gehry emphasized that he couldn't have built the famous Guggenheim Museum in Bilbao and many of his other structures if it weren't for the computer. All the structural elements in the Bilbao Museum building have different dimensions, and these are calculated to a fraction of a millimeter then cut and fit together perfectly. An architect couldn't even imagine doing that before.

The impact of computers in science is immense. Here's a simple example, familiar to any chemist of my vintage. When I worked with Harold Johnston, few physical chemists really understood what a normal mode of vibration was. Obviously Bright Wilson and many other spectroscopists did, but it was not part of the common background that all physical chemists had. Now, it is, and has been for quite a while. Anybody can plug in some standard programs, and calculate electronic structure, force constants, and vibrational frequencies. At times, you have to wonder whether these young people may have bypassed solving the most elementary problems, so won't know as much as you would like them to about what they're actually calculating or anything about its historical evolution.

JR: There's a question, though, that I'm waiting for you to reflect on. Two of your heroes, Bright Wilson and Rabi, for sure Rabi and I think Wilson, argued that theory should be closely connected with experiment. It should be driven by experiment to some extent. In that sense, theory is moving away. It's taking on a life of it's own. Do you not see this?

DH: Oh, definitely.

JR: What do you think of that? You're a theorist now. And you're an experimentalist. Your style would be that you want your ideas connected with the laboratory.

DH: Most of the theory I've done has been of that kind. It's naturally prompted by experimental questions. On the other hand, the dimensional scaling is certainly not. But I don't find it alarming that theory is developing on its own, because of course my early immersion in mathematics made me appreciate how beautiful it is as a pure intellectual adventure. Moreover, it's uncanny that so often scientists find phenomena for which appropriate mathematics is ready and waiting. So some theory that seems disembodied now may be redeemed that way. Some won't. I don't see that as doing great harm. We have lots of scientists now eager to do theory. It's probably good to let them explore all kinds of things. Perhaps if theorists were in short supply, we'd need to nudge more to do work that aids design and interpretation of experiments. I think it's best to arrange things so experimentalists and theorists mingle, so they learn to communicate and share perspectives. Then collaborations will naturally emerge.

JR: But if you take multiple universes where in principle there's never going to be a way to check the validity of that idea in an empirical way, and yet if you accept that, then you can say, 'We understand why the constants are so finely tuned in this universe as to allow life. Because there's lots of other Universes where there are all different kinds of constants.' So we can explain that. But should we call that something other than physics or cosmology?

DH: It might border on theology! Then it is a question. Maybe we should go back to natural philosophy as in the Enlightenment. Again, however, as far as I'm concerned, I'm glad some people chose to explore questions like that. I've not prepared myself to feel comfortable with them. Yet I've met some very bright cosmologists who emphasize that modern astrophysics may be able to test some of their far-out ideas. Among them is Andrei Linde at Stanford. It was at a symposium held there a few years ago with the modest title 'Cosmologies and World Views.' Steve Chu invited me, perhaps to have a specimen chemist. In addition to three cosmologists, and Steve as an atomic physicist, there were a couple of literary scholars and other humanists, including a Jesuit theologian from Loyola. What he said was striking. He didn't phrase it quite this way, but at dinner I said to him: 'It sounded to me like you were saying that God didn't create man, it's the other way around. And that theology is now regarded as a branch of anthropology. Is that what you said?' He replied, 'Yeah, that's pretty much what I said.' Seems that Jesuit theology is now much more down to earth than scientists' cosmology!

As the announced aim of the symposium was to bring together scientists and humanists, I gave a talk called 'Sacred and Profane Love', and started with the famous painting by Titian. It has two female figures, one very opulently clothed, the other attired in a simple gauze-like gown and holding up a lamp or grail. These ladies were looking off in opposite directions. First I asked the audience how many were scientists, then how many humanists; it was 50/50. Then I asked the humanists to indicate which figure in the painting they thought represented the Humanities. Then asked the scientists which represented Science. I was very surprised: it was about 50/50 each. I had expected most humanists would say something like, 'We pursue knowledge for its own sake and hold high the flame of learning, while the opulent and haughty scientists ignore us.' And I had expected the scientists might say, 'We tend the flame of reason and humbly seek to understand the Cosmos, while the arrogant humanists consider us to be clods.' After expressing surprise and pleasure at the outcome of the votes, I told a story about encountering cultural disrespect as an undergrad at Stanford. I took a course in scientific writing. The teacher, a grad student in English, would come in and just look out the window for several minutes. Then he'd turn to the class and say unkind things about how hard it was to try to teach such uncultured slobs how to write. One day he pointed to a fellow sitting next to me and said, 'I feel sorry for you. You're probably going to spend your life improving adhesive tape.' The symposium was held in a building donated by a Silicon Valley company. So I said, 'Actually, that fellow might have gone on to improve magnetic tape, and this building might be a result.'

Later I took part in a 'culture wars' encounter held at the New York Academy of Sciences. It was called 'The Flight from Science and Reason.' I accepted the invitation because I knew Bright Wilson would have done so; he was much concerned about that. For my talk, I used the title 'Imaginary Gardens with Real Toads', a line from Marianne Moore's poem. She was talking about poetry, but I thought that also described science. We construct imaginary gardens, and find there are real toads there too. I wanted to be conciliatory. A chief point was: even if people say silly things, it's a good thing that they're visiting each others' gardens. We should realize that the next generation is also going to look at our gardens and toads. They'll likely laugh, but weed out or nurture what we have planted, as they see fit. So we shouldn't get uptight about it.

Relationship between teaching and research

JR: Okay. I want to just talk a few minutes about teaching and research. Because you've been in between teaching and research. You called yourself earlier this afternoon a public servant.

DH: Yes. I think that's what teaching and mentoring is.

JR: That attitude is exhibited in your devotion to teaching. But I want to push on you a bit. The American Association of Physics Teachers gives an Ørsted Medal to recognize excellence in teaching. The medalist gives a response. And in many of these, particularly the older ones back in the early '30s, a theme runs through their responses, and that is that there was always a tension between teaching and research. That's when their research is going well, their teaching suffered. When their teaching caught their imagination, they didn't spend time in the lab. So there's this real tension. Are you aware of this? I mean, would you acknowledge that there's a tension between your teaching function and your lab function?

DH: I don't think I have that kind of tension, because I think for me teaching has helped my research enormously, because I get excited when I talk with students. If I see the student gets interested and excited, I get more excited. I've gone away on sabbaticals a few times, and my wife always points out how I spend almost all my time writing letters to my students. It's the interaction with the students, graduate students or undergraduates, it's about the same, that seems so important for me. I'm not sure I could even be a scientist otherwise.

There may well have been more validity to the tension back in the '20s and '30s, when faculty were doing experimental work with their own hands. For his oil-drop experiment, Millikan personally made 1000 batteries. Nowadays, even so-called experimental scientists don't get to do experiments very long themselves. They are too busy writing research proposals and papers. Many are almost executives. So the teaching role may become a major way the faculty mentor interacts with the students actually doing the hands-on research.

At least in this mentoring mode, the distinction between teaching and research gets fuzzy. Usually, the mentor contributes important ideas to the research. Yet the teaching component may be more important. Grad students and postdocs do much of the nitty-gritty work on their own. Teaching and mentoring them involves much more than technical matters. It involves, as Bright Wilson exemplified, conveying an understanding of the culture of the field and what is ethically right, how to write papers and give talks. Much of the purely technical things can nowadays be learned from the web. But the personal interactions in a research group contribute greatly to the making of a scientist.

Teaching in regular classroom courses also doesn't seem to me to conflict with research. It takes time of course, but there's compensation for that in revisiting and refreshing your appreciation of basic concepts and discoveries, things you fell in love with when you were a student. I get charged up by teaching. It fosters my enthusiasm for doing research. Progress in research does not go linearly with time, but in fits and spurts. So time devoted to teaching should not be considered as simply subtracting from research. Instead, it contributes positively by stimulating excitement as well as ideas that can accelerate progress in research. Even during my stint as department chairman, I always taught the regular 'load.' I found it was not a 'load' but a 'buoy.' Maintaining contact with students and cherished topics helped a lot to dispel frustrations with burdensome administrative chores.

JR: Let me put it another way. I would argue with you that if I walk up and down these halls in this building and across at Lyman, that the research physicists, the research chemists, are hoping that their research attracts attention.

DH: Yes.

JR: That they become recognized. That they become honored. That they become a prize winner of one sort or another.

DH: Yes, that's a natural thing.

JR: It's a natural thing. So that fundamentally I would suggest that research is nurturing self, whereas in teaching, you are nurturing others. Those are very different human activities; it's a different kind of mindset.

DH: Yes, they are. But if you value the feeling that by nurturing others you are doing something you just deep down feel is very worthwhile, you are also nurturing your self-esteem. If you personally feel grateful that it transformed your life, all the nurturing that went into you, then you feel awfully good about doing your bit for others. It can be more important than anything you could manage to do in research. I just don't see a huge difference. When I'm doing research I'm trying to teach myself and a few comrades something new. Whereas in classes I'm trying to teach things old to me but new to the students. In doing so, I often come to see the old things in new ways. Sometimes that's just as exciting as getting a new insight from research. Also, both the exploratory attitude of research and things learned from it enhance teaching. Even the general chemistry courses I've taught to freshmen have been informed and enlivened by insights from current research, my own and that of others. It makes a difference if teachers of elementary courses are involved in research because they gain perspective on what is important, and how the basic ideas are key in frontier research. Graduate courses help get students ready for research, not so much because of the advanced material per se but because they reinforce and deepen command of the basics. Overmastering the fundamentals empowers students to think in fresh ways. Again, a musical analogy: you really have to play the scales extremely well before you're ready to work up to concertos.

There's a simple policy I've advocated that would help combat the notion that in a research university teaching doesn't matter. Many seminars are held, most given by professors. But in introducing a speaker usually only the academic pedigree, awards, and research are mentioned. It should be customary in introductions at seminars or scientific meetings to always mention teaching done by the speakers. Either hosts, session chairs, or speakers can make it happen.

I've long thought it odd that people tend to think of teaching as something that only goes on in schools. Actually, in the 'real world' everybody does a lot of teaching, much of it inadvertent. Ironically, in a university you can get away with doing a crummy job of teaching. In industry you can't. There you have to teach both your subordinates and your supervisors and those who can do it well are highly valued.

However, at a university when faculty much admired for their research are also devoted teachers, their students and colleagues want to emulate them. I remember Frank Westheimer telling about the big surge in teaching efforts in chemistry when he was at Chicago and Fermi arrived on the faculty and began teaching introductory physics. Quite a few chemists outstanding in research have taught freshman chemistry, among them Linus Pauling and Harry Grey at Caltech; Roald Hoffmann at Cornell; Dick Zare at Stanford; Bruce Mahan, George Pimentel, and Alex Pines at Berkeley. At Harvard, as you know, Ed Purcell insisted on teaching undergraduates. He had many auditors, including some faculty colleagues and a few grad students (me among them). His classes were an absolute joy. His love of physics, his deep understanding and his way of thinking were exhilarating. It made you eager to try to teach like that, although you didn't expect you could nearly as well.

I've known faculty who felt the less teaching they did the better. But I don't think that helped their research. As I said earlier, I've observed situations where the quality of research fell short because of thinking just on a narrowly technical level. If you teach a basic course, it keeps you going back to the basics and focused on big questions. Then you're more tuned to recognize what really matters in a research problem. Of course, also having the opportunity to observe great teachers like Purcell has that effect too.

Debye was another fine example. As I described yesterday, his course was inspiring. He clearly loved teaching and enjoyed his artistry in doing it. I can see him now, with a twinkle in his eye and impish smile.

BF: Could you describe some examples of how your teaching has inspired your research?

DH: Two come to mind. One of course is the dimensional scaling escapade. I was teaching a favorite course, graduate level quantum mechanics. I always liked to show students different mathematical ways to treat the prototype problems, such as the hydrogen atom. That helps them to appreciate how such different ways bring out different aspects and different interpretations of the physics. I came across a tutorial article in *Physics Today* by Ed Witten. It was about quarks, gluons, and quantum chromodynamics. I probably would only have glanced at it, but a subsection labeled 'The Hydrogen Atom' caught my eye. There Witten explained that quantum chromodynamics – which treats the strong nuclear force – is difficult to handle because all the physical variables scale out, just as happens with the hydrogen atom. Of course, that doesn't matter for the H atom, as we can solve it exactly. In quantum chromodynamics (QCD) there's no such luck. So in that case, the approach was to treat as a variable something that is ordinarily considered a fixed parameter, in order to apply perturbation theory. For QCD, the parameter used was the quark color. To illustrate the method, Witten showed how to do the H atom and He atom by taking the dimension of space as the perturbation variable. You go to the infinite dimensional limit, then develop a perturbation expansion in powers of $1/D$. Witten evaluated the $D \rightarrow \infty$ limit and got results that differed from the $D = 3$ values by about 80% and 40 %, respectively. Witten commented that this method is only useful for qualitative estimates. But I thought my students would find such a novel method interesting. When I sat down to work it up as a homework problem, I did it a bit differently. It was very easy to pick a scaling that got the H atom exactly right, so I used that to do the He atom. To evaluate the $D \rightarrow \infty$ limit only required solving a quadratic equation. I knew the He atom at the $D = 1$ limit had been published by Carey Rosenthal, one of Bright Wilson's students. So I interpolated linearly in $1/D$ between the limits. Lo and behold, that gave the ground state energy for He at $D = 3$ with an accuracy between 10^5 and 10^6.

BF: What a plum

DH: Actually, it turned out to be a box of plums. Maybe comparable to the boxes of prunes I used to pick as a kid. Since Witten's paper touched it off three decades ago, there's been a lot of further work on electronic structure using D-scaling, some with intriguing results. It provides a perspective very different than conventional wave mechanics. In the D-scaled space, the electrons take fixed positions; we refer to that as the G.N. Lewis structure, in homage to his prequantum model still used in high school chemistry. I'll mention a nice aspect. It's easy to evaluate the $D \rightarrow \infty$ limit even for many-particle systems. That limit might seem ridiculously far away, but properties of interest usually go like $1/D$. So the infinite dimensional limit should be regarded as the origin, with the real world at one-third. Results calculated at the large-D limit generally provide fairly good first approximations to those for $D = 3$. In contrast, the pedagogical favorite, $D = 1$, is less easy to calculate and a far less good approximation, especially since it can't include any angular momentum contribution.

Other plums came from teaching the same course and also sprang from the H atom. An instructive problem we worked out in some detail has H in a spherical box. As the size of the box shrinks, the various eigenstates are pushed up in energy. Eventually, the box walls interact more strongly with the electron than the nucleus does. When we realized that nobody had treated the diatomic hydrogen molecule in a box that became a research project. It led us to appreciate how subjecting molecules to high enough pressure in effect serves as a universal catalyst. When squeezed enough, the overlap of electron clouds creates strong repulsions that weaken bonds and lower activation energies. Eventually, we even worked on models for interaction of H_2 molecules in bulk, to examine aspects related to the famous prediction in 1948 by Eugene Wigner that sufficiently high pressure could turn hydrogen into a metal.

Also, we got involved with high pressure experiments using diamond anvil cells. In collaboration with Hubert King, we studied pressure-induced shilfts jn vibrational frequencies of solute molecules in solution. That allowed us to get interesting information about the solute-solvent interaction than could not be gotten otherwise. More recently, at higher pressures, I collaborated with Russ Hemley and others at the Carnegie Institution in Washington, DC. Russ is now director of the Geophysical lab there and has done much extraordinary high-pressure work. Another excursion into history led to my main contribution to that collaboration. From a biography of Mendeleev, I learned that his periodic table came about from his teaching. But also that he worked as a consultant to the fledgling Russian oil industry. His opinion, derived from geological evidence, was that oil is mainly created in the vast pressure cooker within the Earth, not formed as a fossil fuel. Ever since a Russian school of geologists has advocated that view. It was also taken up by Thomas Gold, a cosmologist, who marshaled the evidence in a book, *The Deep Hot Biosphere*. I simply suggested an obvious experiment, which showed it was easy to make methane in a diamond-anvil cell loaded with calcium carbonate, a bit of iron as catalyst, and water, at pressures and temperatures corresponding to depths of 30 miles or so in the Earth's mantle. Much more incisive work at the Geophysical lab and elsewhere has made heavier hydrocarbons in similar experiments. As yet, there is no way to know how important oil of pressure-cooker origin might be compared to that of fossil origin. The Geophysical lab now has a major 'Deep Carbon' project underway. Russ kindly (probably too kindly) claims that he got interested in pursuing high pressure as a result of the H atom in box problem he met when he took my quantum course as a grad student. If so, a lot of plums have come forth from that H atom in a box!

Profusion of publications

JR: Let me ask more about teaching and research, one more question. And I'm asking you this. I would be uncomfortable asking anyone else. Here's the issue. Rabi published about 50 papers. Purcell published about 50 papers. Feynman published about 50 papers. Dudley Herschbach has published 500 (Updated) or something.

DH: Yes. That's the total published from my research group. I'm not a coauthor on about 20% and roughly another 20% are nontechnical, popular, or historical articles.

JR: The reason I can ask you is that I know that you are absolutely devoted to all of your responsibilities. But it's now common for people to end up with 300, 400 papers. Something is out of balance. Not with you, because—

DH: Well, no, it's generally out of balance. I would rather have published only 50 research papers but written 500 directed to the general public, especially young people. Of course, the funding system compels publication. If you have a grant, now typically for three years, to get a renewal you have to have published results. If the grant is for $300,000 a year or so, if you haven't published at least two or three papers a year, there's no chance for a renewal. At the 1911 Solvay Conference, Sommerfeld made a remark I've often quoted. It pertained to Einstein's paper on specific heats, but applies here too. He said, 'Herr Einstein has shown us that degrees of freedom should be weighed, not counted.' In one sentence, Sommerfeld brought out the key difference between quantum statistical mechanics and classical statistical mechanics.

Research papers also should be weighed, not counted. Sometimes a one page paper is much more significant than dozens of the garden variety. But a funding agency and the peer review system often don't weigh significance reliably. So faculty feel pressure to turn out a respectable quantity. Also, grad students and postdocs have to have publications for job applications. If people were allowed to publish, let's say, only one paper a year, it would be quite different. Maybe a lot better.

JR: That's right. *Physical Review* would be readable again.

DH: Yes. But there's no likelihood of getting to that point. I've had about 60 graduate students and 50 postdocs total. After deducting nonresearch articles and reviews from the list, our average production of research papers was about 4 per capita, or roughly 1 per person per year. Not embarrassingly high. I have tried to emulate Bright by encouraging students to publish papers without the impediment of me as coauthor. But only 20% of the papers are in that category, in large part because on the rest I did most of the writing.

JR: Bright kept his name off a lot of papers.

DH: Yes, absolutely. Bright had 90 PhDs and 60 postdocs. His group published about 400 papers. About 240, that is 60%, were without him; of the rest, he was sole author on 80 and coauthor on another 80. I don't have a paper with Bright. I wish I did. I would give him a manuscript and he never changed a word. Of course, I had worked very hard to try to make it perfect to give to Bright. Well, he once pointed out a misspelled word. I begged him to put his name on a couple of papers. But he wouldn't do it

JR: And what would he say?

DH: He just said, 'Look, it's your idea. You did everything. I don't feel I contributed sufficiently.' I would say, 'But Bright, there's no way I would even have known where to start if it weren't for you and your lab.' In his book, *Introduction to Scientific Research,* he describes his view. Sometimes I have not put my name on a paper when most people, even Bright, would have because I felt the student needed a solo paper. I remember Norman Ramsey telling about his time as a grad student with Rabi. Back then Columbia had a rule that you had to publish a solo paper for your PhD thesis. The result was students would do something uninteresting but sure-fire to fulfill that requirement, while more challenging work usually involved collaboration. The presumed incidental paper assigned to be his solo turned out to be the discovery of the quadrupole moment of the deuteron!

Actually, I've partaken of both worlds. I was promoted to tenure at Berkeley after only two years; by then I had only a dozen papers or so. Most of them, eight or so, were from Bright's lab. I had a couple of good theoretical papers plus just one experimental paper from Berkeley, our first results on reactive scattering of potassium plus methyl iodide. I was surprised to be promoted so early. Even when I came back to Harvard after another two years, I had only about 25 papers. Now, assistant professor candidates typically have 20 to 30 papers, none have only five or six.

JR: Purcell has 50. That would not get him promoted to full Professor.

DH: Well, that isn't quite true. His NMR paper, for example, was only about number seven. I looked up his list. And I would hope that would get him tenure, but you can't be absolutely sure in today's world.

JR: But 50 papers would not do it today in most cases.

DH: Yes, that's right. But the papers Purcell wrote were such a joy; everything he wrote. I haven't read all 50, but I've probably read 20 Purcell papers.

JR: You like to write.

DH: I like to write. I'm not very good at it. I'm very slow. I am always trying to make it better. But I write too much because I feel guilty about all the things I haven't written. There's always something I've got to do right away, so other projects I want to do don't quite make it to the top.

Science & Religion

BF: The topic of science and religion interests many people. You mentioned at some point that you think of science and religion as siblings, both born of our innate sense of wonder, and it seems to me that one could make a sensible argument that religion has in fact been born out of fear, and that fear-mongering has indeed been religion's main preoccupation (at least in some of its pagan or in the Abrahamic varieties). Instilling obedience rather than wonder seems to be religion's main role, if not mission, and justifying the power of the powerful rather than empowering reason to be its rationale. So it seems to me that science and religion can be only very antagonistic siblings, like Abel and Cain.

DH: Well, there's a lot to what you just said, I would have to agree. When I said born, I really meant born as innocent infant siblings. I do think there is a deep yearning in human beings for something supernatural that can account both for the awesome cosmos and why we should be here.

BF: That we are not alone.

DH: As scientists we respect empirical observations, and that yearning is evident in every human society; all have religious traditions and myths. Yet, as you emphasized, the religious sibling has a very nasty aspect. It's long been exploited in service of political power. If you ask why the instinct of wonder has been exploitable, I think it's because the yearning for approval and protection by a God is intrinsic. There are people who fear science too. That seems evident already in the Book of Genesis episode we discussed earlier. In having God forbid the fruit of the tree of knowledge, the religious sibling wants to crush the yearning for knowledge by the science sibling (as we call it for brevity; the urge for knowledge is much broader than science). So maybe antagonistic rivalry between the siblings goes all the way back, destined from birth. I guess I'm starry-eyed by nature, and feel that it should be possible to transcend this antagonism, acknowledging shared wonder and awe. Religion certainly has fostered high ideals and compassionate service but also vicious intolerance. It may have made some people behave better than they otherwise would, but sadly the total summing up looks pretty negative.

BF: Made them also behave worse…

DH: I'm surprised how often I'm asked, especially in recent years, about conflict between science and religion. In response, I usually mention the notion that both might be born out of innate wonder. But mostly I try to bring out a basic point that is rarely mentioned. I think it is really worth emphasizing, especially to people who are in favor of teaching creationism or intelligent design as counterpoint to evolution. Rather than bristling at that, I consider it an opportunity to contribute a little to public understanding. I try to defuse what seems to me to be a needless contention.

In my view, the real issue has nothing to do with evolution per se or even with religion. The key reason scientists oppose such a proposal is simple, indeed utterly mundane. In science, we can ask questions of Nature but must supply our own interpretations of her responses. That typically requires much discussion to assess evidence, often uncertain and most always incomplete. Invoking a supernatural explanation is not allowed simply because it's just not useful. It would stop discussion cold, with no way to go further.

So the real issue does not involve a genuine conflict between science and religion. Both involve much that we don't understand. But history shows that it is unduly pessimistic to presume that limitations of current scientific understanding will not be overcome, and therefore conclude that resort must be made to an inscrutable supernatural cause. For instance, lightning was considered supernatural until 1751, when Franklin showed otherwise.

Sometimes I mention two pertinent stories. One I heard from I.B. Cohen, a distinguished historian of science at Harvard. It's about a visiting minister who had never been to New Enland before, and was invited by the minister at Harvard. They went up to Vermont, where there are many picture-postcard, beautiful farms. The visiting minister especially admired one particularly lovely farm. It happened that the farmer came by, presumably behind his mule and plough. The visitor exclaimed to him, 'Oh, what a beautiful farm! It's marvelous to see what you and the Lord have accomplished here.' The farmer, after the traditional pause, spoke slowly, as a Yankee would, 'Yes, this is a beautiful farm. But you should have seen it when the Lord took care of it by himself.' To that I add, I consider the work of scientists to be much like that of the farmer.

The other story is another about Ben Franklin. On his deathbed, he got a letter from Ezra Stiles, then president of Yale. Stiles was a minister, as were all the presidents of the few colleges in colonial America back in 1790. Stiles asked several questions about Franklin's religious beliefs. The most interesting question was the last one: did Franklin believe in the divinity of Jesus? Franklin responded, 'Well, I haven't made a serious study of the question. I don't see now any reason to undertake it, because I expect soon to have a chance to check up on it directly.' It's typical Franklin, wrapping a serious point in a whimsical remark. As a corollary: For someone who believes in an afterlife, doesn't this mean you'll have the chance to check up on things then? And also, that during your brief sojourn in this terrestrial life, there's no need to be concerned by presumed conflicts between science and religion?

Science & Society

JR: Now, let me ask you about one more area, and that now has to do with you're sitting here, 2003, and you started your science in 1954, 1955, 1956, somewhere in there. As you look over this period of American history, of the history of science, its impact and so on and so forth, how would you characterize your life in science and the changes that you've seen? Do you have cause for concern? Do you think everything is great?

DH: Well, I have a lot of concerns. A major one is the paradoxical situation we've alluded to a couple of times. Science has hugely transformed civilization and is crucial for coping with big problems as well as creating big opportunities. Yet, understanding of science as a shared adventure of humanity and the ways of thinking that it should foster seems to be ebbing lower. One reason people are alienated is because so few can understand how their computers, automobiles, and much else actually work. Yet there's so much science material in bookstores and on the web now that's accessible to any literate person. It's strange that so many people seem to feel that science is not something they can possibly understand and don't want to try. A lot are downright antagonistic to science, such as those who want to reject evolution and/or climate change.

Of course, I'm starry-eyed. I think of science as a grand exploration of the world inhabited by our species, finding out things about it, ourselves and other creatures, and developing ways to find out more. It all becomes a common legacy for humanity. It also offers a mode of thinking that should transcend cultural, religious, and political differences. Twenty years ago, I wrote an essay urging this view of science, not from a starry perspective but that of co-inhabitants of our earth that preceded our species by many million years, the dolphins. My essay, titled *The Dolphin Oracle,* was prompted by an allegory published 50 years ago by Leo Szilard, another remarkable Hungarian. He founded the Council for a Livable World, devoted to efforts to restrain the nuclear arms race between the US and Soviet Union. I have served on the Council for about 15 years, since Ed Purcell recruited me for it. Here, I'll just quote from the last paragraph of my essay; I offer it as an earnest creed:

'Think of yourself as a dolphin oracle and ask about any issue of the day. Try problems involving differences in gender, race, religion, political persuasion, national identity, or the like; all recede when confronted by our common humanity. Let your mind try out also, now and then, other supercivilized traits of the dolphins, including exuberant leaps, whistles, and happy chortling. It can only do humankind good to become more aware that along with the dolphins and other incredible creatures, we really belong to a much wider universe of the mind; it could be called mindkind.'

You have probably been asked, as I have, ‘What about science should people know?’ The response I start with is a quote attributed to Richard Feynman. Although I’ve never been able to find the exact citation, it certainly sounds like him: ‘Science is not about what we know but about what we don’t know.’ This conveys what I regard as two of the most important things about science: it is an ongoing exploration and deals very much with uncertainty. We can expect, especially as new tools become available, to find out both new things and revised understanding of old things.

BF: I wonder whether we can talk about the academic community. Question number one: why American college professors tend to the left of the American political spectrum?

DH: Since we are used to thinking of relative motion of interacting particles, you could equally say, isn’t the larger mystery why so much of the American public is to the right of professors? It could be a reaction to professors, because they’ve suffered through examinations and all the rest. I don’t know. But we would hope that these people who professionally – it’s usually true – are thoughtful and probe deeply into things, are naturally more inclined, both for that intellectual reason and because they’ve had the privilege of working with young people, to have a broader view of society.

BF: They may also know other societies, not just the American one.

DH: That’s right, a broader view of all humanity. That should enhance wisdom and compassion. Many of the far right seem selfish, and narrowly focused. Again, Ben Franklin exemplifies genuine patriotism. He was truly interested in the wider society, and fostered many civic institutions. But he was interested in building what is now called social capital, not just acquiring personal wealth. He retired midway in his life, at age 42, and thereafter was occupied chiefly in public service. He set-up in business young people who had worked in his print shop, and made many philanthropic contributions. He was the only founding father to free his slaves and moreover in his will required his son-in-law to do likewise as a condition on his inheritance. His view on taxes deserves attention these days. In a 1783 letter to Robert Morris (de facto Treasurer of the not-quite born nation), Franklin wrote:

‘I see in some Resolutions of Town Meetings, Remonstrance against giving Congress a Power to take, as they call it, the People’s Money out of their Pockets, tho’ only to pay the Interest and Principal of Debts duly contracted .... Money, justly due from the People, is their Creditor’s Money, and no longer the Money of the People. All the Property that is necessary to a Man, for the Conservation of the Individual and the Propagation of the Species, is his natural Right, which none can justly deprive him of: But all Property superfluous to such purposes is the Property of the Publik, who, by their Laws, have created it, and who may therefore by other Laws dispose of it, whenever the Welfare of the Publik shall demand such Disposition. He that does not like civil Society on these Terms, let him retire and live among Savages. He can have no right to the benefits of Society, who will not pay his Club towards the Support of it.’

BF: What do you think college professors should do, to matter more on issues of research policy, (which is of course their self-interest), of education (that’s public service, really, 100 percent), environment, as well as on general political issues, such as discussion of unbridled capitalism versus a social democratic model, or methods of voting, or other such issues?

DH: Well, quite a few do speak and write about those issues. (I’ve done a bit on most of the issues you mention, should try more.) But academics seem drowned out by the louder and harsher voices of right-wing commentators, who rail about the so-called liberal press and liberal faculty. I don’t really fathom whether many academics are really intimidated by such stuff, or just disdain to contend with it, or feel they couldn’t accomplish much. Some like Noam Chomsky are certainly fearless. So are scientists who warn about climate change and global warming and face severe ridicule ....

BF: They get even death threats.

DH: All kinds of stuff. Again, since the eighteenth-century is a hobby of mine, the contrast with the Enlightenment is pitiful. Nowadays we hear many statements about what the founding fathers intended, but often those are bogus, quite contrary to history. As illustrated by Franklin’s view of taxes, most of the founders would now be classed as extreme left-wing radicals.

BF: A related question: It seems to me that many scientists or academics would be able to speak on key issues of public interest with little or no preparation, yet few seem to do so or can be heard, which is a somewhat different issue. Isn’t this in part a result of them being too busy writing research proposals? Given that it’s Congress that allocates funds to the NSF and the other federal funding agencies, don’t its members in effect kill their potential competition and critics by underfunding academia? An almost unavoidable thought.

DH: People like Pauling of course did not hesitate to speak out. Others, who didn't have the courage and confidence that Pauling had, are probably intimidated, because they're beholden to the government. I can well imagine they feel that if they drew attention by being politically active, it might affect research grants. You know, for a long time Senator [William] Proxmire awarded Golden Fleece awards to researchers whose project titles invited ridicule. Likewise 'curiosity-driven' research was routinely attacked. That sort of thing is again on the upswing. In the current political and economic situation, there is grave concern that funding for all levels of education and for research may suffer severe cuts.

Science education

BF: You have been fond of using the phrase 'taking ownership' in the intellectual sense. Could you expand on this a little bit, and say why 'taking ownership' in this sense appeals to mindkind so much?

DH: By 'taking ownership' I mean experiencing science as something like language. Once you reach at least a modest level of fluency, you belong to a wide society whose members own in common an empowering knowledge. Viewing science that way emphasizes that it is a shared adventure of our species. By speaking of 'mindkind' I wanted to call attention to intellectual capacities that actually extend beyond our species, specifically to Dolphins. At present, Dolphins appear to understand more of human language than we do of theirs. But clearly their minds like ours share an innate eagerness to learn.

We can be optimistic that the power of the Web will enormously enhance all sorts of education A grand goal for the 21st century should be achieving world-wide literacy, including in science. No longer does that seem impossible. Already there are remarkable harbingers, including the Kahn Academy websites, which draw many million viewers, and more and more universities are offering courses free on-line.

BF: Right. A question about high schools: are high schools as good as the teachers who teach there? How is it in this respect with colleges? Is there a difference?

DH: The quality of the teachers is very important. But also the methods used are very important. I've told you about the modest then-rural high school I went to, and my algebra class with Mr Drummond. Was he a disappointment as a teacher? No, despite his admitting he 'didn't know much' about algebra. As an army man, he was not afraid to encourage any students who thought they understood something better than he did to go right ahead and explain it. I think that helped all the students to 'take ownership.' Nowadays it's very fashionable to emphasize peer instruction. For years, Eric Mazur at Harvard has done excellent work developing that in his physics classes and demonstrating its efficacy. He poses conceptual problems. Students individually submit their choice of answer or guess via 'clickers.' Then in small groups argue for two or three minutes with others who had a different opinion. After another clicker vote, which usually shows movement toward a consensus, he gets students to volunteer to explain their reasoning. This active involvement, explaining things to others and considering wrong as well as right answers has been shown to be much better than listening to lectures about the concepts. In arguing with each other, the students are full participants in teaching and learning. Especially with web resources now available, a high school can be very good. A key requirement is that the students recognize that the teachers, whatever their level of expertise might be, are very earnestly concerned that the students really get a good grip on the subject, and that it's important for them to do so.

I don't think there's a fundamental difference between high school and college courses. Both should strive to avoid a ritualistic approach, done for the sake of exams. Even a teacher without a deep technical knowledge of science can convey a lot to the students that motivates them. The teacher should not pretend to be an infallible authority that knows everything, but rather get discussions going that encourage students to be active in figuring things out. In my freshman seminar: *Molecular Motors: Wizards of the Nanoworld,* lots of questions come up that I don't know the answer to. The students are the wizards; usually within thirty seconds they've found relevant references and websites. Then we discuss things back and forth, and all learn together. That kind of learning is a shared adventure, akin to research. The best of my high school courses, long ago and of course preweb, got the students fully involved. When I went to Stanford I discovered how good my high school education had been.

BF: The role of self-study increases when one switches from high school to college, and on.

DH: Yes, we hope so.

BF: OK, a question from applied psychology: does praise, deserved or undeserved, improve performance? What is your experience in this respect?

DH: Praise is a very powerful thing. Of course, it has to be used judiciously. If you're too lavish in your praise it loses value, like inflation of a currency. If you're very meager in your praise it can gain great value. Wolfgang Pauli was famously hypercritical. Long ago I heard that when Vicky Weisskopf, who had worked as assistant to Pauli, came to this country he had a sealed envelope of introduction in his pocket, as was the custom then. He presented it to Hans Bethe, who he was going to work with at Cornell. Bethe read it and then handed to Weisskopf, saying, 'How did you get such a good letter from Pauli?' The letter just said, 'I have nothing to say about this man.' From Pauli, that was the height of praise. If you're a Pauli-type character, you don't have to write any lengthy letters of recommendation. On the other extreme, Gilbert Newton Lewis, the famous chemist at Berkeley, supposedly used a rubber stamp: 'best man I ever had.' That I find hard to believe. But I mention these limiting cases to my students who go off into academic life. Writing letters of recommendation is part of being a good citizen in the scientific community. People need to have these, and you want to be careful and judicious in them.

It's very important to help young people, who often need to gain confidence. In going through college and graduate school students need to gain two things above all: competence in some area, and confidence. And they have to be commensurate. If you have more confidence than your competence justifies that's not good. If you have less it's not good either, because it will limit what you feel you can do, and you won't appreciate how much you can actually do. So professors should try to foster the right level of confidence. More often than not it's better to err in the direction of praising people, but only a little more than maybe would be objectively deserved. But it's a matter of personality. My PhD mentor, Bright Wilson, did not dispense a lot of praise, but a little smile or just a word or two meant a lot.

Somewhere I read an essay by Edwin Land, a very creative scientist, emphasizing that for undergraduates it was important that they should have the feeling that they're special in some way. That they will be able to do something in their career that was worthwhile and special. A mentor can help a student to get that feeling. That's important for grad students too. Often the transition from student to research scientist is difficult. Sometimes it's particularly difficult for students who as undergrads got excellent grades in all their courses. Such students are accustomed to doing well in exams and problem sets. But those are designed to provide just the input information needed, nothing more, nothing less, and in a context where the bright student usually quickly recognizes how to proceed to get the right answer. Frontier research is entirely different. Nobody knows the right answer, often even the right question or approach. It may not be obvious what input information is relevant. Or whether the project will actually pan out at all. So the neophyte researcher must get used to being confused about what to do and how to proceed. That can become quite uncomfortable, especially for a student who had been very comfortable with course work. Support from other students in the research group and astute mentoring can matter a lot in such cases. In my experience, the transition to research typically is easier for students with less than stellar course grades; in their undergrad years, they'd been accustomed to being confused. I like to think that I help build the confidence of my research students when they see that I often become confused.

JR: We've discussed some concerns about science education and literacy; do you see grounds for optimism?

DH: (Update: See remarks above about the global power of the Web.)

Although science education and literacy are overall far weaker than befits the 21st century, there really are strengths to build on. Among them are science fairs. In the U.S. these are increasingly a really significant mode of 'informal education.' Premier annual events for more than 50 years have been the Science Talent Search, long sponsored by Westinghouse, and the International Science and Engineering Fair. Both are now sponsored by Intel. Anyone who attends these events or serves as a judge will become a lot more optimistic about our future. The high school kids who enter are doing the real thing; on their own they take ownership of a project. In the course of developing it, and exhibiting it, often at a series of fairs, they arouse the interest of friends and family and lots of curious neighbors. Both the Talent Search and the International Fair are conducted by a small nonprofit outfit, Science Service. (Update: it is now the Society for Science and the Public.) It also publishes *Science News,* written for laymen, that provides an excellent survey of what's happening in all fields of science. For 30 years, Glenn Seaborg chaired the Board of Science Service, and a few years ago he recruited me as his successor. Science Service hopes to get Science News into every high school in the country, via the web, and to further enhance the Talent Search and Fair.

The International Fair (ISEF) is as yet much less well known than the Talent Search (STS). The ISEF is held every May in a different city. It has grown to more than 1400 kids from about 50 countries, although over 90% are still from the U.S. Those kids are all winners of hundreds of local, state, and regional fairs, in which more than a million other kids took part! The ISEF also involves about a thousand volunteer judges and hundreds of volunteers who help in running it. Hundreds of scholarships are given as prizes.

Considering all the friends, relatives, and teachers of the kids entering the preliminary fairs, all told there must be several million people with links to the STS or ISEF. The kids displaying and explaining their projects are fine ambassadors for science. I wish the major media would pay more attention, particularly to the ISEF. I'd like to see TV news programs include, just as regularly as the weather report, a one or two minute episode featuring a student presenting an engaging and instructive project. That would surely attract a devoted viewership, since so many other kids, parents, and teachers would want to tune in. A year's supply of such segments could be taped, with unusual efficiency, at the annual STS and ISEF events.

Coming back to concern about science education and literacy, I'd like to mention a notion for a college-level core course for non-scientists. It might be called Great Experiments, as an echo of Great Books. The students would get personal experience, without having a regular science course necessarily, by doing experiments with things they've all heard about and know are important. They would read about, write about, and discuss the cultural and historical impact and consequences of the Great Experiments, considered from humanistic rather than technical viewpoints. And they'd devote one or two afternoons to each of the experiments, with no concern about getting 'right' answers but rather getting 'up close and personal' experience. They would do something with DNA. They'd build a primitive computer. They'd do a primitive version of NMR or other kind of spectroscopy. They'd synthesize a chemical compound, perhaps Indigo, a dye hugely important in trade on the Silk Road for many centuries and now still produced in great quantity, mostly to dye blue jeans. For instance, an outline of a DNA experiment has been prepared by our younger daughter, Brenda, who has a PhD in molecular biology. The experiment involves extracting some DNA and holding it your hands. You'll work with a certain bacteria that in its native form is immune to UV light. But that immunity can be degraded chemically. Then you restore the immunity by splicing in a little piece of DNA that you can easily separate from something else. My fantasy is that the students would find that it's easy, it's fun, it puts them in contact with things they're curious about. And it becomes so popular every Harvard student insists on taking it! I'd like to try out such a course, even in my so-called retirement years.

JR: That would be terrific.

DH: This project is only a partial rough draft now; I need to find a young collaborator to carry it on.

There's so much you can do with the web. In my freshman seminar, I see every week how skillfully the students fish out information from the web. The other day, a student told me about a project for a biology course. The aim was to find where a certain sequence of DNA bases, constituting a particular gene, might occur in animals. He said that in only half an hour he found using the web three very different kinds of animals that had that gene. Until just a few years ago, you couldn't even think about doing such a project. Such powerful tools can surely revolutionize education. We need them. I don't think we can provide an adequate corps of science teachers for K-12 in the foreseeable future, or perhaps ever. However, I'm convinced that this gap can be significantly offset by empowering able students to a much greater extent than occurs today. This becomes practical via the web. I've told you about students teaching students algebra almost 60 years ago back in my rural high school.

JR: Yes.

DH: That's part of what convinced me it could be done. But now I think many teachers feel they have to be authority figures. So they wouldn't dare have the attitude of Mr Drummond: 'It's okay if I don't understand much about Algebra. I'll just make sure you kids are learning it.'

There's another serious intrinsic problem in teaching K-12, and even beyond. It severely impacts teaching science, especially to minority students. From an early age kids are conditioned to view their teachers as judges who grade them. It needn't be so. That was brought home to me when our older daughter Lisa had a year at Oxford, so experienced the famous tutorial system. Every week she had to deliver a ten-page paper to her tutor. He criticized it vigorously and thoroughly. That did not discourage Lisa, for two reasons. (1) The ideas were her own and her tutor clearly was helping her to sharpen them and her presentation skills. (2) The exams, which came at the end of the year were set by a faculty group that did not include her tutor. So the tutor was not a judge, but a coach, helping her to develop her capacities. In all of our large cities, about 50% of minority students drop out without finishing high school. This is attributed to a nasty syndrome: if you try but don't do well, it confirms the stereotype that you are inferior; not trying avoids that. In sports, those same kids will take strong criticism from a coach.

I'm glad that now many experiments trying out new approaches in K-12 education are going on and in prospect. For instance, Leon Lederman is a great advocate of physics first in high school; I think such a change is good to try. I wish I had whatever it takes to get a range of schools to try out the 'coach rather than judge' approach. It takes a big effort to do such things. Likewise to get an experiment going on reforming voting to avoid the dangers of the plurality system. I don't expect to be able to accomplish much on such things, but keep talking about them in hopes that someone will take up the torch and do far better than I can.