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University of California
Berkeley, California

John Prausnitz

John Prausnitz: Chemical Engineering at UC Berkeley, 1955–2020

University History

Interviews conducted by
Paul Burnett
in 2018 and 2019

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John M. Prausnitz

Abstract

John M. Prausnitz is Professor of the Graduate School in the Department of Chemical Engineering at the University of California, Berkeley. A graduate of Princeton University, he is best known for his pioneering work in the field of molecular thermodynamics: the application of thermodynamics to the behavior and interactions of compounds of solids, fluids, and gases. He began research on applications to the petroleum industry, but he has contributed to improvements in many manufacturing processes, including in the field of biotechnology and renewable energy. He has been on the faculty since 1955.

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Foreword

C. Judson King
Berkeley, California, 2020

John Prausnitz has for sixty-five years been a major intellectual figure in Berkeley chemical engineering, and indeed in chemical engineering worldwide. He is the originator and still the principal academic shepherd of the field of molecular thermodynamics, wherein fundamental properties of molecules and functional groups within molecules are used to predict macroscopic phase equilibria, which underlie the selection and design of large-scale separation processes such as distillation, extraction, and adsorption. He has expanded that work over the years to ever more complex systems, including as polymers, colloids, and biological substances. His contributions reflect the benefits of the association of chemical engineering with chemistry in UC Berkeley's College of Chemistry, an unusual organizational feature that drew him to Berkeley in the first place.

John is one of the most, if not the most, prolific chemical engineers ever, in terms of publications, graduates and impact of his accomplishments. In 2011, his PhD graduate John O'Connell reported for the Prausnitz Festschrift issue of the *Journal of Chemical and Engineering Data* that John had published over 760 articles in 134 journals with 421 different co-authors from sixty-seven different organizations in over twenty countries. His article with Denis Abrams on the UNIQUAC excess-free-energy model has, as of July 2020, been cited 4,640 times. He has on the wall of his Gilman Hall office an academic family tree from 1988, with relationships through PhD guidance, showing 305 academic grandchildren, 104 academic great-grandchildren, forty-two great-great grandchildren and three great-great-great grandchildren. What these figures must be now, with John and his academic progeny having been active over thirty more years, boggles the mind!

His accomplishments have been well recognized over the years, starting with the Colburn Award of the American Institute of Chemical Engineers (AIChE) in 1962, and going on through numerous other awards, named lectureships, and other recognitions from a variety of sources. He is an elected member of both the National Academy of Sciences and the National Academy of Engineering, and in 2003 he received the National Medal of Science. AIChE's annual Institute Lecture is now named for him.

John is a man of broad and classical interests. Throughout his Berkeley career, he has been known by students and faculty for being in his office on Saturday morning with the Metropolitan Opera broadcast on the radio. He is an accomplished writer with a strong interest in grammatical polish. He has been a consistent and cogent voice for full attention to the humanities and to breadth in the education of engineers and has inspired these interests in generations of students. In recent decades he developed what he has named the Leonardo Project. In that effort he and co-worker students prepare vignettes of the human and social aspects of the circumstances surrounding various past technological accomplishments. These are available to faculty members around the world who can use them to enrich their technical courses.

John has consistently been a wise voice in discussions of curriculum and departmental policy. His incisive and questioning mode of conversation keeps everyone focused upon the most important issues at hand and always stresses the inherent values of universities. Beyond all this, he has been a good and true friend, with whom I am honored to have had the privilege to work over what is now more than fifty-seven years.

Interview 1: October 29, 2018

01-00:00:12
Burnett: This is Paul Burnett, interviewing Dr. John Prausnitz, for the University History Series, and we're here in Gilman Hall at University of California, Berkeley, and this is our first session, and it is Monday, October 29, 2018. Welcome, Dr. Prausnitz.

01-00:00:31
Prausnitz: Glad to be here.

01-00:00:32
Burnett: I want to start at the beginning, as is customary in our oral histories. Can you tell me a little bit about where you were born and where you grew up, and maybe a little bit about your ancestry, if you can talk about your family a little bit?

01-00:00:52
Prausnitz: Yes, I was born in Berlin, Germany, 1928, and my ancestry goes back quite a ways in Germany. We have some records of family members going back to about the time of Napoleon. My mother and my father were both native Berliners. And by and large I had a happy early childhood until I was five. When I was five years old, my sister was four years older, and I was sent to a camp, a summer camp. And while we were there at the summer camp I got a bad bellyache, and they took me to a local doctor there, who was a quite elderly man, and it turned out was incompetent. And he says, "Oh, that's nothing to worry about. That'll pass." Well, it didn't pass, and I was quite miserable. And then, when we took the train back to Berlin, my father was at the train station, and he's a physician, and he took one look at me and he says, "You've got a ruptured appendix." So that was quite serious in those days when we didn't have antibiotics, so I was immediately rushed to the hospital, and the surgeon couldn't find the appendix; it had moved quite a bit. So to this day I have a huge scar on my belly here. And finally they did find it. And then another unfortunate thing happened after the operation was finished: the big scar was held together with clamps, but it turned out that there were not enough clamps, and so some hours later when I coughed the whole scar opened up and all my guts fell out. [laughs]

01-00:02:52
Burnett: Oh my God.

01-00:02:53
Prausnitz: And so this was not so good. I fainted right away. But they had to wash it with some sort of disinfectant and stuff it back in again and put more clamps on. Well, I was in bad shape, and I was told that it was a miracle that I survived. Again, in those days antibiotics were not available, so it was a serious matter. Now, it affected my life. First of all, just having been so sick changes your outlook on life, but then, perhaps more important, because of this event I was barred from doing any sort of sports. They didn't want to take any chance

about the scar opening again, and so for several years I couldn't do anything physical that would cause exertion, and that's not good for a child of that age. Now, later on, when I was about eight, there was another operation where they took all the tissue out that had been damaged, and sewed me up again, and this time they put in enough clamps. And so that helped to normalize my life. After that I was able to do physical things more easily. But for about three years, from five to eight, I was quite severely restricted.

One of the interesting things about that second operation was they sewed me up with a silver wire-like thread, fairly good diameter, maybe a tenth of a millimeter or so. And when we went on vacation, we went to Czechoslovakia, I think it was. At the border they asked my mother why I had all these bandages around, and she said, "Well, he's had this operation, and he had to be sewn together again." And the people at the border asked, "Well, what did they use?" And she said, "Silver wire." "Oh," the official said, "well, that's a valuable item," and said, "that's worth a lot of money, so it's going to be difficult to allow you to leave." Incredible. That's strictly harassment. Well, after a while they said, okay, we could go, it was all right, but it took some doing. And that, of course, made quite an impression on me, too.

My whole family had been Jewish for generations, and Jews, of course, were harassed badly, and this was one example of it, but it was okay. We finally did get to Czechoslovakia, and we went to the town of Pilsen, which is, of course, famous for its beer. And so at age eight I was introduced to pilsner beer, so that was a happy compensation [laughter] for all the various troubles we had. And my father was not with us—he stayed home to take care of his patients—but my mother was not a big beer drinker at all, and so she would order some beer, because that was the thing to do in Pilsen, but then she wouldn't pay much attention to it, and my sister and I would finish the glass while she wasn't looking. So that was a happy experience.

01-00:06:39
Burnett: [laughs] In amongst a lot of challenging turmoil politically and in society.

01-00:06:43
Prausnitz: It was pretty bad. I was in a public school, and I was harassed there, although not really very much. It got much worse later on when we left Germany in 1937, and so by that time there was harassment but it got much, much worse later on when we were already gone. I have one interesting little memory of my school days. I was about seven or eight years old, and school started very early, and, of course, in Germany the main meal—at that time, at least—was around the middle of the day, right? *Mittagessen*, "*mittag*" meaning "middle of the day." And then evening you just had a light supper. So because of that there was a custom to have a little repast in the midmorning, which was called the second breakfast, and it was customary for the students to have a little satchel with a string around the neck, and then a satchel, you have a sandwich or something like that. So, sure enough, we did that every day, and if the

weather was good—and it was, many times—we would be allowed to go out in the courtyard for our second breakfast, and we were told to walk around in a big circle, very leisurely, while we were munching on our second breakfast. And, of course, as little boys are, they don't like to walk peacefully around the ring. They would do all sorts of shenanigans, they'd run around and play tag and whatnot, and that, of course, was strictly forbidden. And in the middle of this courtyard there was a teacher with a stick, and there were only boys. There were no girls in the class. It was strictly segregated.

And so one day I was walking around very peacefully, and the teacher comes up to me and he starts beating me with a stick, and he'd obviously made an error. It was clear to him that he'd made a mistake, that he really was looking for somebody else, not for me, and he said, "Oh, you're not the one that I was looking for." But then—this is a sort of a really German characteristic of officials—instead of apologizing or saying "I'm sorry" or anything like that, no, that he did not do; what he did was after he admitted his mistake he pulled out a notebook, and on a piece of paper he wrote me a receipt, a receipt that I've gotten a beating and that the next time I really deserved a beating I could give him the receipt, you see, and I would not get another beating because I have the receipt for this other one. [laughs] It's sort of funny when we think about this today. At the time, that didn't strike me as strange at all. This was part of the Prussian mentality.

01-00:09:57
Burnett: Disciplinary accounting.

01-00:09:59
Prausnitz: Yes, you account for it and then it's all right. There's no need to apologize or anything like that. Well, regrettably, I never cashed in on that receipt. Not long after this event we left for America, so unfortunately I didn't keep it. I wish I'd kept it. It'd be a nice little document.

01-00:10:21
Burnett: Yes. I can't help, of course, in these days, think about the anti-Semitism in Germany at that time, and most recently we have experienced the worst anti-Semitic attack in American history.

01-00:10:43
Prausnitz: In Pittsburgh, yeah.

01-00:10:44
Burnett: In Pittsburgh, just a few days ago. As someone who encountered that kind of interpersonal hatred in an organized fashion in those days, do you have any reflections on that?

01-00:11:01
Prausnitz: Well, yes, I think I do, but let me first point out that this teacher who gave me the receipt, that was not a question of anti-Semitism. That had nothing to do with it. He didn't know who I was, and so he just beat me because he thought I

had been violating the rules. But yes, of course there was anti-Semitism, and it was noticeable, especially with my father. In 1933, when Hitler came to power, shortly thereafter all the Jewish doctors and lawyers lost their licenses, except those who had fought in World War I, and it wasn't enough to have been drafted; you had to have been at the front. That was a requirement. And, of course, Germany has very good documentation for everything, so they knew exactly who was at the front and who wasn't. Well, fortunately, or unfortunately, but anyway my father had been a *Frontkämpfer*, "front fighter," and so he was allowed to keep his license. And many, of course, were not, but he was, and my grandfather—my mother's stepfather, it turns out—my mother's stepfather, he was also a physician, and he had also been a *Frontkämpfer*, so they both retained their licenses. But nevertheless, I, of course, knew about these things, and that made quite an impression on me, that such a thing could happen. And then every so often somebody from the Brown Shirts, the SA, it was called [*Sturmabteilung*, "storm detachment," a name adapted from the "shock troops" of the German Army in World War I]; not the army, a Nazi group—and they would come around every so often and make life a little difficult for my father. They never really did any damage, but I remember I think one of them came in and went into the waiting room, where patients were waiting, and announced that Dr. Prausnitz was Jewish and that they really shouldn't be here, and so on. So there were little things like that, but I must say it wasn't really very bad. I didn't really suffer from it.

Another memory I have of those years is that I was sent out to get some milk. Well, in the street where we lived, very nearby, there was a milk shop, and the milk shop consisted—it was a big container of milk that was, I guess, delivered every morning. And so I would go down with a bottle, and I would go to the shop, and then they would fill up the bottle. They had a device for filling the bottle and closing it, and then I would go back upstairs to my apartment. It's sort of strange now when we think of it that you actually go down there and have your bottle filled, although I thought of it the other day when there was this similar situation with beer here in the United States. I think they're called growlers.

01-00:14:25
Burnett: That's right, yes.

01-00:14:26
Prausnitz: So what they do in America with beer is what we did with milk in the old days. So that's another memory I have of that particular time.

01-00:14:38
Burnett: Well, it's certainly more environmentally-friendly. Instead of a disposable container, you can just fill it up again and again.

01-00:14:45
Prausnitz: Yes, it is. So by and large I had a pretty good time, and people were generally nice to me. I didn't personally experience any overt anti-Semitism. I'm sure

there was some that I just didn't notice. But I noticed also later on, much later on, when I went back to Germany on sabbatical, that most people were very, very nice. There was no real vestige of anti-Semitism that I could discern. What happened usually in those later years is that if I was with people who were older than I am, then I would be somewhat uncomfortable. I would feel, now, see, where were you during the Holocaust period? But with people who were my own age or younger, there was never any problem. I felt perfectly comfortable with them. There was no difficulty. And now, of course, the last two times I've been in Germany, people who are older than I am have died, and so I feel very comfortable in Germany. I have no real problem at all. Of course, it helps that I speak the language. That makes a big difference.

At age nine we came to the United States, and my father's brother lived in Lynbrook. Lynbrook is a town in Long Island, Nassau County. And my sister and I stayed with my uncle and aunt. They were very generous in taking care of us. And we went to school, and the school, of course, completely different from what I had been used to. One of the things I found absolutely amazing is the hygiene course. There was a hygiene course. Every so often somebody would come in and talk about hygiene, and we were taught how to use a toothbrush, and how to use a comb, and so on. And I thought this was absolutely amazing. After all, this is school. It was unheard of in Germany at that time. Now it may be different, but you have hygiene. Of course I know how to use a toothbrush! I don't have to have a teacher at nine years old telling me how to do that. [laughter] Also, another thing I found is about reading. In the German school—again, at that time; it's maybe different today—we start school at age six, and in the first year we learn how to read, and that's it. There's no further reading. In one year you learn how to read, whereas here I was in the third or fourth grade or whatever and they were still teaching reading. I found that very, very strange that it takes so long to learn reading. I mean, reading English is no more difficult than reading German, but it just stretched out. [laughter] I found that very, very surprising. And then, of course, the classes were coeducational. That was something new.

But I had a very good time in Lynbrook. We were there for a couple of months, and I learned English very, very rapidly. Another memory I have of that time is my parents stayed in Manhattan while my father was getting ready to take the language exam. That was all that was needed at that time. His medical license was recognized in the State of New York, his German medical license, and all he had to do was take a written exam to show that he could speak English. So he was preparing for that, and when he took me and my sister out to Lynbrook on the Long Island Railroad he started talking about the German government and Hitler and whatnot in a very negative sort of way, and I was terribly scared. I said, here we are in a public place, there are people all around, and here you're saying these things. That's terribly dangerous. Because I had been taught in Germany always to keep my mouth shut and not say anything in a public area. And so then I learned about America. My father explained, "Look, we're not in Germany anymore. We're in a new country where you can talk if you wish and it's all right. Nothing's going to happen." That was an illumination to me, that you could sit in the train with other people around and speak openly.

Then, after our Lynbrook days, we lived with our parents in Manhattan for a while. It was a rooming house. When I say "we," I mean my sister and I. And one thing I remember: it was a hot summer, and we went out to get the newspaper, the *New York Times*. And this is my father and I. We went out to get the newspaper, and the newspaper, the *New York Times*, was two cents. And so we walked awhile, and then there was a choice. My father said, "I can either buy you an ice cream, or we can take the subway back." It was hot; one didn't want to walk too much. "But we can't afford both. Either one or the other: either we can have ice cream, but then we can't take the subway and we have to walk back." And, of course, I picked the ice cream, naturally. [laughs] But, again, that made quite an impression on me, that I had to make a choice between these things. My father came from a family where his father had died early, and there was not much money. They were not poor, but they lived very carefully. And I remember him telling me at the time when he was a child that at breakfast they would have a French bread type of bread, and there was a choice: on the bread you could have either butter or marmalade, but not both. They couldn't afford that. Either one or the other. And that I remember vividly every morning when I have butter and marmalade, [laughter] of course, now. But it always strikes me that this is really a luxury, and that I really should be more modest, and have one or the other but not both. So that is a memory every morning I think about.

01-00:21:34
Burnett: And the values imparted by your father and by your family, not just by circumstances but also these are teaching—he was teaching you about values.

01-00:21:45
Prausnitz: Being frugal.

01-00:21:46
Burnett: Frugality, absolutely.

01-00:21:47
Prausnitz: That's right. That's right.

01-00:21:51
Burnett: And so was that the only time he reflected on the past? Did they close the door on the past, or—?

01-00:22:01
Prausnitz: Well, not immediately, because we still had relatives. My father's parents had died, but my mother's parents were still alive, her mother and her stepfather. And they were complete, convinced Germans. There were not going to leave Germany. This was out of the question. That was their country, and so on. My grandfather in World War I had a major position. He was a major in the

Medical Corps, and he served in the Crown Prince's regiment, and he knew a lot of the other officers there, and they would have annual reunions, good old days. So he knew everybody there, and he thought with his connections nothing would ever happen to him. Of course, he was wrong. But so we still had the connection with Germany through my mother's parents. Eventually they came out, after much difficulty, and so then the contact with Germany essentially ceased.

01-00:23:06
Burnett: So they were able to escape.

01-00:23:08
Prausnitz: My grandparents, yes. They were able to escape. Yes, they were able to get out. Again, I don't know all the details, but it was a quota system at that time. Immigration was a quota. And my grandmother was born in Germany, and so she was on the German quota, which was quite liberal. And so she was able to come relatively easily. She came, I think, in early 1941, when America had not yet entered the war. But my grandfather had been born in the eastern part of Germany, in Silesia. And a good part of that country became Poland after World War I, so according to the rules he was on the Polish quota, and the Polish quota was very small. The [United States] Congress didn't want to have Polish people come. They're perfectly happy to have Germans come. Anyway, he was on the Polish quota, so he had to wait until his number came up, and so in order to get out he fled to Italy, which was relatively simple at that time. He could do that. And he spent quite a bit of time in Italy. He was actually helped quite a bit by the Catholic Church. They were quite helpful, and he was there, oh, I don't know, a year or so, and then he was able, finally, to come home, through Portugal. That was the way you would do it: you would try to go to Portugal, which was neutral, and then there were ships from Portugal that went to New York.

01-00:24:45
Burnett: Were there other extended family members that you know of who were able to get out?

01-00:24:48
Prausnitz: They had all gotten out. My father was one of seven children, and they were all out beforehand, many of them, like my uncle in Lynbrook. He'd come to the United States long, long before Hitler. It was before World War I. And there was another brother who went to Chicago long before Hitler came. And the others also were out. One of my uncles was a very wealthy man. He was in the hotel business. He owned a whole chain of hotels. And he had most of his money outside Germany, which was not really legal, but he felt so uncomfortable about this that he wanted to get out and get all of his money out. And so he did something amazingly clever: he went to the Honduran Embassy and said, "Look, I would like to become an honorary consul of Honduras, and I'll become consul of Honduras to Monte Carlo." It was, of course, a joke; Honduras and Monte Carlo have no dealings with each other.

But he, obviously, had to pay for this, and he was perfectly willing to do that, because he now got a Honduran passport—not only a Honduran passport, but a diplomatic passport—so that he could go back and forth without being checked, like a diplomat. And so he went to the bank or whatever, took out his money, his securities, whatnot, and he was able to go to Monte Carlo without any difficulty. So he saved all his money, and he was never upset in any way. So he was really very, very clever about this.

But no, they were all out except my father's youngest brother, and he got out a little after we did and went to Israel. He was in Haifa. But everybody else was gone. And my mother had two sisters, and they were out, too. One came to New York; the others went to Australia. So my family didn't really suffer much. Everybody was out by the time the Holocaust got going.

01-00:27:18
Burnett: Thank goodness. The time that you were in Germany, you would have had three years of primary school?

01-00:27:28
Prausnitz: Yes, three years, that's right.

01-00:27:33
Burnett: Okay. So whether in Germany or in the United States, can you talk a little bit about education, about your parents' encouragement, or lack of encouragement, to learn?

01-00:27:48
Prausnitz: Oh, the encouragement was very strong. I was expected to get good grades. It was not discussed; that was just expected. So I did. [laughter] Fortunately, I didn't have any problems. I was a good student, and I enjoyed it, so there was never a difficulty. And it wasn't really discussed; it was just expected that's what you do. I mean, just like you get up in the morning and brush your teeth, that's what you do, and you go to school and get good grades, and it wasn't really talked about. [laughter] I think, looking back, the education was very good. As I mentioned earlier, we learned to read in one year and it was never touched again. You knew how to read. And arithmetic, and geography, and history, it was all taught very well. There were good teachers, and there was no playing around in the classroom like you have here. We didn't draw pretty pictures to hang up on the walls. It was a much more serious academic atmosphere. I think that's all changed now—it's much more Americanized—but at that time it was rigorous. And you had respect for the teachers. You did what they told you to do.

One thing that I remember that I did not like at all is penmanship. We learned penmanship, but we were not allowed to use fountain pens. We had a pen, a piece of wood, and there was a metal pen at the end, and in the desk there was an inkwell, and you had to keep dipping in the inkwell to write. That was a bit of a nuisance, [laughter] but that's a very minor thing, but I still remember that. Also, of course, you got inkblots all over. It was not a very clean way to do things.

01-00:29:55
Burnett: So at that time there was a big enthusiasm for physical culture, right?

01-00:30:00
Prausnitz: Yes.

01-00:30:00
Burnett: And as you mentioned, you had been ill, and you were prevented.

01-00:30:04
Prausnitz: That's right. No, those years I couldn't do much of that. My father was a great proponent of that. He had thought that physical exercise was very important, and he'd been quite active in sports. He played tennis all his life until the last years. And he went skiing, which was rather unusual at that time. And he did all sorts of gymnastics as a younger man. And also—this is a little bit strange—he learned boxing, which was not so unusual in those days. Boxing was quite common, as a sport, not a competition or championships. I don't mean in that sense, like you have here, but as a relaxation. So he did a lot, and he certainly encouraged me and my sisters to do as much as we could in that area, but because of my illness problem sort of let me down I couldn't do much. But I was not unhappy about that. I'm not the type who particularly enjoys physical exercise, so it came in handy, in a way. [laughs] But then after I was nine, of course, I couldn't use that excuse anymore, so then I had to start doing exercises.

01-00:31:27
Burnett: And so during that time, were other students exercising and you had to stay in?

01-00:31:33
Prausnitz: Yes.

01-00:31:35
Burnett: And you would do drills or some kind of rote work?

01-00:31:38
Prausnitz: Yes, that's right. I had to do something else. And I think my physical activity at that time was to play chess, the little chess club at the school. So that was my physical activity: playing chess. [laughter]

01-00:31:57
Burnett: That's great exercise, good exercise for the mind. So your family encouraged you to do well in school. Was there any enthusiasm around—did your father take you to his doctor's office? Was he thinking—

01-00:32:19
Prausnitz: The office was right there. The office was part of the apartment. The front part of the apartment was his office. He had several rooms there, a big waiting room, consultation room, and a treatment room, and a restroom, and then after there was a big door, and then when you opened the door you got into the private parts: dining room and living room, the bedroom and the kitchen, and so on. No, we lived very well. We lived very comfortably, and we had balconies, and then on New Year's Eve we would throw out fireworks. It was a very pleasant life.

01-00:33:01
Burnett: Well, your family is established in the United States, in Long Island initially. Can you talk a little bit about that further adjustment? It was quick. You learned English right away.

01-00:33:14
Prausnitz: Very quick. Very. I had already learned some English before, but not very much. But yes, I certainly could read it, and then in the Lynbrook school I picked it up. Children pick up languages very quickly.

01-00:33:29
Burnett: And so you attend high school. Essentially, two things happen: one is secondary education, during World War II. Do you remember anything about the start of World War II? Or the entry of the United States into World War II?

01-00:33:44
Prausnitz: Oh, yes, I remember Pearl Harbor. Of course I remember Pearl Harbor day. I was listening to the radio, and it was terrible news. Yes, I remember that distinctly. Now, it didn't really affect us very much. After a while there was some rationing, like sugar and meat were rationed, but not significantly. We were still able to eat quite well. Gasoline: now, that was more of a problem, gasoline rationing. But my father being a doctor—in those days doctors made house calls—he got an adequate amount of gasoline. So we didn't suffer from that, either. No, I wouldn't say that World War II really affected us. Now, as enemy aliens, we had to register, and, of course, we did. And we were restricted: if we wanted to leave our home for a distance—I think it was fifty miles—we had to report that. Well, we don't go fifty miles very often, but once in a while. So there was really a minimum problem. So those years went quite well. Well, I was in grammar school until I was thirteen, and then I went to Forest Hills High School, which was a brand new high school at that time, and I was there for four years. Those were good years. I must say I enjoyed those years.

01-00:35:20
Burnett: So you settled into Forest Hills from further out on Long Island.

01-00:35:27
Prausnitz: No. No, no, my sister and I were in Lynbrook for just two months or so, and then we spent some time with our parents in Manhattan, and then that fall we were in Forest Halls.

01-00:35:41
Burnett: And Forest Hills is a beautiful, leafy kind of—

01-00:35:44
Prausnitz: Well, it certainly was. It was gorgeous at that time. I loved it there. But I don't know what it's like now. It's probably all built up. The subway had just come to there, and, of course, that changed things tremendously. It became much more populated. But yes, the school I went to was Public School 101. That was right in the best part, so beautiful homes and lawns and so on. So, no, I remember those years quite, quite fondly.

01-00:36:18
Burnett: And the subway extends out to Forest Hills now, and it's a stop.

01-00:36:23
Prausnitz: Oh, yes, the subway came to Forest Hills about 1937, '38, and then it's gone out further. It goes out to Jamaica, and I think some points beyond. So yes, the subway is there, and has been there now for many years, and is used a lot.

01-00:36:44
Burnett: Do you remember the first time you went on the subway into Manhattan? Do you remember the first time going to Manhattan?

01-00:36:49
Prausnitz: Oh, yes, I do remember. It was from the ship. The ship was there on the Hudson River, and we got off the ship, and I see all these huge buildings. Of course that made a tremendous impression on me. And then we got into a car, and we drove around, and, oh, yes, it was very impressive to see all these skyscrapers.

01-00:37:14
Burnett: And when you were a little bit older, did you go in by yourself? Do you remember the first time you went in by yourself?

01-00:37:17
Prausnitz: Oh, yes. Oh, yes, I would go in there. In those days, one wasn't worried about the sort of crimes there are today. Oh, yeah, no one considered that the subway might be unsafe for anybody. Sure, I would go in for whatever reason.

01-00:37:36
Burnett: And there's all these tremendous opportunities for education, for learning. Did you go to museums? Did you—

01-00:37:47
Prausnitz: Oh, yes. I would go to museums, and New York is full of them. I don't think I've seen them all, but I've seen perhaps half, or maybe more than half of

them. And concerts: I early developed a great love for music. And so we would go for concerts. In fact, one concert, I remember going with my mother. We heard Toscanini. Toscanini conducted the NBC Symphony Orchestra. He was already quite elderly at the time, but still quite vigorous. [laughter] He kept the orchestra going. So that was very impressive. And also, my first opera was *The Barber of Seville*, and I saw that with my mother and my sister, I think, was there, too. And I just loved it, and I thought this was just marvelous. And my love for opera has never waned. To this day, I love opera. And I enjoyed that very much. You may know that the Metropolitan Opera in New York broadcasts every Saturday—perhaps you've heard it sometime—and that started just about the time when I was twelve years old, and I was glued to the radio every Saturday. And I kept that up for a very long time, and even when I started here in this office I would come Saturday morning. Because of the time difference with New York the operas started at around ten o'clock in Berkeley. So I would be here at ten o'clock, and I'd have the music on. So that was very nice. I enjoyed that tremendously. And I know quite a bit about opera because of that, because in those days when the intermission came they would teach you. They would talk about the opera, they would talk about the composer, and all sorts of relevant things. They don't really do that anymore. Now, it's been watered down a lot. They have interviews with the singers, and the singer talks about his or her children, and so on, which I don't care about. I'm not interested in that. [laughs] I want to learn something about the opera, and in those days they did that, but now they don't do that.

01-00:40:20
Burnett: There's not just the drama of the opera itself, and you might not have access to the language, and if it's sung in Italian or whatever you might not have immediate access to the story, so getting the context and getting the context of the story of the opera, the writing of the opera itself, right? So it's the story that captivates you? It's the music?

01-00:40:42
Prausnitz: Well, the two go together, of course, and I learned very early that the story is not to be taken literally. It's symbolic. It's suggestive. Because that's always the objection I hear from non-opera-lovers, who say, oh, the plot is so ridiculous, and you have these people, they're running around doing all sorts of silly things. Well, yes, if you take it very literally then it is silly, but if you take it symbolically then it's not silly at all. There's a lot of meaning and a lot of philosophy, but you've got to make an effort to get that. And these broadcasts that I listened to help you in that. They gave you background material so the effort would be somewhat lessened. But I read a lot about opera, and I would see opera movies. Going to the opera itself is, of course, a very expensive business, now almost prohibitively expensive if you want a good seat, so I couldn't go to many operas, but I would get records and videos now, so I learned a lot about operas from videos and radio.

01-00:42:05
Burnett: Was it a private passion of yours or did you share it with your family?

01-00:42:07
Prausnitz: Absolutely. No, no, my family has no interest in opera, which always upset me very much. I wish that they had some interest, but they don't. And, even worse, my wife doesn't care about opera, and my two children don't care about opera, either, so I've completely failed in that regard, although they'll go with me to an opera, you know, just to show that they're cooperative, but it doesn't really mean very much for them like it does to me.

01-00:42:36
Burnett: But it's a key passion in your life.

01-00:42:40
Prausnitz: I think so, yes.

01-00:42:41
Burnett: Yeah, and it restores the soul.

01-00:42:44
Prausnitz: Restores the soul, yes. [laughter] Yeah. No, no, there's some just wonderful experiences, and I love the opera quiz. They still have that in the intermissions. They get a bunch of experts there, and the questions are sent in by the listeners, and this board of experts answers them, and it's often quite illuminating, and also often very humorous. They often make jokes. Only once did I send in a question to the opera quiz, and this was, oh, about five years ago or something like that. And I said, "It's well-known in opera that the plots of many—not all, but many—operas is that the tenor is trying to get the soprano, and the baritone is trying to prevent it. Many operas, that's the essential plot." And so I said, "Well, now, experts, name some operas where the baritone is trying to get the soprano and the tenor is trying to prevent it." There are some, not many.

01-00:44:10
Burnett: Well, you're asking me? [laughter]

01-00:44:13
Prausnitz: So that was a pretty good question. However, my wife, who is a bit of a whiz, says, "Oh, no, no, no. You shouldn't send it that way. After all, you're coming here from the San Francisco area. You should write, 'Where is there an opera plot where the soprano is trying to get the tenor and the baritone's trying to prevent it?'" But I didn't do that. I didn't change it. [laughter] Didn't change it. I left it in my original. But they didn't take it. They didn't take the question. That was very disappointing.

01-00:44:55
Burnett: Well, why don't we pivot a little bit to talk about science. When was the first inkling that you had that you wanted to—, or maybe to ask a more open-ended question: did you have a passion for science?

01-00:45:17
Prausnitz: "Passion" is probably too strong. I enjoyed it, I liked it, but I think "passion" would be an exaggeration. I had a very good high school science instruction, unfortunately not in biology. I never took a course in biology in high school, which was too bad, but we had very good chemistry and physics, and also mathematics. So I enjoyed those courses. I enjoyed all my courses, but I enjoyed those very much, and I did very well in them, and especially chemistry. I really recognized that chemistry is life. Chemistry is how we live, and the body, what we eat, and what we inhale, and so on, and so on. Chemistry is it. And so I wanted to do something in that area, but I also wanted to do something in that area that would be applicable, and so I thought maybe chemical engineering would be suitable. And that was greatly encouraged by one of my teachers from grammar school. One of my teachers from grammar school was a good friend of my mother's, and her brother-in-law was a chemical engineer, and so she told about him, about how successful he was, and so forth. And this news got to my mother, and my mother said, "Oh, yes, chemical engineering. That really sounds good." So that's what I decided I wanted to do in college, and I did when I went to Cornell. I started right away chemical engineering.

01-00:47:06
Burnett: Oh, you did? It was something you declared as soon as you were able?

01-00:47:09
Prausnitz: Oh, yeah, decision—

01-00:47:12
Burnett: Were you able to declare in the first year?

01-00:47:14
Prausnitz: Yes. Yes, definitely. Now, remember those were different times. This was right after World War II. The place was overrun with veterans coming back, and so everything was expanded. You couldn't kid around. I mean, this is it. I want to do this. Now, sure, if it turned out that you changed your mind, you could do that with some difficulty, but no, no, when you come in they want to know right away. Today it's different, probably, but at that time we had to be clear right away what we wanted.

01-00:47:53
Burnett: Well, there was a tremendous crunch, between returning veterans— There were probably fewer spots.

01-00:47:59
Prausnitz: Well, that's right. It was hard to get in, but the university expanded, to the extent that they could. But yes, I was seventeen years old, and I was rather naïve, compared to the others, who were several years older, had been out in the real world. I had lived in a rather sheltered world. And I think I mentioned in that memoir that I sent you about the director of the School of Chemical Engineering, a man named Rhodes, how he greeted us freshmen. He explained

to us, he said, "The person in front of you and the person in back of you and on either side, you've got four immediate neighbors; three of those won't be here next year." Oh, he was tough. He was very tough, and he made no jokes about it. He said, "I know you've heard terrible things about what a bastard I am. Let me tell you: it's all true." Oh, he was something. He scared the hell out of me, [laughter] but fortunately I prevailed. I was very dedicated, I worked hard, and so I was able to get a degree. In those days, it was a five-year program. That has since changed. But I was there five years, and I learned an awful lot in five years, there's no doubt.

01-00:49:38
Burnett: Do you think that severity—it was partly the function of the times, but it did point to the seriousness of the endeavor, right? That this was your chance to shine. It was very competitive, and they stressed that competitive element, that this was—

01-00:49:55
Prausnitz: Oh, yeah, absolutely. It was competitive.

01-00:49:57
Burnett: You had to excel. You didn't—

01-00:49:58
Prausnitz: You had to prove yourself, yeah.

01-00:50:00
Burnett: Well, we've gone right ahead here. Have we talked about siblings? Did you have brothers and sisters?

01-00:50:08
Prausnitz: Yes, I had one sister, and she's four years older, and we had a very good relationship all the time. She died, let's see, it's eight years ago now, and that was a big, big loss for me.

01-00:50:24
Burnett: I'm sure. I'm sorry.

01-00:50:26
Prausnitz: My parents died. It's a big loss, but you figure, well, they're another generation. But Laura was her name, and Laura and I were the same generation, although she was older, of course. But we had a very good relationship. We always got along very, very well, and I liked her husband, and she liked my wife, so it was a very close relationship. But she died of cancer, and that was a big, big blow. We spent a lot of time together, you know, early youth. We didn't go to the same school, but, well, for many years we had the same bedroom. So that was a very good experience.

01-00:51:12
Burnett: And she went to a different school, and you went to Cornell. What was the reason for Cornell? Is it because you got in and that was it, or did you look at Columbia or other places?

01-00:51:30
Prausnitz: The place that I had really picked out was Rensselaer Polytechnic Institute. They were somehow attractive; I don't remember what the reason was. But then I was advised by the high school advisor, try Cornell, which he thought was a better university. I'm not sure that's true, but anyway, may have been true at that time. So I applied to both, to Rensselaer and to Cornell, and I was accepted at both, but Cornell—I don't remember exactly why I picked Cornell, but I guess it was the reputation.

01-00:52:11
Burnett: And, of course, you have to move upstate.

01-00:52:15
Prausnitz: In either case. Rensselaer's in Troy, New York. Cornell, Ithaca is a little further. Not much.

01-00:52:27
Burnett: And can you tell me a little bit about the experience? You've already alluded to the competitive and frightening atmosphere. [laughs]

01-00:52:34
Prausnitz: Well, I was quite happy there. I had a nice roommate, and we had a nice room there, right in the center of the campus, and this was the first two years. And I learned an awful, awful lot there. The thing that I particularly enjoyed when I first got there was the fact that I could decide what to eat. When I lived at home, I didn't have anything to say about it. I was given something to eat and I was expected to eat it and finish it, and I did. You know, that was the way you lived. That was the thing to do. And, all right, most of the time I liked what my mother prepared, but sometimes, of course, I didn't. But nevertheless, I ate it all. And I didn't have that problem at Cornell. There was a cafeteria there, and I could pick out what I wanted, and this was a wonderful new experience, [laughter] that I could eat what I wanted to eat and not what my mother wanted me to eat.

01-00:53:43
Burnett: Could you have marmalade and butter?

01-00:53:45
Prausnitz: Yes. [laughter]

01-00:53:46
Burnett: Yes, finally!

01-00:53:47
Prausnitz: Yes. Well, no, no, that was okay even then.

01-00:53:49
Burnett: I'm teasing, yeah.

01-00:53:52
Prausnitz: That remark about either/or referred to my father's youth. No, no, I never suffered from that problem, but I'd remember it. Every morning I think of it.

01-00:54:03
Burnett: Yeah, no, absolutely, not taking anything for granted. Absolutely. Can you tell me a bit about your mother?

01-00:54:11
Prausnitz: She was not as big an influence on me as my father was. Certainly in Germany she was quite repressed. It was the situation, quite common in the middle class certainly, that the husband is the dominant person, and the wife is sort of subservient, and that was certainly true of my parents at that time. It changed radically later on in the United States when my mother felt much more liberated. She helped him quite a bit in the medical practice. She would develop X-ray pictures. She would do urine tests to measure sugar. She kept all the books. She was very good at that. She was a very good accountant. So it wasn't as though she didn't do anything. She was quite active in helping the practice. But the influence on me was not much at all.

01-00:55:17
Burnett: But the family was tight in the sense that they worked together and raised you.

01-00:55:21
Prausnitz: Oh, yeah, we worked together, and we would always eat a main meal. *Mittagessen* we'd always eat together. So yes, it was a closely-knit family, and we all loved each other, and so on, but my mother didn't exert much influence. And she was, of course, also very devoted to her parents, who lived not far away, a couple of blocks. And I have one recollection of my grandparents. They were Jewish, but completely assimilated, and on Christmas they would have a Christmas tree. My father didn't like that at all, but they had this Christmas tree, with real candles on it, and the candles were lit, and oh, it was lovely. They were quite well-to-do, and they had a beautiful apartment. It was really very nice. Well, one day—I think I was four, five years old—the tree caught fire, and the fire engine was called. Well, this was the most glorious Christmas I ever had. It was wonderful: you'd see the firemen coming with the hose. They put out the fire. Nothing much happened, and it was— But boy, what a thrill that is a four-year-old boy, to have the fire engine come to your place. [laughter] So I have very vivid memory of that. I was very thrilled.

01-00:56:50
Burnett: I imagine for some time after that you had to be dissuaded from your desire to become a fireman. Was that—?

01-00:56:55
Prausnitz: Oh, I never wanted to become a fireman. No, no, no, it didn't go that far, [laughter] but I was thrilled to see them come.

01-00:57:03
Burnett: Yeah, the spectacle. Oh, yeah. Well, so returning to Cornell, you were immediately in chemical engineering, and it was difficult, it was challenging, but you excelled.

01-00:57:20
Prausnitz: No, I didn't excel.

01-00:57:21
Burnett: You didn't?

01-00:57:22
Prausnitz: No, I don't think I could claim that. I did well. I did quite well, but "excel," no. There were certainly some students who were higher in their grades than I was. And I think one reason that I didn't excel as much as I perhaps should have is because I had many other interests, intellectual interests. The thing that Cornell did at that time, for which I am forever grateful, is we were required to take a year, two semesters, of history of science. And there was a marvelous teacher there—Professor Guerlac was his name. He was of French origin, but he spoke perfect English. And Professor Guerlac taught this course on the history of science, and that opened up vistas for me that I had never heard of before. And the connection between science and culture became evident. And so I really enjoyed that tremendously, and started to do reading, and also it encouraged me to read literature, to see the connections there, to read general history, learn something about economics, history of science. I bet you know this better than I do: history of science has everything. And so my interests and my reading broadened tremendously, and I spent a lot of time on these things, and perhaps I should've spent more time on my chemical engineering. Anyway, although I learned a lot, I would not say I excelled. No, that's not true. I did okay, but "excel" is, I think, too strong. I started to excel when I went to graduate school, and I really buckled down and I exceled then, but not as an undergraduate, no.

01-00:59:30
Burnett: Can you talk a little bit about that year? And it sounds to me that it's something—you said you're forever grateful—it's something that you kept up?

01-00:59:41
Prausnitz: Oh, yes.

01-00:59:41
Burnett: So it opened pathways of thinking—

01-00:59:44
Prausnitz: Absolutely.

01-00:59:45
Burnett: —pathways of learning, that you then continued later in life, or throughout your life.

01-00:59:48
Prausnitz: Right. I continue to this day. Yes, it opened areas that I think most of my colleagues and most of our students are not aware of. They just don't realize how science is a part of, rather than apart from, the way we live, of our culture. Not just the way we live in the material sense, that we have clothing and food and so on, but in our mental sense, the way we live, the way we think about things. Science is just one part, and there are all sorts of other things that influence us, and most of our students don't quite appreciate that. I tried very hard in all the years I've been here to change that, and I've had very limited success. Just, oh, a couple of months ago I asked the Townsend Center, the humanities center, I said, "What can we do, or what can you do, to introduce more humanities into the College of Chemistry's operations?" And I don't mean courses. That's not what I'm talking about. I'm talking about some sort of joint research or seminars or something like that. It's a little vague; I don't know how to do it. And I hoped that they would come up with something, but they haven't. I talked to the director there. He's a professor of French, I think, very nice, very pleasant [Timothy Hampton. And I talked to the assistant director [Rebecca Egger], who really runs the show there, and also very nice, very cooperative, "Oh, yes. That's part of our mission at the Townsend Center, to build bridges, and so on." Well, we had a big meeting that the dean attended, and it didn't come to anything. It was what I feared. My colleagues, by and large, aren't particularly interested. It's okay, they don't object, but they aren't particularly interested. They say, "if we have students here who are interested in history or whatever, okay, let them go down and take a course in history, but don't bother me with this," you know? And so the will to do something about it is not obvious. And, of course, everybody is busy up to here. Everybody has a lack of time. They can't get all the things done as it is. And then to say, "Well, why don't we spend some time thinking about science and culture and so on," there's no time for that. It doesn't have a high enough priority. Nobody's opposed to it, but they never get around to it. Other things always come first.

01-01:02:45
Burnett: So in your year with Guerlac—is that right?

01-01:02:49
Prausnitz: Guerlac.

01-01:02:49
Burnett: Guerlac. His year of history of science, it was integrated into—?

01-01:02:55
Prausnitz: It was part of the curriculum. That's one reason it was five years, rather than four years, because they had extra things in there, extra electives, and this course, but this course was not an elective; this course was a requirement, and I think that's great. I wish we could do that here, but, of course, there's no chance. [laughter] Our program is already so full, there's no way to do that. And to go to a five-year program, I know that there are many educators who

believe that we should do that, especially Jud [C. Judson] King—Jud King is certainly in favor of a five-year engineering program—but it's economically not popular. Going to college is expensive, as you know, and to add on another year, it's just not popular at all.

01-01:03:47
Burnett: That is something that is added. The increasing cost of education has inspired, in a sense, forced people to become more instrumental and goal-oriented, and not do the kind of exploratory work that was being asked of you when you went to your undergraduate experience. You really learned about the world, which is kind of a liberal-arts vision of the cultivation of the individual so that they can then think about their life calling in a larger context.

01-01:04:23
Prausnitz: Well, or do something in the world to make the world a better place. I learned that attitude at Princeton. I was a PhD student at Princeton, and they certainly at that time very much encouraged this idea that you should become a citizen, you should become a cultured person, you should become aware of how your work fits in with the rest of your life. You should become integrated, you might say. At Princeton, that was very much encouraged. Not here.

01-01:05:00
Burnett: Well, you had those salutary experiences when you were at Cornell, and at Princeton. And you were able to finish in five years, and it's now 1950.

01-01:05:17
Prausnitz: Yes.

01-01:05:19
Burnett: But speaking of larger context, there's so much going on while you are in college, learning about your place in the world. You're learning about your place in the world with the end of World War II, and returning veterans that you're side by side with. Can you talk a little bit about that larger context, as it made you reflect on your purpose and your studies?

01-01:05:49
Prausnitz: Well, the presence of the veterans was an influence, because they would tell stories about their experiences, and so that broadened my range of interests and range of awareness. But we didn't really have too much to do with them. There was a sort of de facto segregation. The younger students, who had just come out of high school, taught a bunch together, and the veterans bunched together, so we didn't have too much contact. We had some, but we didn't have too much contact with them. No, I don't think that I was in any way made aware of the world by contact with the veterans; possibly a little bit, but they didn't have too much influence on me. But what made me more aware of the world was the courses I took. I took electives in philosophy, in literature. I took two courses on Goethe. The first course was a general description of Goethe's work, and the second semester was exclusively for *Faust*. We read

both parts of *Faust*, and commented on it, and so on. That's almost unthinkable here, that somebody in engineering would do that.

01-01:07:16
Burnett: [laughs] A full semester devoted to *Faust*.

01-01:07:18
Prausnitz: Well, yes. It's unthinkable. And at that time, at Cornell, that was not only possible, it was actually encouraged. You were supposed to follow your interests. And I keep mentioning that to undergraduates here, but, again, with little success. I'm not an advisor anymore because I've been retired now, officially retired, for some time, but I vividly recall when I was an advisor to students they would come in and say, "Oh, I've got to take an elective. What would be a good elective for me? What should I take?" they would ask me. I said, "But do you have a catalog?" At that time we still had a catalog; this was before everything was online. And I would say, "Well, look at the catalog. Isn't there anything in there that appeals to you in history or literature or economics?" "Oh, no, no. I want an elective that meets Monday, Wednesday, Friday at nine o'clock. That's a good time for me. I have an open slot then, so I want to see what's available at that time." That was the criterion, whether the elective would fit in with the program, not what it really was intellectually. It was very discouraging to deal with students like that. One other thing I learned at Princeton: Princeton had the—well, "custom" I guess is the word: that is, you should not choose courses; you should choose instructors. In other words, students were encouraged to find out who are the great teachers, and take a course with one of the great teachers, never mind what the subject is. You'll get interested. If the teacher is really good, you'll enjoy it. You'll learn something. So I mentioned that often to my students, and they were completely surprised at this. It never occurred to them to do that. But, of course, it's difficult, because most of the courses are required, and they don't really have a choice. They've got to take the course when it's given, and whoever gives it, that's it, there's no choice. But they do have some choice, because they do have some electives, and we have some very great teachers on this campus. We also have some that are not so great, and so the trick is to find out who the great teachers are and take courses with them.

01-01:09:53
Burnett: Do you recall moments of inspiration that were derived from the excellent teaching that you received? Were there particular teachers? We haven't talked about Rochester yet, but it could be at Rochester, Cornell.

01-01:10:09
Prausnitz: Well, I mentioned Guerlac, and he certainly was one of them. Were there any others—? I can't recall if there are any other names. There was an instructor who taught a course in modern philosophy, and I don't remember his name, but that was also a very exciting course for me. I learned about Bertrand Russell, and about Heidegger, and that whole Oxford school of analytic philosophy. I didn't too much care about that; I cared more about the

humanistic philosophers, the French. I learned about Bergson. So that was very, very good. I enjoyed that.

01-01:11:04
Burnett: Interesting.

01-01:11:05
Prausnitz: I also enjoyed a course on law. There was a course given on engineering law, the kind of law problems that come up in the practice of engineering, and I thought that was very good, very interesting.

01-01:11:20
Burnett: So you got an appreciation for the multiple dimensions of life as it pertains or does not pertain to specifically—

01-01:11:28
Prausnitz: Well, both. Both, yes, both.

01-01:11:32
Burnett: Right. So you finish your five-year degree, and then you do a master's in science at—

01-01:11:41
Prausnitz: In chemical engineering.

01-01:11:43
Burnett: In chemical engineering? But it's—

01-01:11:44
Prausnitz: A master's of science in chemical engineering at the University of Rochester. That took one year.

01-01:11:49
Burnett: Okay. And so that's the M.C.H.E.

01-01:11:50
Prausnitz: And this was actually a little fast. The reason it took only one year is because they gave me credit for some of the courses that I've had in my fifth year at Cornell. You see, that was rather unusual having the fifth year, and so they counted some of those courses towards the master's degree, and that's why I was able to finish that in two semesters. I enjoyed Rochester, and I was a teaching assistant there, and that sort of encouraged my interest in teaching. I enjoyed that very much. Rochester is a much smaller school than Cornell, very good quality, and there were very good people there. It's a private school, as is Cornell, but the classes were small, and you got to know people. And then in the write-up that I gave you I mentioned that there was a professor there of physical chemistry who edited one of the major chemical journals, and he would give us manuscripts submitted to the journal for publication. He would give that to the students to judge them. Whether he actually made use of our criticism or not, I don't know, but anyway it was a very good education

for us. It took us right to the cutting edge, the forefront of the subject. Rochester was very good. And, of course, the Eastman School of Music is there, and so I was able to hear a lot of live concerts and learn about music, so that was very nice.

01-01:13:29
Burnett: So you continued that. You continued the pursuit of music, or the engagement with music that—

01-01:13:33
Prausnitz: Oh yeah, I've done that all my life, yeah. One other thing that I think I might mention about Princeton: the Princeton Chapel, beautiful building, had services every Sunday. They were sort of nondenominational, nominally Protestant but not really very departmental; it was very general. And first of all, they had excellent music. They had a superb organist and director of the choir, so that going to the service was a musical delight. It was just marvelous, except for the hymn singing. Then, of course, everybody sings, not the choir, and the musical quality goes way down. So I could do without the hymns, but when the choir sang, or when he played the organ, that was a very, very nice experience. But the best part is that they got very good people to come and give sermons, and they would invite people from the seminary—there's a seminary at Princeton, Presbyterian seminary, and then the Union Theological Seminary in New York, which is one of the great seminaries in the world—and they would have support people, and they would come. And I particularly enjoyed Paul Tillich, who was one of the leading theologians of that time, and even more Niebuhr, Reinhold Niebuhr, and he would come every so often, maybe once or twice a year, and I would never fail to come hear him or Tillich. I learned an awful lot from that, because not only did I listen intently to what they had to say, but I would also look up books that they had written. And then also another person who had a great influence on me at that time was Martin Buber. Martin Buber became very popular later on. This was in the later fifties. He wrote some very fascinating books that were very popular about—oh, I forget the name now [*I and Thou*]. There was a Jewish group of people in the eighteenth, seventeenth century, and he wrote fascinating books about them. But he came, and he was a very convincing personality, white beard, and he looked like he just stepped out of the Old Testament. And he gave a few seminars. He was not a professor there—he was a visitor there—but he gave a few seminars that I attended, and that really impressed me tremendously.

01-01:16:26
Burnett: Wonderful.

01-01:16:27
Prausnitz: No, I enjoyed the Princeton years. It was very, very nice, and I made good friends for life.

01-01:16:34
Burnett: Right. And that's an important component of that, and going through quite rapidly, can you talk a little bit about your graduate research, what you were doing there, and—?

01-01:16:52
Prausnitz: Yes. By far the leading chemical engineering professor at that time was a man named Richard Wilhelm. He was the best-known and most celebrated chemical engineer at Princeton, and I was fortunate in being allowed to do my Ph.D. thesis with him. And I learned a lot from him. We did some very nice experiments, and then in order to interpret the experiments we had to do some interesting mathematics, which I had to learn, and I'm very happy with that thesis. It really worked out very, very well. Now, it was completely different from what I was doing here in Berkeley. Here in Berkeley I spent almost all of my career on thermodynamics of mixtures, whereas what I had done at Princeton had to do with fluid mechanics in chemical reactors. Well, when I first came to Berkeley, I had a very good student who worked on chemical reactors. He's still here: Elton Cairns was my first Ph.D. student, and he's now Professor Emeritus here. And so when I first came I continued the work on chemical reactors, but I soon stopped that in favor of doing the sort of work that Professor Joel Hildebrand—I followed his line of thinking. I had been introduced to Hildebrand's ideas through his books, so before I came here I knew about his books. I never met him, but I knew about his books. And then when I came here I met him and fell under his very attractive personality. He was quite old already at that time, but still amazingly active. So after a while I stopped the chemical reactor work. I think I had two PhD theses on chemical reactors, but that soon stopped. It was okay. The work that was done, I think, was of good quality, but I wasn't particularly interested in it. I was interested in other things.

Now, I must make a correction: I talked earlier about my listening to the Metropolitan Opera on the radio, and I told you about this question that I wanted to send in, but I made an error there. Let me correct it. I said my question was: in my operas the tenor is trying to get the soprano and the baritone's trying to prevent it. That part was correct. But then when Susie, my wife, said, "Come on now, you're from San Francisco; you have to modernize this question," what I said was wrong. What she wanted me to write is: in what opera is the baritone trying to get the tenor and the soprano is trying to—? [laughter] So I told it wrong the first time, but I didn't do that. I think that if I had said it that way there would be no chance of them accepting it.

01-01:20:07
Burnett: Right, probably not, yeah. In those days, right. This is more recently?

01-01:20:12
Prausnitz: Well, about ten years ago.

01-01:20:14
Burnett: Oh, okay. I see. So, one of the things that we'll talk about next time is the unique nature of Berkeley chemical engineering, and so I wanted to ask you, going back to the time of your work at Cornell and Rochester and Princeton, talk to me about chemical engineering as a discipline. Where is it done? What's the identity of the chemical engineer?

01-01:20:56
Prausnitz: Well, chemical engineering has changed a lot over the years, but the general idea is still, I think, the same. We have numerous, hundreds and thousands, of chemical products. Our life is unthinkable, the way we live today. We have chemical products all the time that we use: medicines, gasoline, rubber tires, nylon, soap. Many, many, many, and thousands of things that we do every day without thinking about them. And the role of chemical engineering is to provide these chemical products in as economic, as pure a way as possible. In other words, the quality of the product is our responsibility, and the price. We want to produce it at a price that people can afford, and that allows the company to make a profit. That part, I think, has remained. That part has not changed. What is somewhat new is that chemical engineers now are concerned about making new products, and that was not so much emphasized in the old days. The old days was: the chemists do that. Chemists make new products, and they make it in a test tube or whatever, they make perhaps a few grams, and the chemical engineer's responsibility is to scale that up to a much larger production, maybe tons per day. That was the big task of chemical engineers in those days, and it still is, and most chemical engineers today still do that. But as I said, the new part that has come out in the last, I don't know, twenty years is that not only are chemical engineers supposed to scale up, to take chemical products for large production, but chemical engineers are also expected to participate, with chemists and others, to make new products. And that has now become a very important part of chemical engineering.

01-01:23:24
Burnett: So one of the things that's clear is that it has this connection with industry, historically—

01-01:23:33
Prausnitz: Oh, absolutely.

01-01:23:33
Burnett: —and so that that notion of scale, and scaling, is very much wedded to industrial processes. And so I imagine in chemical engineering there are various drop-off points, including perhaps before people even finish their undergraduate degrees, that there is a thirst for people with training, and they might just do two years and go off. I don't know. But at various points—undergraduate, master's, PhD—people would drop off. You went all the way.

01-01:24:11
Prausnitz: Well, yes. The drop-off before the bachelor's degree is relatively rare. If people leave before the bachelor's, it's usually for some health reason or

economic reason, don't have any money, or they just don't like to go to college or whatever, but that's relatively rare. However, once the bachelor's degree is obtained, then the overwhelming majority of students go into industry. It's only a relatively small fraction that go for further training. I don't know what the number is; maybe something on the order of ten percent, something like that. So the connection with industry is still essential. It's industry that makes us possible, because they want our services. And usually chemical engineers do very well. They usually have the kind of background that industry wants. And one thing that is very nice about chemical engineering education is there's a versatility. We have our students do all sorts of things, which is not so much true in the other engineering fields, where they're much narrower. They have a much narrower curriculum, whereas chemical engineers have a very broad curriculum. They learn all sorts of things that the other engineers don't, especially, of course, chemical things. And also, it instills in the minds of our students that they could really do anything, given a little help and given a little time to read and so on. Our students are not afraid of anything. If somebody comes in and says, "We'd like you to do this and this and this," and this was I never learned anything about it, then some students would say, "I can't do that; it's out of the question." Well, our students don't do that. They say, "Well, no, I don't know anything about that, but I'll learn it. I'll learn to do it." So it's that attitude which we try very much to encourage, and we do, and with good results. Our students are not afraid of anything. They'll do whatever it is and learn how to do it. We're very proud of that.

01-01:26:25
Burnett: Well, I understand that a large percentage finish their studies, that their terminal degree is the Bachelor of Science.

01-01:26:35
Prausnitz: Oh, yes.

01-01:26:37
Burnett: And they go into industry, but there's such demand. But you didn't. Can you talk a bit about the pursuit? Did you know you wanted a PhD, and at what point did you decide you wanted to do that?

01-01:26:48
Prausnitz: I think in my final two years at Cornell I realized that I was really more of an academic personality, and I would be probably much happier in university, rather than working for some big chemical company. I think I realized that toward the end of my Cornell days. And I knew in order to do this I would need further training. I knew that in industry it's the mighty dollar which is the important point, and everything else has to give service toward that point. I knew that, of course, and I didn't like that. [laughs] I mean, I wanted to do science, and I wanted to help increase knowledge, and sure, I wanted to produce something that would be economical, would be used. It wouldn't just be an article in the literature; it would actually have some consequences. But subjugating everything to the dollar, which is what you have to do in industry,

is why I didn't much like that. And I loved the university, as I still do. I love living in the university, and I thought that's really where I would want to go. I wasn't sure, but anyway my thinking was in that direction. And I enjoyed graduate school. I really loved it. I was twenty-two years old when I got my bachelor's, and I knew I was still pretty green behind the ears. I didn't really know much, and no matter where I would go to work I felt unprepared. I needed more instruction.

One regret I have is that I didn't use my summers to get more experience. I would go off to boys' camp, where I was a counselor, and that was very nice, but that, of course, doesn't do anything for my professional life. I did this just about every summer, and that was stupid. In hindsight, I should've done something more useful in summer. But I loved horseback riding, and I still do, although now I don't go horseback riding anymore. And I was a horseback riding counselor in the boys' camp, and I enjoyed that very much. I loved riding, and it was— And I made all the boys—oh, it was a boys' camp; there were no girls—I made them always get on the horse for the first time without a saddle. Have you ever gone on horses?

01-01:29:34
Burnett: Yes.

01-01:29:34
Prausnitz: You know about it. Going on without a saddle, like the Indians did; the Indians didn't have saddles. And you learn how to use your knees so that you don't fall off, and only after you learn how to do that, then you could use a saddle, an English saddle. "Western saddle," I consider that a motorcycle. [laughter]

01-01:29:58
Burnett: An English saddle is better in what way?

01-01:30:01
Prausnitz: You've got to work harder. It's real exercise, especially when the horse stops. You have to pull, whereas on a Western saddle you sit there, hold onto the horn. That's not a sport; that's a motorcycle. Have you done English saddle?

01-01:30:20
Burnett: No. No.

01-01:30:23
Prausnitz: You do Western saddle, then.

01-01:30:24
Burnett: Yeah. My experience on horses was very, very brief, [laughs] so I can't claim any kind of expertise at all. But it occurred to me that perhaps what you're drawn to is the cycle of learning and teaching, that you're—

01-01:30:43
Prausnitz: Oh, yeah. They go together.

01-01:30:45
Burnett: —that that's what drew you in.

01-01:30:47
Prausnitz: They get to go together, and the best way to learn is to teach. Well-known: if you teach a subject, you've really got to understand it.

01-01:30:55
Burnett: Right. Well, perhaps we should pause for now, and we'll take up next time.

01-01:30:59
Prausnitz: All right.

Interview 2: November 14, 2018

02-00:00:14
Burnett: This is Paul Burnett, interviewing Dr. Prausnitz, for the University History Series, and we are here on the campus of UC Berkeley, and this is our second session, and it is November 14, 2018. Welcome, Dr. Prausnitz, again.

02-00:00:31
Prausnitz: Honored to be here.

02-00:00:32
Burnett: So last time we got about as far as your finishing your undergraduate at Cornell. And then you did a master's right away at Rochester. Can you talk about that transition, and then moving into your PhD experience?

02-00:00:50
Prausnitz: Yeah, it was quite different, because Rochester is a very small university compared to Cornell. The number of students is maybe just a fraction of the number that was at Cornell. And the Chemical Engineering Department at Rochester was very small. There were only three or four faculty, and there were very few graduate students. So I liked it there very much because everybody knew everybody else, so it was much more of an intimate atmosphere. And I spent a year there, because my fifth year at Cornell, a lot of that was credited toward my master's degree. So it only took two semesters, which is generally short for a master's. And town of Rochester, I thought, was very interesting, because they had a lot of music. The Eastman School of Music is there, and I would attend their concerts.

So I had a very good time there, and it was important that I went to University of Rochester, because I got very good recommendations from the professors there for my subsequent application at Princeton. At Cornell, very few of the professors really knew me. They sort of superficially knew who I was, but the faculty at Rochester, they knew me quite well, because we interacted on a more personal basis. The professor I worked with was a man named Su, a Chinese who had been in America many years, Hu-Xian Su. And I enjoyed very much working with him. He had a very good sense of humor. And I do remember a very delightful evening at his house when he and his wife prepared a superb Chinese dinner. At that point I didn't know anything about Chinese food, so that was one of the things I learned from Professor Su. The thesis was in the general chemical thermodynamics area, nothing very exciting, but it was good for my education.

And I lived in a private home. I was able to rent a room not far from the campus, and when I look back now it's absolutely amazing. It was a family with a father, mother, and a son who was about ten or twelve years old. And I had a nice room, a small room, but it was perfectly sufficient. And it's absolutely amazing when you think back: I paid eight dollars a week for this room, and it included breakfast, and it included laundry service. So it's just absolutely amazing. [laughter] But that was good, because I was a teaching assistant at Rochester, and, of course, the salary of a teaching assistant was very low.

02-00:04:03
Burnett: Did you have any kind of scholarship at Rochester? I guess the teaching assistantship is that.

02-00:04:08
Prausnitz: No. No, the teaching assistantship. So I didn't have to pay any tuition, and I got a small stipend, just about enough to live on. But another thing that happened at Rochester is I bought my first car. It was, of course, an old, used car, but it was, of course, very thrilling to have my own car for the first time.

02-00:04:32
Burnett: Do you remember the model?

02-00:04:33
Prausnitz: Yes, it was an old Plymouth, prewar Plymouth. So this was 1950, '51, and the Plymouth was from the thirties, so it was a real old car. But I loved it, of course. And so that was also part of my education, trying to get the car to work properly. [laughter] And I would buy used tires because I couldn't afford new tires. So I had a very, very good time there, but, as I say, from a point of view of my education, it was important that I went, because the letters of recommendation that I needed for Princeton were very good. And so I was accepted at Princeton without any difficulty.

02-00:05:18
Burnett: Can I ask, to interrupt for a moment? In your 1963 paper, in the *Journal of Engineering Education*, which we'll talk about a little bit later, you talk about this shift from classical thermodynamics to molecular thermodynamics over the previous thirty years. Can you talk about that transition with respect to your own education? Was that taking place at that time?

02-00:05:50
Prausnitz: Yes. Well, that took place much earlier. I was at Rochester in 1950, '51, and this paper was in 1963, so that's twelve years later. No, at Rochester I particularly enjoyed a course in physical chemistry for graduate students, and that was really a very good course. It was taught by a professor who at that time was the editor of the *Journal of the American Chemical Society*. And he would give us manuscripts that he had received from people who wanted to publish, and ask us—it was a small class—ask us to review the manuscripts prior to publication. And, of course, that was really unique. It was a superb education. So that I became acquainted with what you might say was the frontier, the cutting edge of physical chemistry, and that made a tremendous impression on me, and I began to realize that thermodynamic properties follow from molecular properties. If you understand what the molecules are doing, then you can make some predictions about what the thermodynamic

properties are. I'd never learned that at Cornell. It was really at Rochester that I first got that insight.

And the other thing that I really enjoyed there was my teaching. I would give classes, strictly voluntary, for the undergraduate students prior to their exams. They would have a midterm in physical chemistry, or midterm in some chemical engineering subject, and I would hold review sessions. I wasn't required to do that. I just did it on my own, and I enjoyed doing it, and then, of course, as is well known, when you teach something you really learn it, and that was certainly true. To prepare for these review sessions I had to learn the material thoroughly. So that was very, very good. Again, I look back upon my Rochester year quite happily.

02-00:08:01
Burnett: We'll talk about teaching throughout this set of sessions, but do you find that teaching stretches you—

02-00:08:12
Prausnitz: Oh, absolutely.

02-00:08:12
Burnett: —pulls you into different areas that you didn't know much about?

02-00:08:15
Prausnitz: Oh, of course. I learned all sorts of new things. No, teaching is the best way to learn, there's no doubt about it. [laughter] This is well known. This is nothing new, but it was sort of new to me. I didn't know about this. But I found by my experience that if I try to explain a subject to someone else, I really have to know what I'm talking about.

02-00:08:37
Burnett: Well, how do you explain—? There are plenty of scholars and faculty who dislike teaching.

02-00:08:44
Prausnitz: Yes. [laughter] Yes, and Einstein was one of them, in fact. Yes, I know that, and I find that very difficult to understand, because my whole intellectual outlook requires that teaching be a part of my research. Now, teaching has to be taken here in a very broad way. It doesn't mean necessarily that you stand in front of a class at the blackboard. That's one form of teaching, but there are other ways of teaching, and that is more of the apprentice system, where somebody works with you on some topic and you have discussions about it, and you try to alert the young student about various topics and dangers and things to do and things not to do. So that's much better teaching. And I've always found that I do my best teaching in my office. I have a blackboard, and a student comes in, and then we talk about some problem, and I gently lead the student around. So, what about this? And what about that? And what do we know? And what do we not know? And are there any relations between what we know and what we don't know? And so by questioning, the student

gets what's called the "a-ha" experience, and, of course, that's very rewarding for me when a student says, "Oh, oh, that's how it works."

02-00:10:18
Burnett: But that's effective, one-on-one education.

02-00:10:20
Prausnitz: That's one-on-one. It could be two-on-one, but certainly not more. But I saw that in action many years later when I was at Oxford, where they have the tutorial system. And, of course, it's expensive. You can't really do this when you have mass education. But I've always done that throughout my years here at Berkeley. The big problem is to get the student to come in. Students are very reluctant. They're afraid to show their lack of knowledge, afraid to show what they call their stupidity, which is ridiculous. They're not stupid. They wouldn't have gotten into the university if they were stupid. They're not stupid at all. They're just inexperienced, and often they're not sufficiently motivated. That's a very big problem. But once you get the student into your office and get them up to the blackboard, then you've already gone halfway. You're already halfway at the goal. But, as I say, that's often difficult, and when the student says, "Well, I'm reluctant to come because I really don't know anything, and I don't want to waste your time," and so on, "and I don't want to ask stupid questions," I said, "You can not only ask stupid questions; you should. That's why you're here." And I always say to students, "You cannot shock me. No matter how stupid your question is, I've heard worse." Well, that's true, but they don't always believe it.

02-00:12:00
Burnett: That's it. I think that a lot of students don't feel worthy of your time, and that's always a problem. I also wonder how much of that one-on-one teaching is required. Is it transformative? Can you have one session with one student and it changes the way they approach learning?

02-00:12:18
Prausnitz: Yes. Absolutely. Absolutely. It makes a tremendous difference. And I've had many cases where a student is very unsure of himself or herself, and then by being here for a half hour or so, he or she begins to see, "Well, maybe I can really do this; maybe it isn't so difficult after all. I can do it. I'm up to it." And, of course, that's half the battle, if you can get the student to recognize that. And yes, I've transformed a lot of students who thought they weren't able to do anything, and they weren't good enough to go to graduate school, and I've shepherded them and said, "Of course you're good enough to go to graduate school. You won't be accepted by MIT, but you can go to a perfectly good place." And they do, and we've had a lot of success.

02-00:13:16
Burnett: Right, right. And that is a problem of mass education, especially if people come with different levels of ability and different levels of training, and they are lumped into the same class, and they see that other people are getting it

and they aren't, and they draw conclusions from that that are erroneous. And part of what you're doing is having an open door so that those who feel like they're not getting it can have that transformative experience, and can see that they can get there, but it requires a certain application of effort, and a certain style of thinking, maybe?

02-00:13:53
Prausnitz: Well, the main thing is self-confidence, and many, many students lack it. And if you can show them that they're not as stupid as they think they are, [laughs] they're really not stupid at all, they just have to look at the problem the right way and then they can solve it.

02-00:14:11
Burnett: Right, right. And so you did have some transformative experiences, both as a student and as a novice teacher at this point, and teaching was key for you, even at that early stage.

02-00:14:27
Prausnitz: Absolutely, and that convinced me that I really wanted to have an academic career, because I obviously enjoy doing this.

02-00:14:34
Burnett: That fits into another question, too, about path, because one assumes that there is a better living to be had working in industry as a chemical engineer, right? [laughs]

02-00:14:46
Prausnitz: Oh, definitely. That was definitely true at the time. I think now that difference is much smaller than it was. But certainly academic jobs at that time—this was in the early fifties—were paid very poorly. The salaries were very, very low. Now, it's much better.

02-00:15:07
Burnett: Yeah. It was a vocation. It was a calling.

02-00:15:10
Prausnitz: Right. You had to do it because you loved to do it.

02-00:15:12
Burnett: Right, right, absolutely. So one happy outcome of the Rochester experience was the letters of recommendation, as you said.

02-00:15:23
Prausnitz: That was very important, yeah—

02-00:15:24
Burnett: And that enabled you to attend—

02-00:15:24
Prausnitz: —and that got me into Princeton.

02-00:15:27
Burnett: Did you have an idea of whom you wanted to work with at Princeton? Was there a particular—? Were you recommended, "You have to work with this person"?

02-00:15:39
Prausnitz: I don't remember if anybody recommended, but it was clear to me when I got to Princeton that there was one personality there who was clearly ahead of all the others. His name was Richard Wilhelm, and he was clearly the leading intellect of the department, and so I wanted to work with him, and fortunately I was able to do so. And I have no regrets. It worked out very well. And it had nothing to do with thermodynamics. We worked on the chemical reactors, and I did a thesis on concentration fluctuations in a packed bed. So thermodynamics really have nothing much to do with it at all. But I have no regrets. It was very good training. I learned a lot. I learned experimental techniques, and also I learned how to write an article for publication. Professor Wilhelm was very generous with his time, and I enjoyed working with him. He, unfortunately, died early. He wasn't even seventy. Was he even sixty? I don't even think he was sixty. I think he was in the late fifties. And in retrospect, that was not too surprising because he was very heavy. He was really badly overweight, and very intense, so it was really not too surprising that he had a heart attack. But it was a big loss for chemical engineering.

02-00:17:21
Burnett: He was a star at that time. He was department chair for one thing, and he had won the Walker Award.

02-00:17:31
Prausnitz: Yes. Oh, he won all sorts of awards, and very important for my future, he was a consultant for Shell Development Company. And Shell Development Company at that time was in Emeryville, which is part of the San Francisco Bay Area. And because of this connection, I came to California, because he recommended me to Shell, and Shell invited me for an interview, and I had never been to California before. This was totally new to me. And so when I came to California—I think it was 1954—on a visit, I immediately fell in love with it and wanted to stay here.

02-00:18:20
Burnett: And this was before the courtship of Berkeley, or the courting of Berkeley?

02-00:18:25
Prausnitz: Yes, this was before. I came in I think it was 1954 for an interview with Shell, and I asked the Shell people if I might visit the University of California at Berkeley and if I might visit Stanford, and they generously said, "Okay, you can do that." But they also said that under no circumstances was I supposed to go to Chevron. That was out. [laughter] But your two universities, well, that was okay. So I went to, first, Stanford, and I spent a day there, and I liked it very much, but chemical engineering at that time at Stanford was minimal. It was a very small program. They only had two faculty, and the physical

facilities were extremely limited. So while Stanford was a beautiful place, chemical engineering at that time was very, very tiny.

02-00:19:31
Burnett: Before we continue, can you draw us a little picture of the state of chemical engineering in the mid-1950s? I imagine because of its association with the petrochemical industry, and the chemical industries in general, that it's going to be an East Coast phenomenon. It's going to be MIT, and there might be some satellite operations in Texas. I don't know. What's the map of the United States with respect to chemical engineering?

02-00:20:02
Prausnitz: Well, chemical engineering was very closely tied to petroleum and petrochemicals. There was almost nothing in inorganic chemicals, very little. There was almost nothing in electrochemistry. The emphasis was definitely on the petroleum industry. There was nothing about food. There was very little about polymers.

02-00:20:28
Burnett: Mining?

02-00:20:29
Prausnitz: There was something about mining, but that was sort of peripheral. So the textbooks that we used also emphasized more the petroleum application. The standard book in those days was a book by Lewis, Warren K. Lewis, first came out in 1923, then there were later editions. And Lewis was a longtime and very successful consultant for Exxon. At that time it was called Esso, not Exxon. And, of course, that was petrochemicals and petroleum, and that's reflected in the book. So that's where the emphasis was. The leading schools at that time were MIT and also the University of Wisconsin. They were, I think, the most highly respected programs in chemical engineering. And then the University of Delaware soon came after that and became quite prominent. But MIT was sort of the leader.

02-00:21:40
Burnett: Is there an industrial linkage with Wisconsin? Is there any industry? Was there a company facility near Madison, or—?

02-00:21:51
Prausnitz: I don't really know. I can't say at that time. Later on, certainly, there was, especially with paper companies, but I don't really know how it was in the fifties, and the industrial connections, I think, came later. But there was a certain rivalry between Princeton and Wisconsin. At that time, there was a series of books by Hougen and Watson. Hougen was the Chairman of Chemical Engineering. And he was certainly one of the great leaders of chemical engineering education.

02-00:22:36
Burnett: And he was at Princeton, or—?

02-00:22:37
Prausnitz: No, he was at Wisconsin, and there was sort of an artificial rivalry between Hougen and Wilhelm. They worked on similar problems but in quite different ways. Wilhelm was much more fundamental.

02-00:22:59
Burnett: That had its influence on you, I guess.

02-00:23:01
Prausnitz: Oh, it had a tremendous influence, yes. And then, of course, in 1960 came the book by Bird, Stewart, and Lightfoot, who were the people at Wisconsin [Edwin N. Lightfoot, Robert Byron Bird, and Warren E. Stewart. *Transport Phenomena*. New York: Wiley & Sons,1960]. And that changed chemical engineering education tremendously, for years. And only in the last, oh, ten, twenty years have we gotten away from that, and, oh, it's these articles which say, "Transport phenomena: have we gone too far?" [laughter] And the answer is yes, we did go too far. It was a great influence, it was very good, but it meant that other things in chemical engineering were neglected. And so there's been a rebalancing in the last ten or twenty years.

02-00:23:53
Burnett: But at that time—so this is interesting from just—if we can place ourselves back in that moment in the mid-1950s, at this point that BSL text is just a classroom exercise. It hasn't—

02-00:24:15
Prausnitz: Yeah, that's right.

02-00:24:15
Burnett: —even 1957 or so.

02-00:24:17
Prausnitz: It was the practice at Wisconsin but nowhere else, until the book was published.

02-00:24:23
Burnett: Right. And so just from your perspective, you were thinking—and there was a strong recommendation from your advisor, Wilhelm—that you talk to folks at Emeryville, in the Shell Development Corporation, and talk about becoming a consultant there. And that conversation went well? Did that result in something, or was it a stepping stone to Berkeley?

02-00:24:55
Prausnitz: Well, it turned out it was a stepping stone, but yes, I interviewed there, and it was very nice. I enjoyed it, and a week or two after I came back to Princeton I had an offer of employment. They offered $6,000 a year, which was pretty good at that time. And I also had an offer from Stanford, just on the basis of that one afternoon that I spent with them, and that was $5,000 a year. And then Berkeley also came through with an offer, and that was $5,100 a year. [laughter] So I had a real choice between those three places, and I had to think

about that quite a bit. But I finally decided Berkeley was my preference, not because of that extra $100, but because Berkeley already had a modest but going chemical engineering department. They had about six, seven faculty at that time, and they had a lot of students, which was not true at Stanford; the number of students in chemical engineering was very small. And Stanford didn't really get going until several years later. Chemical engineering was almost negligible at Stanford, until they hired Professor David Mason, who came from Caltech, and Mason really started the department there. And within five or ten years, the Stanford department became one of the leading departments in the country, and certainly is one of the top departments now. But that took quite a while. So Stanford really lagged in time. Berkeley was ahead in time.

02-00:26:52
Burnett: Well, I imagine—and you can confirm this or not, but—at that moment Berkeley was top of the world in terms of chemistry, generally.

02-00:27:07
Prausnitz: Yes, and it has been. It's remained there. And that, of course, was a very important point that I greatly preferred. At Stanford, chemical engineering was part of the Engineering School, and still is to this day, but here, of course, it's part of the College of Chemistry, which is a most unusual arrangement. We have good relations with the College of Engineering, but we don't really have very much to do with them. There's some connection, but not much, whereas the connection to the Chemistry Department is very strong. And one reason for that is just proximity. The Chemistry people are just a two-minute walk away; the Engineering people are about, oh, a six- or seven-minute walk away. [laughter]

02-00:28:00
Burnett: That's important, isn't it? The situation—

02-00:28:02
Prausnitz: Location makes a tremendous difference. It really does.

02-00:28:04
Burnett: Can you talk a little bit about the unique circumstances of the situation of chemical engineering in chemistry at Berkeley? This was not an accident; it was the consequence of a struggle over where it would be, and what it would be.

02-00:28:27
Prausnitz: Right.

02-00:28:28
Burnett: Do you remember what the stories were about that?

02-00:28:33
Prausnitz: Yes, most of that happened before I came. Chemical engineering started as a sort of option, a division within the Department of Chemistry, and it started in

1946, and remained quite small for a number of years. And there was a dean of engineering—his name was O'Brien—who was very anxious to have Chemical Engineering in the College of Engineering, and there was a sort of a struggle between O'Brien and the Dean of the College of Chemistry, whose name was Kenneth Pitzer. Clark Kerr was the first Chancellor at Berkeley. This was 1952 or 3, and one of the first things that Kerr had to do was to decide where Chemical Engineering was going to be. And he said it was very easy for him to make that decision because the students had voted with their feet. I remember that was his expression. The number of students who took Chemical Engineering in the College of Engineering was very small, whereas the population here in the College of Chemistry was much larger. So that was one of the compelling reasons for Clark Kerr to decide that Chemical Engineering was going to be in the College of Chemistry, not the only reason but certainly that was part of it. And so chemical engineering was not called chemical engineering in the College of Engineering; it was called Process Engineering. That died out, and they were able to get other positions for their faculty. Some of them went into the new Department of Nuclear Engineering, and I think others were taken up by Mechanical Engineering. So no one was thrown out, but some left voluntarily. So by 1960 it was all over, or even earlier. I think 1958 is when the Chemical Engineering Department was founded here, in the College of Chemistry. So it was all over by then.

02-00:30:59
Burnett: Right, it had been resolved. So there was no negative fallout from that, or was there any cost to establishing it in chemistry, or was it all an upside, was it all a benefit?

02-00:31:17
Prausnitz: Well, depends on your point of view. [laughs] I think it was all benefit, yes, because the Chemistry Department, as you mentioned earlier, was very strong, and we had very good connections with the Chemistry people, which had a big influence on the academic program here. And at that time, College of Engineering was not nearly as distinguished as it became later. It's now one of the leading colleges of engineering in the world, but that was not true at the time. And so the Engineering people were much more down-to-earth, empirical. Not anymore, but at that time they were much more, I would say, non-intellectual than the people in Chemistry.

02-00:32:06
Burnett: It is striking just to do a rundown of important figures, Nobel Prize winners, when you arrived there in '55, the people who are around: Glenn Seaborg; G.N. Lewis; Edwin McMillan, who discovered neptunium—

02-00:32:23
Prausnitz: William Giauque.

02-00:32:25
Burnett: William Giauque.

02-00:32:26
Prausnitz: Yeah, let's not forget William Giauque.

02-00:32:27
Burnett: Chemical thermodynamics.

02-00:32:29
Prausnitz: Yes, that's right. He was the first Berkeley chemist to win a Nobel Prize. That was 1949, and it was for chemical thermodynamics, right. And then Melvin Calvin—I don't know whether you mentioned him—

02-00:32:42
Burnett: Yes, photosynthesis in 1961, and John Northrop, whose work on enzymes, although that's mostly at the Rockefeller Institute, I guess.

02-00:32:49
Prausnitz: Yes, we didn't have much to do with him, no.

02-00:32:51
Burnett: And Joel Hildebrand is around. He's emeritus in '52, but he is—

02-00:32:57
Prausnitz: He's here. He was active until about age ninety-nine. So he was, of course, one of the main reasons why I wanted to come to Berkeley. So I interacted a lot with Joel Hildebrand, and that was a very good influence.

02-00:33:15
Burnett: Can you talk about that? What was his reputation? Why were you drawn to working with him?

02-00:33:20
Prausnitz: Well, that was a rather interesting detail. At Princeton at that time, as part of the PhD requirement, you had to present ten propositions. A proposition meant a proposal for a research project. You don't do the research project—you just propose it—but you have to be able to defend it. You have to be able to say why this is important, and how you would do it, and so on. So as a result of this requirement, like everybody else, I went to the library, looked around to see some interesting topics that I could propose research for. And I don't remember how this happened but I found this book by Hildebrand, and his co-author Robert L. Scott. And it was the third edition of a book called *Solubility of Nonelectrolytes*. And I found that extremely interesting, and I really enjoyed that. And I think one or two of my ten propositions were based on material that I had learned from that book. There was also another book called *Mixtures* by an English physical chemist called Guggenheim. That also had a big influence on me. And so because of this proposition system at Princeton, I found out about Hildebrand's work. I don't know if they still have that system—I hope they do—but I found that that was extremely good, because it got you out into the library to learn about new things. The rule was that out of your ten propositions, two, maybe three, two or three could come from your thesis, but that's all. The others had to come from other areas, and if

you wanted you could include some philosophical propositions. It didn't have to be science. So that was a great inducement to spread out and learn something about the world.

02-00:35:46
Burnett: So Princeton wanted its graduates to be broad-gauged. They wanted them to engage in the—and isn't that the motto of Princeton is to engage in the world [Princeton in the nation's service and in the service of humanity]?

02-00:35:54
Prausnitz: Yes, very definitely that was the motto then. I don't know if it still is. I hope so. But I certainly benefited a lot from that. And we had a similar system here at Berkeley when I first came. There were propositions that you had to present at the time of your qualifying exam. Unfortunately, that's all gone. We don't do that anymore. The examination now is primarily on your thesis, and I think that's a mistake. I don't like that one bit. We don't do enough to broaden our students. And the reason for that, I think, is pretty clear. We don't publicize it, but I think the reason is well-known. The reason is that when you are a graduate student, and you work with professor so-and-so—I'm exaggerating—but to some extent professor so-and-so owns you. He wants you, the student, to spend time working on the thesis, because the thesis is then later on published, and the publication is essential to get research funds. So one reason, not the only reason, why the proposition system at Berkeley died is because of monetary pressures. The usual rule in America if you want to understand something is "follow the money," and boy, that sure is true here. Now, it's not the only reason. There are other factors. I don't mean to say it was strictly that, but that, I think, was the determining factor, that the student is supposed to produce, and anything that takes him away from producing is frowned upon and eliminated. Very unfortunate. I think it's very sad, but it's understandable.

02-00:37:56
Burnett: Yes. The students themselves are under such economic pressures with the rise of tuition and things like that. We should talk about that maybe in our last session, talk about how things have changed. But on the subject of following the money, I was wondering—and I don't know if you learned anything at the time when you went to Emeryville, Shell Development Corporation, but I'm wondering about the political economy of the petrochemical industry. Is there something about World War II and the development of petrochemical facilities on the West Coast that they needed more programs and more brainpower out of the West Coast? Or is it just a consequence of the growth of academic and industrial linkages on the West Coast, period?

02-00:38:52
Prausnitz: Well, no, I don't think it's the West Coast. I think it's the history. Now, Shell, as you know, was originally a company from Holland, and also England. To this day—

02-00:39:06
Burnett: Royal Dutch [Shell].

02-00:39:06
Prausnitz: —the headquarters, the royal headquarters, are in the Hague in Holland. And the Dutch tradition was very much, I would say, fundamental. They believed at that time—I think they still do—they believe in research, in rational research. Now, you can contrast that with Chevron. It wasn't called Chevron in those days. It was called—

02-00:39:33
Burnett: I think Esso.

02-00:39:34
Prausnitz: —Standard Oil of California. But anyway, completely different philosophies at that time, and I think it's probably still true today, to some extent. Chevron was interested in much more empirical knowledge. Just do something to make it work. Never mind how you do it; just do it. And that was not the situation at Shell. The situation at Shell was more, well, let's look at the fundamentals. Why is this occurring? What is the basis of the phenomenon? And so on. And so they had very good scholars at Shell, mostly physical chemists and analytical chemists, who worked out the fundamentals of the problems that they were considering, whereas at Chevron it was more, "quick, quick, quick, let's get something that works, never mind why, that isn't so important." At least at that time. I think now it's probably different. And when one talked to the Chevron people and said, "Look at how Shell is doing it now, you're doing it—," they would always come back and say, "Well, we make just as much money as they do." In fact, maybe even more. I don't know. So they were just strictly oriented [toward] how much profit can we make. Again, this may be different today, but at that time there was a very big difference philosophically between the two places. So I don't think it's the West Coast. I think it's the tradition of the company.

02-00:41:10
Burnett: Of different companies. It sounds like that mirrors some of the transition between the old chemical engineering and the newer chemical engineering. Can you talk about what chemical engineering was, say, in the 1920s? From a scientific perspective, what did you do in chemical engineering?

02-00:41:32
Prausnitz: Well, in chemical engineering what you would do is you would take some product that some chemists have made in small quantities, and you would make into big quantities. It was a scale-up. And the easiest way to see that is the following: a chemist makes some product, some chemical product, in a test tube. He makes maybe a few grams of the material. Well, now it turns out that there's a market for this chemical, often a big market, so we need not grams; we need tons of this material. Now, what can you do? Well, you can say, okay, if I need tons and each chemist makes a few grams, I can line up a thousand chemists, and each chemist makes a few grams in his test tube, then

we put it all together, and then we have a ton. Well, clearly that's highly, highly inefficient, and so you have to devise other methods for making this chemical, and that's what chemical engineers did, and still do today. But that was the essence of what chemical engineers were doing in the earlier days. Chemical engineers at that time were not oriented toward making products; they were oriented toward making a process. The product was already fixed. There was nothing to be changed. The chemists had made the product, and that was it. The question chemical engineers had to answer: how do we make this product economically in very large quantities? And to some extent that is still true today, but much less so. Now chemical engineers are equally interested in the product, just like chemists are.

02-00:43:28
Prausnitz: Chemical engineers today are product-oriented, toward new products, which was certainly not the focus in the earlier days. The focus in the earlier days: how do we scale up? How do we take something the chemist has made and produce large quantities? That was process design.

02-00:43:51
Burnett: Right. And I guess the basic advantage of industrial chemical processing is that it's tremendously energy-intensive, right? So you have this feed of some kind of energy source to do something physically and chemically to a base product that has many different chemicals in it, I suppose, and that the trick is you want a continuous process by which you can extract relevant products from it at different stages, so something like a fractionation column. So is that something—

02-00:44:34
Prausnitz: Well, that's certainly true in petroleum. I think that's generally correct, but what you try to do now, of course, is to do a process with much less energy, and with so-called green chemistry you want to do it in such a way that you don't hurt the environment. There's also the question of what raw materials you use. There are various raw materials that are available which you should use. That's often a major problem. But nowadays chemical engineers are increasingly concerned with biological products. That was hardly the case years ago. And they're concerned with developing new products, and earlier that was not chemical engineering; that was left to chemists.

02-00:45:33
Burnett: And you've talked about this again in your *Education* article from '63, but in the old chemical engineering, it was some cut-and-try methods, that by trying out experimentally a method, it worked, and it was not necessary to explain why it worked the way it did, and you had classical thermodynamics, for example, to talk about thermodynamics, to explain what was happening. And those are relatively rough compared to the methods that were developed later. Can you talk about how far you could get with classical thermodynamics with these kinds of processes?

02-00:46:17
Prausnitz: You could get very far, but you need a lot of data. Thermodynamics is not a magic wand that gives you an answer. Thermodynamics, chemical thermodynamics, helps you to organize experimental data and to build a hierarchy of data, and then you can, on the basis of the data and thermodynamic equations, you can make some predictions. But without a lot of data, you can't do much. I had one interesting experience. Oh, this must be thirty years ago, forty years ago. I was called up by some industrial firm—I don't even remember what the firm was—and they said, oh, they had a problem of some sort, and could I help them with this problem. And I said, "Surely, I'll be glad to try." And I said, "Now, what experimental data do you have?" And then there was a long pause. And the other person said, "Experimental data? We don't have any experimental data. If we had data, we wouldn't be calling you." [laughter] In other words, there was this idea that thermodynamics was a magic wand, and then the numbers you want suddenly appear. There's still some people who think that way today, not many but some, and of course that's totally wrong.

02-00:47:47
Burnett: Right. But we're sort of inching towards this—I don't know if it's too strong to call it a paradigm shift, but this movement towards a kind of molecular way of thinking.

02-00:48:03
Prausnitz: Oh, yes.

02-00:48:04
Burnett: And so, looking over some of your papers, which we will get to later, there is this explanation that the classical thermodynamics was a powerful tool but it leaves out the behavior of the molecules that have, at a scaled-up level, have a tremendous impact. And if you understand some of those processes at the molecular level, rather—then you don't necessarily need as much data.

02-00:48:47
Prausnitz: That's right.

02-00:48:47
Burnett: You can operate from first principles.

02-00:48:48
Prausnitz: That's right, that's right. You can operate—well, first principles is maybe too much, but you can establish correlations which have a physical basis, and on the basis of these correlations you can make predictions. In other words, a molecular approach greatly reduces the amount of experimental work you have to do.

02-00:49:13
Burnett: So you're at Berkeley as an assistant professor in 1955.

02-00:49:24
Prausnitz: Fifty-five, yes.

02-00:49:25
Burnett: And surrounded by an extremely strong college of chemistry, with people working in every different domain, with Lawrence Berkeley Laboratory up on the hill, and really exciting avenues. Did you feel excited about the possibility of branching out, of drawing on some of this expertise? Was it set up in a way that you could do that?

02-00:49:58
Prausnitz: It was set up in a way that I could do that, but the initiative had to come from me. Nothing was offered on a silver platter. You had to go get it. It's not true that all areas of chemistry were represented here. For example, polymer science: when I first came here, there was almost nothing on polymer science, because I think chemists felt that polymers were sort of hard to characterize. They were sort of messy. They would much prefer to work with nice, simple molecules, especially spectroscopy. Spectroscopy was the big thing, and with polymers that was just too complicated. They didn't want to be bothered with that. And also, the attitude in the Chemistry Department was that they were not interested in applications; the interest was in pure chemistry. You had to learn things, study things for their own sake. That's, of course, completely changed now, and one reason it's changed is because of financial pressures. If you want to get financial support for your work, you have to show how it relates to the world, and people make up all sorts of interesting, [laughter] fantastic stories to answer that question. But today, applications are considered not only essential in many cases, but they're respected. In the middle and late fifties, if you did applications, that was somehow a lower level. And there was a lot of that attitude toward chemical engineers here. We were called plumbers, not openly, but sub-rosa. There was a lot of feeling on the part of the chemists that we were really not up to what they were doing. We were doing something a lot lower.

I remember very distinctly one case where we were going to hire a new professor, and we looked at his advanced qualifications, and we thought he was really very good. And on the committee looking at his qualifications was a professor of chemistry. And he said quite openly at the committee meetings, "Yes, this candidate is probably okay for chemical engineering. Of course he's not nearly good enough for chemistry, but for you chemical engineers I guess he'll be all right." I'm not saying the exact words, but that was the gist of what he was telling us. So that attitude was quite common in the early days.

And I recall my first lecture—once a week we had a college-wide seminar series. That went out long ago, and now we have seminar in organic chemistry, physical chemistry, nuclear chemistry, etc., etc. In those days, we had a seminar in chemistry, period. It was called Seminar in Chemistry and Chemical Engineering. And this was after I'd been here about a year or two, and I talked about some chemical work I was doing, some solubility, maybe liquid equilibria or something like that. And I remember at the end of my talk I said, "I hope that the audience appreciates that in my talk I am probably the first chemical engineer who has not mentioned the Reynolds number," and there was wide applause. [laughter] All the previous talks by chemical engineers always talking about flow of fluids, and fluid mechanics, and it was the Reynolds number, and so the Reynolds number became sort of a tag. Chemical engineers were tagged with the Reynolds number, and I didn't mention the Reynolds number, and people really liked that.

02-00:54:43
Burnett: What did it signify for them? That you were just plugging in a constant, and it's not imaginative or creative or interesting?

02-00:54:51
Prausnitz: Right, right. It's just plumbing. That disappeared, of course. By now that's completely gone.

02-00:55:00
Burnett: But you were completely aware of this in making that joke.

02-00:55:02
Prausnitz: Oh, I was totally aware, yeah.

02-00:55:04
Burnett: And opening with it, and relaxing people, and—

02-00:55:06
Prausnitz: Totally aware of it, so it was a self-serving joke, but nevertheless I couldn't resist. [laughter]

02-00:55:15
Burnett: So you wanted to signal to them that you're a new kind of chemical engineer, that you are deeply passionate about fundamental processes.

02-00:55:21
Prausnitz: That's right.

02-00:55:24
Burnett: So what was your first task, then, as a young assistant professor, out there to prove yourself? How did you want to make your mark as a chemical engineer who was interested in fundamental physicochemical processes and concepts?

02-00:55:46
Prausnitz: Well, I followed, more or less, along the lines of Hildebrand. I was definitely a disciple of Hildebrand. And also, there was—still is—a prominent physical chemist who's not a member of the faculty but who is at Livermore. His name is Bernie Alder. And Bernie Alder and I became good friends; we still are to this day. And Bernie did a lot of computer work, computer simulations, on fluids. And I used some of his results to describe thermodynamic properties of

fluids. So that was another big help in getting me started here, and making me respectable. [laughter]

02-00:56:45
Burnett: Well, so Hildebrand is still around. He's emeritus in the fifties, but he's still a force.

02-00:56:51
Prausnitz: Oh, yes, still active, still active.

02-00:56:54
Burnett: So when you said that you drew from his research, did you seek him out physically? Did you go to see him?

02-00:57:00
Prausnitz: Oh, yes, I had many discussions with him, and he and his coworker, Robert Scott, who was a professor at UCLA, the three of us wrote a book—not a big book, a small book—in 1972 or something like that, which talked about the various correlations that he had made, and correlations that I had made. So I was quite close to Hildebrand. And he had some postdoctoral people working with him. Bernie Alder was also involved. And so I was an unofficial member of that group. I knew the postdocs, and they certainly influenced my thinking.

02-00:57:50
Burnett: And he was supportive of— So, by contrast, I think G.N. Lewis said that physical chemistry is a catchall. He says, "Physical chemistry is whatever is interesting." But one of the things he thought was uninteresting was chemical engineering.

02-00:58:09
Prausnitz: Well, I think you're giving him a little more credit than he perhaps deserved. [laughter] The quote that I heard was, "Physical chemistry is what I find interesting," [laughter] or "what interests me," was the word, I think. "Physical chemistry is what interests me." So that's a little different from the way you put it.

02-00:58:31
Burnett: That's true, that's true.

02-00:58:33
Prausnitz: But yes, whatever's interesting, physical chemistry. And, of course, I very much regret that I never knew Lewis. He died nine years before I came here, so I never knew him. But when I first came, his spirit was still very much alive, and, of course, many of the chemistry professors were either former students of his or postdocs that he had worked with, so his influence was still very, very strong at the time when I came here. I had a direct experience of that influence when I looked around for space. I needed not much space but some space to set up some equipment, and so I wandered around Gilman Hall to see if there was any space that might be suitable, and I found on the third floor a small room that had a bed in it, a desk, a chair—a cot, not really a bed,

a cot—and a little toilet was in there, too. And I said, gee, this space is very interesting; no one's using anything. So I went down to the college office, which was on the first floor of Gilman Hall at that time, and I asked about this space. And the secretary, who had been G.N. Lewis's secretary and was sort of the major administrative person in the college, was horrified, said, "Oh, no, no, no, you can't possibly use that room! That was G.N. Lewis's retreat. He would go there for a nap, or if he just wanted privacy." He, of course, had been dean here for thirty years, so whenever he wanted to get away he would use that little room, and that was holy ground. This is where G.N. Lewis had been. He sat on that toilet. You couldn't possibly do anything with that room. [laughter] So that was out.

And then a couple years later there was a complete physical revision of Gilman Hall. A new elevator was put in. The previous one was a really old, rickety elevator, known as the G.N. Lewis Memorial Elevator, [laughter] and the whole building was physically revised. And at that time, that room went. It was revised. It became an office. But for years, that room was holy ground, not to be touched.

02-01:01:29
Burnett: So you arrive in—there's shortly to be a kind of—certainly, at the very least, a dramatic reform of chemical engineering, of what it was, what it could become. And were you aware that this was on the way? Did you feel like you were already a part of that when you arrived?

02-01:01:54
Prausnitz: Yes. Oh, yes, definitely. Definitely. That's one thing I found very attractive, that here was a brand new chemical engineering department which had a different philosophical basis, and the leading person at that time in chemical engineering was Charles Wilke, and he was the first chairman. Actually, I'm not sure he was the first chairman, but he was the first prominent chairman; let's say it that way. And, of course, I thought he was a brilliant man because he had hired me. [laughter] But I admired Wilke very much, and he did a lot of very nice fundamental work on transport properties of fluids. And he also was somebody who I thought was very courageous. He had made his reputation in this area of transport properties, and he decided somewhere in the sixties that that field now was sort of dying out. There wasn't much more to be done there, and he would then switch. And he switched to biochemical engineering, which was brand new at that time. That was really a totally new thing. And he did quite a bit of work in that area, in the latter part of his career. So that was, I thought, very courageous of him, and sort of inspired me to ask similar questions when I was on sabbatical in Germany in—this is later, of course—1985. I said, well, I'm going to try and apply my thermodynamics knowledge to biochemical problems, and I did so with some—not a lot—but some success. [laughter]

02-01:04:13
Burnett: Yes. Well, in those early years, between your arrival in '55 and the publication of *Transport Phenomena* out of U. Wisconsin-Madison, what were you working on, and working towards, in those years? You were working on like things, transport phenomena, these—?

02-01:04:43
Prausnitz: No, I wasn't doing any transport phenomena. No, I didn't do that. I worked in thermodynamic properties, and I was still very much beholden to the petroleum and petrochemical field. So we did a lot of experiments of phase equilibria in petrochemical systems, and we worked on correlations and equations of state and models for activity coefficients, but it was primarily for petrochemicals and petroleum. And then later on we did quite a bit on polymers, phase equilibria in polymer systems.

02-01:05:23
Burnett: Can you talk about that beholden state? Did you continue consulting, or was the door open at Shell Development Corporation? Did you develop a relationship with them, or was there—?

02-01:05:40
Prausnitz: Well, that was just on the basis of personal knowledge. In other words, I knew some of the people over there and became friendly with them. I was never a consultant for Shell. They were way ahead of me. [laughter] I was a consultant for Chevron for a while, and that worked very well.

02-01:06:03
Burnett: In the late fifties?

02-01:06:05
Prausnitz: In the late fifties, yes. I worked there one summer. The summer of 1956, I spent the whole summer working at Chevron, and that also convinced me that I really preferred the academic career. The bad thing that really bugged me was that I had to be there at eight o'clock in the morning, and that I didn't like at all. In fact, what happened was I had a desk at Chevron, and then about two minutes after eight the telephone rang, and the voice would say, "Oh, good morning, Professor Prausnitz," and that was it. That was the whole conversation. They were just checking to see if I was there two minutes after eight. And I was there, but I didn't like it. [laughter] I didn't like that at all.

02-01:06:57
Burnett: Right, the supervision and the verification, yeah. So where was that office, the Exxon office?

02-01:07:05
Prausnitz: In Richmond.

02-01:07:06
Burnett: In Richmond? Right.

02-01:07:07
Prausnitz: Yeah, where they still are today.

02-01:07:10
Burnett: And there's, of course, a massive petrochemical facility up in Vallejo, San Pablo Bay area? There's tanks there, and—

02-01:07:22
Prausnitz: But that's not Chevron.

02-01:07:24
Burnett: No. No.

02-01:07:26
Prausnitz: I think that in Rodeo [CA], there's a big one, but that's Union Oil.

02-01:07:36
Burnett: Okay. But there is a reason that Chevron was here, to capitalize on some of the human capital—

02-01:07:43
Prausnitz: Well, they have a harbor. They have a harbor, so ships would come in from all over the world, bringing raw oil, and then they would refine it here in Richmond. I don't know if— Do they have a facility further inland? I don't really know if they do or not. There are certainly refineries there, but are they run by Chevron? I'm not sure. There's one Shell refinery, and there's certainly a Union Oil refinery, but I'm not up to date.

02-01:08:18
Burnett: So when you were there—and this was during the summers, or there was also during—

02-01:08:22
Prausnitz: Summer of '56, right.

02-01:08:23
Burnett: Summer of '56.

02-01:08:26
Prausnitz: I spent the whole summer with them.

02-01:08:27
Burnett: So the role of a chemical engineer who is on loan, let's say, or consulting for Chevron, is to be there and respond to questions from the process folks, or to do your own research?

02-01:08:43
Prausnitz: No, I was given a particular problem to work on. They had methods, very crude, very crude methods for calculating vapor equilibria in paraffinic hydrocarbon systems. And increasingly they were getting oils with aromatic molecules, benzines and other things. And the idea was now how to extend

what they were doing to take care of these aromatic molecules. And I tried to convince them, don't extend. These methods you have are very rough, very crude. The thing to do is start using a theory of solutions called the theory of regular solutions, which Hildebrand had worked out in the early 1930s. And they didn't know a thing about it. They said, "Well, we don't know what this is." And, in fact, some of the people there didn't like at all what I was saying. They said, "Well, that's academic stuff, and that's very nice for you people in university, but we want results." Oh, I had a real tough time convincing them that the theory of regular solutions might be useful. And they finally saw it. We worked out correlations using this theory, and after I had left they put some of their other people to work on it, and for years it was used. Now it's out of date, but for years they had computer programs based on the regular solution theory that they used quite successfully for a long time. But I had a tough time convincing them this was the thing to do.

02-01:10:42
Burnett: And so in getting them to use this theory, they would adjust the calibration of their—I don't know if it was fractionation that they were doing, to remove benzine and isolate it—

02-01:10:58
Prausnitz: Well, to separate it, yeah.

02-01:10:59
Burnett: To separate it. And so that would involve changing relationships of temperature and pressure—

02-01:11:07
Prausnitz: And composition. That was the main thing. Their methods had temperature and pressure effects but they didn't have any composition effects, and they assumed ideal solutions, and that was just very crude, very crude at that time.

02-01:11:23
Burnett: This is something you write about. There's ideal gases. This was like Boyle's gas laws, and these basic relationships of temperature, pressure, and volume, but there's an actual substance there, and the substance is different [laughs] from the ideal substance.

02-01:11:40
Prausnitz: Absolutely, and I had a real tough time convincing them that they should modernize. "Modernize" in this case meant using theoretical ideas that had been worked out twenty-five years earlier, right in their own backyard. They didn't know—they knew Hildebrand, they knew there was this person Hildebrand, but they had absolutely no idea of what Hildebrand had done, and using Hildebrand's theory was shocking to them at first. Eventually they came around. Especially, there was one man there—he was fairly high up in the administration—and he had gone to the University of Michigan, and these crude methods that I mentioned had been developed at Michigan, and that's what he had learned. And so his approach was, well, okay, we have a

somewhat different substance now with aromatics, so we'll make a modification, a correction factor, an "aromaticity correction factor" is what he wanted. [laughter] And I had a tough time. I was dismissed as an irrelevant academic. It was really bad. He finally came around.

02-01:13:00
Burnett: So here's a portrait. You're a new scholar, and on the one hand you have a fight on your hands to demonstrate that you're interested and engaged in the latest theory—

02-01:13:20
Prausnitz: It wasn't late! It was 1930! [laughs]

02-01:13:22
Burnett: No, no, for sure, but I'm talking about your acceptance by the larger community of chemists in the college. When you made that talk and you made that joke, you signaled to them that you're open for business as far as having conversations about theory, about experimental results, about new ways of doing things—an innovator, in other words. And on the other hand, you're dipping your toes in the applied world, and you're getting slammed as an abstract theoretician who's an academic, and "this is not relevant," and "we need to make a profit." And if there was a contest for your heart and your mind at that time, it was clear, it seems to me, that it wasn't—[laughs] I don't think you were considering a career in the petrochemical industry, but if you had been, that kind of sealed it for you. You weren't interested. The culture was such that you couldn't grow.

02-01:14:34
Prausnitz: That's right, and I've heard this from other people. I'm not the only one who noticed this. The petroleum and petrochemical industry was very conservative. "Don't bug me with anything new. I don't want anything new. This is the way we do it. We've done it this way for twenty years. We know this works, and if we have a new situation, well, we'll tweak it a little with the correction factor." They were not interested in doing things another way. And many colleagues of mine have noted they're very, very conservative. Another thing I noticed which was sort of, well, discouraging, at Chevron they had very good chemistry people. They had very good analytical chemists. And yet the process design people would not do anything new about chemical analysis. They had their empirical methods that were developed in the twenties, and so if they wanted to know what the composition of an oil was, they used these very crude methods. And here, next door, were these analytical chemists who'd worked out some very nice new chromatography methods that could give you information about what was in the oil. Well, they didn't mix. The analytical chemists were doing very nice work but the process design people didn't make use of it.

02-01:16:11
Burnett: That's fascinating. This is a period of the industrial research laboratories, so companies had their own research labs, and they didn't rely, as they do today, on universities to do their fundamental research for them.

02-01:16:24
Prausnitz: No, they had very good people there, but the process design people never made use of it. Now, again, that was then; today it may be quite different.

02-01:16:33
Burnett: Well, I think in those days—this is the time of the Seven Sisters and this oligopoly that basically fixed a world market for the price of oil, so the price pressures on oil refining are not so much there. There's this abundance of oil. It's more of a political consideration to consider who has what amount of oil. So innovation is not being highlighted in that context, and there's no environmental consideration either at that time.

02-01:17:07
Prausnitz: No, they didn't worry about it. Not until later. Now they worry about it, but not then.

02-01:17:12
Burnett: Now they worry about it, internally at least. So you had these rich, brief experiences and encounters with an industry, but you said you were working on problems to do with petroleum processing.

02-01:17:30
Prausnitz: Right, and we published quite a few papers that eventually were accepted, but not initially, no.

02-01:17:37
Burnett: Right, so there's an uphill battle. Meantime, though, you're cultivating relationships with people, Bernie Alder at Livermore. How did you meet someone who was working at Livermore?

02-01:17:50
Prausnitz: Well, he lived in Berkeley, and he had an office, small office here in the chemistry area, and he worked with Hildebrand, so that's how I met him, and our wives are good friends—that makes a difference—still to this day. Bernie is about ninety-three by now. So he's one of the few people still around who's older than I am. [laughter] Not much older, but a little bit.

02-01:18:29
Burnett: Sounds like we need to interview him.

02-01:18:31
Prausnitz: Well, Bernie would be a nice man to interview. He's one of the originators of molecular simulation methods. He's made a very substantial contribution to science.

02-01:18:47
Burnett: Wow. I'm fascinated by how science works socially. So you mentioned a weekly seminar across the college, which sounds like a wonderful—it sounds almost like a Manhattan Project thing, where they would come at the beginning [of the day] and anyone could talk about a problem from any branch, and be heard by the whole community.

02-01:19:12
Prausnitz: Yeah, G.N. Lewis started that when he came here. He started this seminar, and he would preside. And it went on after his death—Kenneth Pitzer was presiding—but eventually it stopped because people wouldn't come. It was in the evening, I recall, and people didn't want to stick around. They wanted to go home, have dinner.

02-01:19:43
Burnett: In the early days when you were there, most professors lived in Berkeley, would you say?

02-01:19:51
Prausnitz: I think that's right. Well, quite a few lived over in points east, but there was no great problem of commuting. You could drive, and traffic was minimal, and you could park on campus.

02-01:20:11
Burnett: [laughs] Which is unimaginable today.

02-01:20:12
Prausnitz: Today it's a real hurdle.

02-01:20:16
Burnett: And so socially people would—were there gatherings for beer at a local haunt? How did it work? How did people spend time together? Was it dinners? You mentioned wives, and—

02-01:20:32
Prausnitz: No, it was primarily lunch. There was a table, fairly large table, in the Chemistry Department. That table is still there, and at the Faculty Club there's a table. And if you didn't get there until a few minutes after twelve, you didn't get a seat. You had to get there on time. So there was a lot of socializing at lunch. And also at that time there were no women faculty, so the wives would have teas together. I remember we had a tea at our house, and various faculty wives would come. And there were all sorts of rules about the tea, lest there be a competition about who could make a more glorious tea than the next, and I think you were only allowed to have two types of cookies or something like that. There were rules. [laughter] But, of course, that completely died, especially now that we have quite a few women faculty members.

02-01:21:45
Burnett: Yeah. Were there chemistry wives who themselves were chemists? Because there were quite a few of those couples who were—

02-01:21:53
Prausnitz: Yes, there were a few. It was rare. Now it's much more common.

02-01:21:58
Burnett: Right. Well, we're kind of on this subject, then. You met someone around this time, did you not?

02-01:22:08
Prausnitz: What do you mean, I met someone? [laughter] I don't understand.

02-01:22:12
Burnett: Sorry, I don't mean to be—

02-01:22:12
Prausnitz: I got married, if that's what you mean.

02-01:22:14
Burnett: Yes, yes!

02-01:22:15
Prausnitz: I got married in June of 1956. My wife's name is Susie, and she would go to these teas. And, as I said, I remember we had one tea at our house. But, again, that's all died out now.

02-01:22:32
Burnett: Yeah. And so you met her through these tea networks.

02-01:22:36
Prausnitz: No, no, she was in New York, and I met her through mutual friends in 1954 or '55. No, no, she had absolutely no connections here at Berkeley. She was strictly a New York type, and the idea of coming out to live in California was shocking to her.

02-01:23:06
Burnett: I'm sure.

02-01:23:07
Prausnitz: Because New Yorkers are very loyal, and the idea of leaving New York was really a bit of a problem, but she got used to it, and she began to see that one can live very nicely and colorfully. [laughs]

02-01:23:24
Burnett: And what is she like? What does she like, for example? Her interests or her—

02-01:23:35
Prausnitz: Well, she likes the freedom, I guess. In New York, life at that time was much more constrained. You couldn't do this and you couldn't do that; it wasn't proper. Here, you just do what you please. [laughter]

02-01:23:54
Burnett: So there was a West Coast culture—

02-01:23:57
Prausnitz: Oh, it's still true today. It's much more *léger*, much easier here. You do what you want. You don't really care so much about what other people think.

02-01:24:10
Burnett: And in terms of diversions, we talked about opera extensively last time. And so when you had the chance did you go tripping into the city to see San Francisco Opera?

02-01:24:24
Prausnitz: Yes, and of course occasionally the opera came here in the Greek Theatre, they would play, but that's all stopped, unfortunately. At that time, you could still go to the opera without becoming completely poor. The tickets nowadays are so expensive that I go relatively rarely. And also, Susie's not an opera fan, unfortunately, so—

02-01:24:53
Burnett: She had other qualities.

02-01:24:54
Prausnitz: Well, we both are very fond of chamber music, and fortunately there's quite a bit of that around, and you can still go to chamber music concerts without going to the poorhouse afterwards. [laughter] The prices are high, but nothing like the opera.

02-01:25:12
Burnett: Right, absolutely. So you're establishing, together with others, a new way of looking at and doing chemical engineering at the end of the 1950s.

02-01:25:31
Prausnitz: Yes.

02-01:25:31
Burnett: So the arrival, then, of *Transport Phenomena*, the text, is confirmation of a wave that's already underway.

02-01:25:40
Prausnitz: Yes.

02-01:25:41
Burnett: Okay. And was it helpful for you for that book to have the impact that it did? Did it confirm that your path was something to pay attention to?

02-01:25:53
Prausnitz: Yes, it definitely gave support to this wave, this movement.

02-01:26:03
Burnett: Did you teach using it?

02-01:26:06
Prausnitz: I didn't, because the courses where I was involved it wouldn't have been appropriate. I taught graduate and undergraduate thermodynamics. I was also involved in the undergraduate unit operations laboratory, and I enjoyed that, but there, the book by Bird, Stewart, Lightfoot [*Transport Phenomena*] would not have been appropriate.

02-01:26:34
Burnett: Yeah. And unit operations is that kind of process engineering.

02-01:26:38
Prausnitz: Yes, and we still have that laboratory today, and I was quite involved with that for many years. But my major contribution in that particular laboratory course was technical writing. I was very much interested in teaching students communications, technical communications. And I tried very hard, with success, to get us to offer a course in technical communications, which, indeed, we did. And I had to convince my colleagues that that was part of our professional responsibility, teaching our undergraduates to write and speak. And I was greatly supported in that by the feedback that we got from our employers, the people who employ our students. They were very much in favor of this. They said that our students, when they get out, are technically okay, but their communication skills were not good. And so we instituted that course, and unfortunately now it had to be canceled because of budget reasons, but I'm sure it'll come back soon. The budget eventually is going to get better, and so that course will come back, but it is not available now, unfortunately.

02-01:28:14
Burnett: And that's been a reputation that has dogged engineering in general, that there's insufficient attention paid to writing and to language that's not mathematics.

02-01:28:23
Prausnitz: Right. But we had our own course, and we will undoubtedly have it again as soon as the budget situation improves.

02-01:28:31
Burnett: And it's clearly important to you from the writing that you've done that you're interested in clear communication. It's also evident in *Transport Phenomena*. I was reading it the other day, and it is—of course, I can't do the formulas, [laughs] but I was astonished at the clarity of the examples, and the structure of it, that it can be taught in several different ways, and it has columns and rows for the chapters, and you can teach it one way for more advanced students, and a more basic way for beginners. And I really appreciated the care with which this new approach to engineering was being undertaken. It sounds as if this was very, very deliberate, that there was a need to convince people that engineering students need to know this material. It follows from their natural curiosity. So I think you had a kind of—I'm speaking now of your article in 1963 for the *Journal of Engineering Education*—it's almost a

manifesto, to some degree—that's too strong a word, but it has a model of a certain kind of student, because the old ways were, as far as I can tell, rote learning about basic formulas, and here's how you do this kind of process, and you follow these rules, and you'll be okay. But you're dealing with these ideal properties. And I think you write at one point, "Once a student has learned the first and second laws and some of their applications, and once he has at least a limited familiarity with the various thermodynamic functions, he very naturally becomes curious about how numerical values for these functions can be obtained, but this is not given in the education." So you had an idea of the student who was curious. You wanted to satisfy that type of inquiry, and that's the kind of education that you wanted to cultivate and the kind of student you wanted to cultivate. So can you talk about—this comes out in '63, and it's called "Molecular Concepts in Chemical Engineering and Thermodynamics." And can you talk about the genesis of this article? And it's an important paper. Along with *Transport Phenomena*, it kind of sets up this new type of education that we need to undertake. Can you talk about why you needed to write this article? And it's now fully eight years into your time at Berkeley.

02-01:31:23
Prausnitz: Well, I was trying to persuade other departments of chemical engineering throughout the country, or throughout the world, to adopt some of these new ideas, and most departments have, indeed, done so. I think Berkeley was ahead. We did it before anybody else, but by now it's well accepted. So I must confess I don't remember this article, [laughs] and I'll have to go back and look at it.

02-01:31:58
Burnett: There are some things that I think that we could just talk about in the abstract, because you write—this is on page 525—"This discussion, in turn, introduces the student to some molecular properties with which he is usually not familiar, such as dipole moment, ionization potential, magnetic susceptibility, polarizability, electron affinity, etc. Lest the student get the mistaken notion that all intermolecular forces are completely understood, brief mention should be made of specific forces for which no simple intermolecular potential can be written." So I wanted to ask you about—

02-01:32:41
Prausnitz: The hydrogen bomb is the best example of that.

02-01:32:45
Burnett: Right. So teaching the unknown, I wanted to ask you about that. So is this part of the pedagogy, that you wanted to signal to the student that we don't know everything, that there is a frontier, that there is something we have yet to figure out?

02-01:33:06
Prausnitz: Yes. And there's been a lot of progress, but we're still working at it. The usual example that we give is this: supposing you have a young lady—you know

her; you know all about her tastes and whatnot—and you know a young man, you know all about him, all about his history, whatever he's done, so on, and now you're trying to predict what will happen if this young lady and this young man go on a blind date. You cannot predict it. You know all about her, you know all about him, but you cannot predict what will happen. That's where we are with molecules. We have a substance A. We know all about it, we study it, we know all about the properties. We know a substance B. We know all about it. Can we predict what happens when A and B get together, get mixed? No, we can't. This is our predicament.

02-01:34:18
Burnett: So in the old chemical engineering you had the classical thermodynamics, which are these rules of general principles, and the interaction amongst various aspects of thermodynamics, but it doesn't take into consideration the complexity at the molecular level, and that that's where the nature of the substance matters, where matter matters, effectively. And so you're trying to bring the chemical engineering community to the point where they can wrestle with these and achieve progress, that there are things you have learned even in that time— Did you feel that you had made progress personally as a scientist—

02-01:35:09
Prausnitz: Oh, yes.

02-01:35:09
Burnett: —between '55 and '63, you learned something about molecules that would have an impact?

02-01:35:14
Prausnitz: Yes, definitely. And that's the one thing that we all have in common here in the Berkeley Chemical Engineering Department. We have some eighteen professors, and they do totally different things, but the one thing that binds us that we all agree on is we take a molecular approach. We ask, "What's the molecule doing?" That is a question we all ask for different reasons, some with regard to batteries, others with regard to polymers, others with regard to fluids that are mixing, or de-mixing. The molecular approach is what binds us, what is common to all of us, but we are interested in molecules for different applications.

02-01:36:13
Burnett: So even today, there are chemical engineering departments that don't take that as this—?

02-01:36:20
Prausnitz: Relatively few.

02-01:36:22
Burnett: Really?

02-01:36:23
Prausnitz: There used to be—which essentially all chemical engineering departments didn't ask that question, "What's the molecule doing?" Now most of them do, but there are still some that are old-fashioned, shall we say.

02-01:36:36
Burnett: So this was a vanguard, and this approach to molecular thermodynamics was new in chemical engineering, and you and your colleagues were the real advocates for this.

02-01:36:51
Prausnitz: Yes. We're not the only ones—I don't want to claim that—but we certainly were, yes. It's getting almost twelve o'clock, and I have to go to a faculty meeting, so we'd better stop here—

02-01:37:06
Burnett: Sure, sure, absolutely.

02-01:37:08
Prausnitz: —and continue again whenever convenient to you.

02-01:37:12
Burnett: Perfect.

Interview 3: November 28, 2018

03-00:00:18
Burnett: This is Paul Burnett interviewing Dr. John Prausnitz for the University History Series, and we are here again in 308 Gilman Hall, and this is session three, November 28, 2018. Welcome back, Dr. Prausnitz.

03-00:00:36
Prausnitz: Thank you.

03-00:00:37
Burnett: So we were talking about your approach to education, and as you encountered your students you learned more about what they needed and adjusted your methods accordingly, and we briefly skated over the nature of molecular thermodynamics. But I'm wondering if you could briefly summarize the kinds of research and work that you were doing in the 1950s and into the early 1960s. I understand that this has more to do with the traditional needs of the petrochemical industry, the kinds of questions and problems that had to do with that industry.

03-00:01:30
Prausnitz: Yes. Well, one of the major tasks in chemical engineering is separations. Nature gives us materials that we can work with, but these materials are rarely pure. The best example is air. Air is not a single molecular substance. Air is mostly nitrogen and oxygen, and all sorts of other little things, like CO2. And so one of the big tasks of chemical engineering is to take the materials that nature gives us and separate the various components in that mixture, the components that we want from the components that we don't want. Again, a good example is air. We need nitrogen for making ammonia. We need oxygen for making steel. So one of the first steps is to take air and separate it into nitrogen and oxygen. But then when you have the material in the pure form that you want, you put it into a reactor where the chemical reaction occurs, but the chemical reaction that occurs is rarely complete. Only a certain percentage of the reactant that you put in will come out as the product that you want. So what comes out of the reactor is not just the product that you want, but also side products that you don't want, and also reactions that have not reacted. So coming out of the reactor, you, again, have the problem of separation. You have to separate the product that you want from the product that you don't want, and you have to separate the unreacted reactants so you can feed them back and use them once more for another trip through the reactor.

03-00:03:32
So what I'm saying is that separation of fluid streams is the bread and butter of chemical engineering. There's a tremendous amount of need for understanding these separation operations, and in a typical chemical plant, if you look at the investment, the investment is usually on the order of fifty percent for separation operations. In an oil refinery, it would be even more. So these big columns that you see here in the Chevron refinery in Richmond, these are all

separation operations. They're distillation columns, mostly. So it obviously is a good idea, from an economic point of view, to do these separations efficiently, and that has been the task in chemical engineering ever since the beginning. That problem is now pretty much solved. Now, in 2018, it's not an acute problem anymore, but in the 1950s and [19]60s it still was a very considerable problem.

03-00:04:41

And my contribution to trying to alleviate—I won't say solve, but to alleviate—that problem is to generate the thermodynamic properties that you need in order to design separation operations. The most common one is distillation, and in distillation what you do is you take a liquid mixture and you boil it, and the vapor that comes off from boiling this liquid mixture has a composition different from the composition of the liquid. And you keep doing this in stages until you finally get the purity that you need, so that the essence of the problem is what are the compositions of the equilibrium phases. You have the composition of the liquid. Well, you want to know, okay, what is the composition of the vapor that you generate from this liquid? So this is known as the vapor–liquid equilibrium problem, and that was not really well understood in the fifties. It was essentially empirical. People measured things. There are standard ways of doing that. You actually measure the composition of the liquid, using some analytical technique, and you measure the composition of vapor using not necessarily identical but a similar analytic technique.

03-00:06:06

And so there's a lot of data around but there's one big problem, and that is that the data that you find in the literature on vapor–liquid equilibrium is almost exclusively for binary systems; in other words, you have two components of a mixture. But the mixtures that you deal with in industry are not binary. They have many more than two components, typically five, six, maybe even ten components. And, of course, you don't have data for that. You just have data for binaries. So one of the problems is, how do you take the binary data and scale up to get multicomponent data, and that is something that I worked on for many, many years, and we came up with some techniques. They're not perfect, but they're pretty good, and these techniques we developed are now standard throughout the world. When people want to estimate the vapor–liquid equilibria of a multicomponent solution, they don't measure for that case, because that would be much too much work; they measure the various binaries that are formed in this multicomponent mixture, and then, using techniques that we developed here, you can make a pretty good estimate of the multicomponent behavior. So that was one of the focal points that I worked on in the late fifties and the sixties.

03-00:07:38

Burnett: Could I ask you to perhaps elaborate on the technique that had the biggest payoff? What was the contribution that you think you and your lab made to making refining processes more efficient?

03-00:07:59
Prausnitz: The major contribution was that we provided methods for estimating, with reasonable accuracy, the vapor–liquid equilibria of a mixture. In other words, you have a mixture with a lot of components in it, and you start boiling it, and you want to know what's the composition of the vapor that comes off from this liquid, and that's a problem that we worked on, and it's still a bit of a problem today but much less so. The problem is not solved in any ultimate sense, but for engineering purposes we have very good methods now, and my contribution was to develop those methods, and to show what essentially—it's called scale-up. "Scale-up" is a word that we love in chemical engineering. We take a process for making a small amount of material, and then we develop a similar process for making a lot of the material. That's called scale-up. And in a sense what I was doing was scaling up vapor–liquid equilibria, a scale-up from the binary data that we often have, or that we could get fairly easily, to scale up to a multicomponent, to a mixture, perhaps, of five, six, seven, ten, or even more components in there.

03-00:09:19

So that was one of the main focus points, and we also worked on liquid–liquid equilibria, not only vapor–liquid equilibria, but if you have two liquids, you can use that for separation. For example, supposing you have an alcohol—let's say ethanol—dissolved in water, you want to get the ethanol out. Well, one way you can do that is to contact the water with a hydrocarbon, with an oil, and some of the ethanol—not all, unfortunately, but some of the ethanol—will get into the oil. Well, okay, why is that necessarily an advantage? Well, you've still got to get the ethanol out of the oil, but it turns out that's a lot easier to do than getting the ethanol out of the water by distillation. With ethanol and water you have a particular problem that's called an azeotrope. There are some situations where the vapor that you generate from a mixture has the same composition as that of a liquid. That's called an azeotrope, and that happens in ethyl alcohol, ethanol, and water. So distillation is not going to get you pure ethanol, but if you take the ethanol and put it first into an oil, the ethanol and the oil do not form an azeotrope, so you need to distill the ethanol out of the oil and get pure ethanol. That's sort of a crude example but it illustrates the point. So we worked on extraction, also, and liquid–liquid equilibrium.

03-00:11:03
Burnett: I imagine this kind of research becomes more and more important as the basic elements that are being extracted, whether it's in mining or in the petroleum industry, are getting dirtier, [laughter] right? And so there are more components to remove. Isn't that something that becomes more and more of a problem? It wasn't so much a problem at that time.

03-00:11:34
Prausnitz: When you say "dirty," you mean more—

03-00:11:36
Burnett: Meaning that it has more components.

03-00:11:38
Prausnitz: —more junk in it, yeah. [laughter]

03-00:11:38
Burnett: More junk, that's more difficult to separate. So, sweet light crude, for example, which is relatively easier to refine, and as that supply dries up the industry is—

03-00:11:53
Prausnitz: Yes, yes.

03-00:11:53
Burnett: —looking for more and more challenging—

03-00:11:57
Prausnitz: Yes, that's true.

03-00:11:59
Burnett: —things to separate.

03-00:12:00
Prausnitz: That's true. The easy stuff has been used up, and now you have to start separating things that are more difficult. Yes, I think that's undoubtedly true. We also look at adsorption, and that was a very successful enterprise. I had a very good student by the name of Alan Myers, and we worked on adsorption of mixtures. This is a gaseous mixture. If you have a mixture of gases and you want to separate them, one way you could do it is to contact this gaseous mixture with a solid, and one of the components of the gaseous mixture is more likely to adsorb than the other, so you have a basis of separation. And, again, you need thermodynamic equilibrium data in order to design an adsorption separation process, and Alan and I worked on that, and we wrote a paper on how to do this, and I think that paper received more references than anything else that I've ever done. It's now at least fifty years old, maybe more, but it's still quoted all the time.

03-00:13:21
Burnett: Yes, I think Google has tracked twenty-five hundred citations, roughly, so that's a high-impact paper, I think. [laughter] Can you talk a little bit about Alan Myers, and what was his interest when he came, and how did you work together? And tell me a little bit about his career after you finished.

03-00:13:45
Prausnitz: Well, one of the advantages that Alan had over other students, he was older. He had served in the Navy—I think he was in the Navy for six years—and that is necessarily a maturing experience, [laughter] so he was more mature than the other students that I had, and he was very dedicated. And I remember

distinctly when he left, because—it was in late November 1963—it was on the day when President Kennedy was assassinated.

03-00:14:20
Burnett: Oh my goodness.

03-00:14:21
Prausnitz: I remember that day, of course, very sharply. That was the day he left. He went to the University of Pennsylvania in Philadelphia, and he had a very successful career there. He's now retired, and I think he lives in London. He has a daughter who lives in London with her husband. I think he's there now. But he had a very good career. He's an excellent teacher. He was very, very clear, and people really could understand him easily.

03-00:14:56
Burnett: In the years when you were working on these problems that were really fairly specific to the petrochemical industry, were there sources of support, funding from the industry? Did the industry help you do this research, or was this a public university that was fairly disengaged from that world?

03-00:15:19
Prausnitz: No, industry did not help me directly. Now, they did indirectly. There's something called the Petroleum Research Fund, which is administered by the American Chemical Society, and they give out money for research—not very much, but still, enough to make a significant difference—and I got quite a bit of money from them. And as I understand it, that money that the American Chemical Society administers comes from industry. So indirectly, yes, there was some industrial support, but it was minimal.

03-00:16:01
Burnett: Right. Was there support from the federal government or the State of California, for example?

03-00:16:05
Prausnitz: Yes. Oh, no, not the State. The State [of California] didn't help me at all. Yes, the National Science Foundation. That was, of course, a good source of funds, and in those days you had a reasonable chance. If you sent in a good proposal, the chances of getting it funded were very good—not a hundred percent, but maybe seventy-five or eighty percent—unlike today. You send in a good proposal and your chances of success are very small.

03-00:16:35
Burnett: The National Science Foundation, it obviously grows in size and importance during those years, but in the early years was the NSF important, too?

03-00:16:53
Prausnitz: Oh, it was very important to me, certainly, and, I think, to others, because if you had a good proposal you probably would get funding. And another source was the Lawrence Laboratory here, the LBNL, Lawrence Berkeley National Laboratory. And in the very early years, Professor [Glenn] Seaborg, who was

one of the major figures in that laboratory, he would give money to the Chemical Engineering Department, without specification. He just said, "Well, here's some money," and gave it to the chairman, and so that was a big, big help in those days. But that didn't last very long.

03-00:17:37
Burnett: There was an understanding that the Department was in a somewhat vulnerable growth phase. It had been established in the forties, but it became, in its modern form, in the fifties, when you were hired—

03-00:17:54
Prausnitz: I think so. I never discussed it with Seaborg, but I was very grateful that we got some money. [laughter] I didn't ask him much about the philosophy, but it didn't last very long. That stopped, although some of my colleagues, of course, were investigators at LBNL, as was I, later on. This happened later. I also had a position up there, and got money there, but that was not until later.

03-00:18:24
Burnett: Can you talk about how that works, to work at—? Because I'm assuming you were not getting time on the accelerator [laughter] and that kind of thing, right? So—

03-00:18:36
Prausnitz: No, but we don't need it. I'm not interested in the accelerator. [laughter] Well, you have to sort of convince the people up there that what you're doing is somehow helpful for the energy situation in this country. That was LBNL's main function, talking about energy, and if you could somehow justify that what you were doing was useful in the general energy problem, why, then you had a chance to get onto their payroll. And the way I did manage to do that is sort of the usual way in which things are done in academia: I had an outside offer from MIT. MIT wanted me to come, and they offered me an endowed professorship, and the University here didn't want to lose me so they wanted to do something nice for me, and so I was allowed to become a member of LBNL. So this is the way things usually work. You have to have some sort of a threat, and then the University responds.

03-00:19:46
Burnett: Right, right. But then I guess the energy focus really intensifies more in the seventies for LBNL [Lawrence Berkeley National Laboratory], and so it was not so much— [Glenn] Seaborg's donation to Chemical Engineering was earlier, right? It would've been in—

03-00:20:07
Prausnitz: Yes, it was earlier, before I came, but it lasted for a while longer, and it was a big help. And the people here were working on various separation processes that were of interest to LBNL, so that was a very happy situation.

03-00:20:28
Burnett: Yeah. Can you talk about your colleagues at the time, were there conversations about the—? You're going to a Department meeting after we talk today. So what were the conversations like in the late fifties, early sixties, about the direction of the Chemical Engineering Department and the direction of chemical engineering on the national scene?

03-00:20:56
Prausnitz: Well, in the Department there was an internal debate. Professor Wilke was Chair. He was a wonderful chair; he was very, very good. And his view was that the Department here should be diverse. We should have expertise in various areas by having professor A in this area, professor B in another area, and so on. And in opposition to that idea, which was led primarily by Professor Acrivos, Andy [Andreas] Acrivos. Andy's picture was: no, let us not do that, but let us identify just a few areas—maybe three areas—and build expertise in these three areas—let's say three; it could be four—these areas of concentration, although we would have more than one professor in that area. We would have two or three, or possibly even more, professors in that particular area, and then in another area, but the number of areas would be small. So that was a real difference in concept. And Wilke was quite adamant. He was very pleasant about it, but he felt that diversification is—and by "diversification" I mean intellectual diversification—he was very strong on that. And then the issue sort of died when Acrivos left to go to Stanford. That was in the early sixties. He went to Stanford, and so this controversy stopped.

03-00:22:43
Burnett: Right. And so since Stanford had yet to really mature its chemical engineering department at that time, this would have been the West Coast MIT, [laughter] right? Or the West Coast chemical engineering powerhouse at that time, I would think, right? Or would Caltech have a—?

03-00:23:06
Prausnitz: Well, no, Caltech certainly is in there, and the University of Washington was then and still is one of the top departments in the United States. So I wouldn't say that we were the powerhouse, but we were certainly a very significant part of the chemical engineering picture on the West Coast, yes. There was also University of Southern California—they have chemical engineering—and Oregon State University also has chemical engineering, so we were not alone.

03-00:23:42
Burnett: And we talked about the reputation that chemical engineering had when you first arrived, and you were conscious of that, and you wanted to buck that trend, along with the members of your generation, to say that this chemical engineering is more focused on developing the theoretical with respect to applications, but not completely in service to applications.

03-00:24:17
Prausnitz: That's right, and that definitely was the philosophy at Berkeley then, and still is today. And this puts us in a very difficult position, because the chemists and

physicists don't accept us as equals. They say, "Well, yes, you're doing theoretical work, and it sounds very nice, but it's really old stuff. You're refining old ideas, and applying them, and that's very good, but you're not really at the intellectual forefront." So we get that criticism from the pure science people here, and then we get the complete opposite criticism from our friends in industry, who say, "Why don't you people do something that we can use instead of writing all these equations with integral signs that we don't understand, or couldn't do anything with them anyway even if we did understand them? Why don't you do something useful?" And so this has been a problem that was very severe in the fifties. It's somewhat less so; it still is here. The problem has not gone away, but it is less severe, first of all because people in industry realize that for the future of our profession somebody has to do fundamental work, and fundamental work is not necessarily applicable right away, so people are beginning to be more tolerant about that. And then the chemists and physicists are becoming more tolerant also, especially because of the funding situation. They increasingly are funded by sources that want applications, which was not true in the early days, and so they look upon us a little more kindly, still not as kindly as I would like, but they look upon us a little more kindly than they used to.

03-00:26:21
Burnett: Well, let's take the example, then, of your research in the late fifties, early sixties, that helped the petrochemical industry to manage the separations of multiple components in a single set of elements.

03-00:26:43
Prausnitz: Yes. Oh, that was much appreciated after a while. It took a while. As I think I mentioned last time, people in the petroleum/petrochemical industry are very conservative, and they don't like to change things. What they want you to do is to take their methods and make a little modification so that it can be applied to the problems that they did not have previously. But if you say, "No, no, we can't do that because the method that you want us to modify is no good to begin with; we have to have a better method, and then we can use that for new problems," well, they don't particularly like that idea. They're fond of their methods, although the methods may be totally out of date, but they have an emotional attachment to methods, and they don't want to change anything. It takes real patience to get them to change. It finally will, but it takes a while.

03-00:27:49
Burnett: Well, can you describe roughly what the uptake would have been? Let's say you wrote a paper in '63, or, well, take your 1965 paper. How long did that take to be recognized by the industry?

03-00:28:07
Prausnitz: Well, it varies, of course. It depends on the particular paper, but it's the order of a few years, I would say, at least two years, maybe five years, something of that order.

03-00:28:18
Burnett: That sounds like a pretty short uptake for—

03-00:28:20
Prausnitz: Well, yes. One of my big tasks in doing what I have done is characterized by one word: the word is "translation." I am familiar with what is published in the *Journal of Physical Chemistry*, the *Journal of Chemical Physics*, and similar journals, and I'm also familiar with engineering journals, where I found out what people are worried about, and I try to translate what physicists and chemists have done into a form and language that industry can use. So I'm sort of the middle man, and I've enjoyed that very much. It's a wonderful position to be in, because it really is helpful. But people in industry generally don't read the *Journal of Chemical Physics*. In fact, many of them don't even know there is such a thing. And, of course, the physicists and so on never look at engineering journals. They don't care anything about that, at that time; it's different today. So I had a real role to play, and that's what I did, and that's what I enjoyed doing.

I found out that what industry really wants you to do is not just to translate—and I do mean "translate;" the language is different; the pure scientists have their jargon and the engineers have their jargon, and different units, also, so there really is a difference in language. But what industry really would like is never mind all this translation stuff; give us a computer program. That's what they really want. They want a computer program which they can use directly, without having to think about it. You just push the button and the computer program generates properties, something-or-another properties that they can use in their design. That's what they really would like. [laughter] And so, in some cases, this is what I did, and I would send them the program, and then they would say, "Well, can't you give us a disc?" They weren't even willing to take the program and type it into their machine. They want us to do everything for them. They don't want to lift a finger. They just want to push a button. And so I said, "Well, if you want a disc, okay, we can do that. We'll do that, but you'll have to pay for it." Oh, that's fine, they're perfectly willing to pay for it. And so I would collect—it was nominal—I would collect maybe a hundred dollars for a disc, and that kept us in cookies for our seminars. [laughter] So we would have a group meeting every week, and we'd have some refreshments, and sales of these discs took care of that.

03-00:31:19
Burnett: So an early model of industry–university cooperation.

03-00:31:22
Prausnitz: Well, on a very, very modest scale. [laughter]

03-00:31:25
Burnett: Yes. Cookies. Well, there's a couple of things to tease out there that are so fascinating. I think in 1959 there was this Oxford lecture, C. P. Snow, talking about "The Two Cultures."

03-00:31:40
Prausnitz: Oh, yes.

03-00:31:40
Burnett: Right, that there's the humanities on the one hand and the sciences, and they don't talk to one another. But what you're describing is this other kind of two-cultures problem, where we have the physical scientists and the engineers using different units, and using unfamiliar jargon, that kind of thing. I suppose that's the nature of specialization—

03-00:32:05
Prausnitz: Yes.

03-00:32:05
Burnett: —that you have to struggle with. So you are this—and I use this term a lot in interviews because it comes up over and over again—you're this kind of broker between these worlds—

03-00:32:17
Prausnitz: Absolutely, absolutely right.

03-00:32:18
Burnett: —a translator and a transporter of knowledge from one domain to another.

03-00:32:24
Prausnitz: Yes, that's right. That's exactly the way I viewed my role.

03-00:32:28
Burnett: Yes. And this role, by definition, is comparatively rare. Most people like to work in a particular row, and even though all these people are very clever and doing wonderful work in their domains, the demands of the fields are such that they impose a tremendous cost to look around at other areas. And this didn't seem to affect you, [laughter] because it was generative for you. It spurred you to do more research, to go into other areas and to explore. Was there a cost for you to—? Were there people—? For example, we should maybe talk about the fact you were granted tenure in 1960. Were there pressures for you, as in, "No, you should be publishing in this journal only?"

03-00:33:35
Prausnitz: No.

03-00:33:36
Burnett: No?

03-00:33:36
Prausnitz: No, there were no pressures. No, there were no pressures at all. In fact, I didn't know anything about tenure. I knew eventually I would be up for tenure, but I was never asked about it, and nothing ever happened. I just submitted my publication lists and suddenly one day I learned that I'd been promoted, associate professor. There was no discussion. There was absolutely nothing. It just went. [laughter] In those days, these things were much, much simpler than

today, much easier. There wasn't nearly the documentation required that we require now. So I never really thought about it or worried about it. I knew it would happen sooner or later, but it actually happened a little later than it might have, because Professor Wilke was on leave. He was spending a sabbatical year at MIT, and so the acting chair then was Professor Bromley, and he didn't do anything about promotions and tenure. He said he was going to wait until Charlie comes back. And, of course, Charlie did come back, but that delayed it, actually. I think if he hadn't gone to MIT it might've happened earlier. But this is unimportant; it doesn't matter.

03-00:34:53
Burnett: Right, right. Well, these are sometimes milestones, they may matter or not, but that's 1960 that you're—

03-00:34:59
Prausnitz: Yes, yes.

03-00:35:01
Burnett: —you're granted tenure. And at this point did you have a laboratory immediately when you became assistant professor?

03-00:35:08
Prausnitz: No. No, indeed. This was a definite problem, because this was at a time when Lewis Hall existed, but Latimer Hall had not yet been built, and Hildebrand Hall had not yet been built, so there was a definite shortage of space. And I don't know whether I mentioned it last time we talked, about how I was looking around for space and found this little room.

03-00:35:39
Burnett: [laughter] Yes, you did.

03-00:35:39
Prausnitz: Yes, I mentioned it last time. So yes, there was a definite shortage of space, and I had to really struggle to get space. I finally did, and after a few years I had enough, it was no problem. But it did take a while until I got enough space.

03-00:35:58
Burnett: And so there was a Prausnitz laboratory by the early 1960s.

03-00:36:03
Prausnitz: Yes. It was on the north end, second floor of Gilman Hall, and that was very nice. I had a nice place there, and I would go down there at least once a day and see how things were going. I would come in on Saturday before the opera and talk to people, and then after the opera I would go back again. I find that if the students see the professor there on Saturday, they're likely to show up, too.

03-00:36:33
Burnett: Yeah. Well, they might get some access, too, get some—

03-00:36:38
Prausnitz: The same with seminars. We always have trouble getting people to attend the weekly departmental seminars every Wednesday at four o'clock, and we find if the professor is there his students are likely to come. If the professor is not there, then they probably will not come. You have to set an example.

03-00:37:00
Burnett: Absolutely. And be an inspiration. But also it gives a chance to meet with you, a chance to talk with you.

03-00:37:06
Prausnitz: Yes. Oh, yeah, we would talk about many, many things. And you mentioned C. P. Snow, and I was certainly much, much influenced by that event. And he came here, I vaguely remember. I think he came here to lecture. He had a standard lecture, and I think I heard it here, and, of course, it was available in print. I never met him personally, but I thought what he said was absolutely true, and it's less true today but still much truth in it. When I meet people from the humanities—and I always enjoy meeting them, because I learn all sorts of interesting things—what I often find is that the humanities people are embarrassed by their lack of knowledge of science, and so—it's human nature, I guess—they sort of pooh-pooh it. They say, "Well, science really isn't that important to me anyway; I don't care about it," and so on, which is sort of a human way out. Well, again, I can understand it psychologically, but it's not good. And the problem is on both sides. Not only is the humanist person reluctant to learn about science, but the scientist is not making enough of an effort to bridge the gap to explain what's going on. I remember talking about this at a seminar, student seminar, about building bridges, and one of the students said, "Well, you can't really build bridges unless you have a support at both ends. The bridge has to be tied at one end and the other end." And it was his experience, the student said, that the humanists were not interested in supporting their end. And so I said, "Well, I know that's true, but that doesn't prevent you from building bridges, because you can have a cantilever bridge." That was a brilliant idea that just occurred to me on the moment. A cantilever bridge is, as you may know, sort of tied at one end but not on the other. It's used sometimes over rivers so when a boat comes through the cantilever bridge rises so the boat can get through, but then it goes down again. So it does not have a permanent connection at the other end. It has a permanent connection at one end only, but not the other. Well, anyway, so that analogy was an interesting idea.

03-00:39:56
Burnett: Were they convinced? [laughter]

03-00:39:58
Prausnitz: I'm not sure. I'm not sure anybody's convinced by anything I say. [laughter]

03-00:40:01
Burnett: Well maybe from your own experience history of science was that cantilever.

03-00:40:04
Prausnitz: Well, that was absolutely essential. That's what's got me going. That was the most influential influence at that time and got me started.

03-00:40:17
Burnett: Now, do you think that that maybe affected your approach to thinking about plowing through the literature in the *Journal of Physical Chemistry*, and then looking at the engineering journals, or is that more a local context of the change in chemical engineering at the time?

03-00:40:40
Prausnitz: Yeah, I think the latter, because the other point, the one of C. P. Snow, is it's not one science and another science; it's the humanities and science. And that's, of course, a very difficult situation, but it's getting better.

03-00:40:58
Burnett: I wonder about your description of looking through the *Journal of Physical Chemistry*, and looking at the engineering journals, and what problems and questions did they have, and industry. And I wonder how you'd go about narrowing down the problems, because what you want to do is to look at advances in the theoretical science domain, and think about how that could generate greater efficiency in the petrochemical industry, for example. So how do you know whether a particular advancement or theoretical trail is going to yield that efficiency that you're looking for? Is that a laser focus for you? Are you concerned with efficiency, and that's where you're going?

03-00:42:04
Prausnitz: Yes. I'll give you one example which was particularly successful—not all of my examples are so successful—and that is the question of hydrates. A hydrate is sort of like ice, and hydrates form in natural gas mixtures. A natural gas mixture often has some water in it—not much—and so if the temperature is low and the pressure is high, a hydrate forms. A hydrate is a solid phase where you have, say, a gas, like methane, in the middle, and you have water around it, and it's a solid. And it's been known for years that hydrates form in natural gas pipelines, so people get natural gas out of the ground with a little water in it, and then in the pipeline every so often it can happen that hydrates form. The hydrates precipitate and plug the pipe so that gas doesn't flow. Well, this is an old problem, there's nothing new about it, but what you'd like to do is you'd like to be able to predict when will hydrates form, what temperature, what pressure, what composition is needed to form a hydrate, and then you try to avoid those conditions.

03-00:43:29
Well, people have been aware of this for a long time, and so the studies were made at various places, entirely empirical. They just would plot their data and that was it. Well, I found in the literature, the *Journal of Chemical Physics*, a theory of hydrate formation. It was a statistical-mechanical theory. And so I studied that, and I thought, well, maybe we could use this in order to contribute to the hydrate problem. It was a really severe problem in the natural

gas industry. And I had to develop this model. By "develop" I mean I had to translate it into language that chemical engineers can understand, and also had to get various parameters, various constants, and you get those from the experimental data. In other words, we fit the theory to the data by adjusting some of the numbers that are in there. So I had a very good student who worked with me on this. This was about 1970, approximately. His name hopefully will occur to me.

03-00:44:51

And so we developed this theory. And, again, when I say "develop," I mean cast it in a form where chemical engineers can use it. And so we did that, and, as I mentioned earlier, we wrote a computer program, because otherwise industry will not be so interested, but if you give them a computer program then they're happy. So this student and I, we worked on that, and we had a computer program. And in the meantime, that's somewhat out of date, but the general ideas are still used today, and every major oil company has a computer program essentially similar to the one that we developed in 1970, but modernized, and they all use this, and it's a standard technique now in the industry. If you want to avoid hydrates, you would like to know under what conditions the hydrate will form, and this computer program will tell you that.

03-00:45:55

Burnett: Well, I guess there are two questions about that when we're talking about your research, and one is about the nature of statistical mechanics, and why it's important, and the degree to which that goes hand in hand with the development of computer processing power, and the growth of computing in chemical engineering. Can you talk about those two things during this time period?

03-00:46:23

Prausnitz: Well, computing was just beginning. Now, of course, it's ubiquitous, it's just everywhere, but in the late 1960s, early 1970s, it was just really at an early stage. And, of course, it was much more complicated. You had these cards. You had to punch holes. All that is now so much simpler. But we tried to adjust to that, and that's why I wrote this monograph in 1967 or so, this monograph that I wrote with some of my students. I think it's called "Computer Calculations of Multicomponent Vapor-Liquid Equilibria." That was a real pioneering work at the time. Nowadays it's completely obsolete, but at the time it was a definite step forward. And then we made a new edition of that in 1980 where we included also liquid–liquid equilibria, and I had some very good students who worked on that with me. And I had a colleague here by the name of Edward Grens—he's not with us anymore—and he was a computer type, and so he contributed on how to make the computer programs most efficient. But with my students we supplied the thermodynamic data information.

03-00:47:55
Burnett: And so was there a mainframe here, or was there a mainframe that you had to get time on somewhere else?

03-00:48:03
Prausnitz: No, we could use the mainframes here. There were definitely— Computing on this campus has always been very good, and so we didn't have any trouble. But yes, we did have to go to one of the Mathematics buildings to do that. Nowadays, of course, you have your desktop, and we didn't have that then. So, again, these books—1967, 1980—are now completely out of date, but here it is, forty years later, and there are more modern methods, but they got it going. We sort of pioneered the way.

03-00:48:44
Burnett: Well, when did you and your students first start using computers?

03-00:48:58
Prausnitz: I guess late sixties, early seventies we started using them. Yes.

03-00:49:06
Burnett: Interesting. So statistical mechanics: you want to assess the probability of the behavior of molecules, for example. Can you talk about what this work is?

03-00:49:24
Prausnitz: Well, the important point of statistical mechanics is that it's a scale-up operation. If you know something about how two molecules interact with each other, and if you now want to predict how a billion molecules interact—more than a billion; many, many billions—then statistical mechanics enables you to do that, so a real scale-up operation. So you can generate thermodynamic properties knowing only—I shouldn't say only—knowing primarily the interaction between two molecules, so it's a tremendous scale-up. You have to know two things: you have to know how the two molecules interact as a function of the distance between them; you have to know that, and the other thing you have to know is where the molecules are. How do they arrange themselves in space? What's the structure? Now, in a gas that's fairly simple because they're all over the place, especially a dilute gas. As the gas becomes more concentrated, you approach what's called the fluid state. It could be a liquid, it could be a gas, but they're really very similar at high densities. And then you have to know something about the structure of the material. If you know something about the structure of the material, and you know how the molecules interact, you can generate thermodynamic properties. That's the beauty of statistical mechanics.

03-00:50:55
Burnett: And at this time there's all of these advances in materials science, and structural biology, in X-ray crystallography. So is there a convergence of these advances in these different disciplines that you're witnessing that permit you to do things, or is there a lag? Did you find that, well, this has been

around for twenty-five years and it's time to update chemical engineering by introducing this work? How—

03-00:51:26
Prausnitz: Well, yeah, there is a lag in the following sense: that the statistical mechanics that you generally find in books always talk about simple spherical molecules. Then life is fairly simple. But, of course, we deal with molecules that are not simple. And then, in principle, you could still use the ideas of statistical mechanics, but now you'll have to have much more information, especially about structure. If you have a large molecule, you have to know not just where the center of the molecule is, but also the various segments and the rotations and you're in three-dimensional space, so that in principal you could still do it, but you need information that you generally don't have, and the calculations become much, much more complicated, so that for practical purposes the computer is not fast enough. So you have to make approximations, and that's more or less of an art, what approximations would you make in order to be applicable to large molecules.

So there are two cases that have received a lot of attention. One is a spherical molecule that's large. In other words, it has lots of atoms in it, but it's still essentially spherical. We call it globular, [laughter] which means it's sort of like a sphere. It really isn't, but it's sort of like it. And so that case has been well worked out. And the other case is if you have a rod—in other words, essentially a one-dimensional molecule—and that's usually pretty good for hydrocarbons, until you get to really large rods, because then they begin to wiggle and bend and so on, and that, of course, causes all sorts of troubles. But a lot of progress has been made in this area, and molecules that are fairly complicated can be treated by statistical-mechanical ideas. But, again, for really complicated molecules the programs become quite complicated, and the computers are generally too slow.

03-00:53:50
Burnett: And is it true when you're dealing with refining—and I'm really out of my depth here, but—as you're refining products, the kind of products you want are these kinds of strange molecules, right?

03-00:54:08
Prausnitz: Oh, yes.

03-00:54:08
Burnett: That have these strange shapes, and they are— You need statistical mechanics, or you need that kind of analysis, to help you understand how they behave, because they are so complex in their structure.

03-00:54:20
Prausnitz: Yeah, generally, as I say, if they're really complex in structure, in principle you can do it but in fact you cannot. For these complicated molecules, you have to use more old-fashioned methods.

03-00:54:33
Burnett: Right. And I suppose what makes them desirable as fuels is that they're highly volatile, right? Is that part of it, the volatility has something to do with it?

03-00:54:43
Prausnitz: The volatility doesn't bother us. No, volatility is not a problem. The problem is that a big molecule can have many conformations. It can exist— Well, take a piece of rubber, a rod, rubber rod. You can twist it and turn it, and it's not like a sphere. A sphere has only one conformation.

03-00:55:13
Burnett: Now, you did write an article, a collaboration with C.A. Eckert and Renon from the French Petroleum Institute, and I guess Eckert's at Urbana-Champaign. And—

03-00:55:29
Prausnitz: Not anymore.

03-00:55:30
Burnett: Oh, he was, yeah.

03-00:55:30
Prausnitz: He was there. After he left here, that's where he went. Yeah.

03-00:55:35
Burnett: So he was a student of yours?

03-00:55:37
Prausnitz: He was a student. He got his PhD here with me. We became very good friends. After he'd left, he went to France for a year, postdoctoral work, and then he joined the faculty at the University of Illinois, and he was there for quite a while—I don't know, at least ten years, maybe more—and then he went to Georgia Tech. He had a professorship there, a named professorship, which is now held by my son, and Eckert retired—

03-00:56:13
Burnett: Wow!

03-00:56:13
Prausnitz: —and my son got that chair.

03-00:56:16
Burnett: That's interesting, wow.

03-00:56:17
Prausnitz: Yeah. The chair was endowed by someone with the last name of Love, so, of course, there are jokes about being the Love Professor at Georgia Tech, [laughter] endless humor. Now, Chuck Eckert retired a few years ago, and he now lives in retirement in Florida, and as far as I know he isn't doing anything at all in chemical engineering.

03-00:56:41
Burnett: And so this article is in 1967, and it's "The Molecular Thermodynamics of Simple Liquids." Is this what you were talking about with the—

03-00:56:54
Prausnitz: Yes. Yes.

03-00:56:55
Burnett: And it's the use of statistical mechanics.

03-00:56:58
Prausnitz: We used it, yes, to the extent that we could, yes.

03-00:57:01
Burnett: Right, [laughter] okay. So this was early days.

03-00:57:03
Prausnitz: With all sorts of approximations.

03-00:57:05
Burnett: Right, right. And you are dealing with simple liquids, and you don't have the kinds of problems that are associated with molecules of a more complex structure or confirmation. This is apparently—correct me if I'm wrong—the first use of the term "molecular thermodynamics"?

03-00:57:31
Prausnitz: I'm not sure that that's true. In engineering, probably so, but the real inventor, you might say, the originator of molecular thermodynamics was [Ludwig] Boltzmann, a—

03-00:57:47
Burnett: A Viennese, right.

03-00:57:47
Prausnitz: —one of the great physicists of the twentieth—no, the nineteenth century. He died in 1906, I think. He was one of the really great, great men of physics. So he really is the one who started molecular thermodynamics, but in chemical engineering the term was not really well known until about the sixties.

03-00:58:12
Burnett: And would you say there are others who were working in this domain, and where were the institutions that were working alongside you in this endeavor?

03-00:58:30
Prausnitz: At that time there were probably very few. Now there are quite a few. There was an early book on applications of statistical mechanics in chemical engineering, written by Keith Gubbins [*Applied Statistical Mechanics*, McGraw-Hill, 1973]. He was at the University of Florida, and he later on went to Cornell, and he's now at North Carolina State University. He was one of the early people, and he was certainly one of the leaders. And then there was somebody in England who was very, very good, and his name was John

Rowlandson. And I greatly admire John Rowlandson. I think he's still alive; I certainly hope so. He was knighted by the Queen. He's known as Sir John. [laughter] And he's a few years older than I am. And I'm sure there are others. I don't want to give too many names because I'm sure I'll leave somebody out. But Rowlandson was truly a leader in this area, and so was Keith Gubbins, but there were others.

03-00:59:59
Burnett: In a description of your research program at this time, there's a lot of discussion of modeling, so that you're interested in modeling vapor–liquid equilibria, liquid–liquid equilibria. "Intermolecular pair potential models for virial coefficients—"

03-01:00:24
Prausnitz: Inter, not intra. There's a big difference.

03-01:00:27
Burnett: Right, that would make sense.

03-01:00:28
Prausnitz: No, these are inter-, and that's right. We did a lot of work in the early days about second virial coefficients. That's sort of the first-order correction to gas behavior. The zeroth order is, you assume it's an ideal gas, and you then have a correction factor that is a series and a density, a power series, and the first term in that expansion is the second virial coefficient. And we spent quite a bit of time on that. That's sort of a fun thing to do, because you assume some sort of potential function, which gives you the interaction, and then there are well-known formulas for calculating the second virial coefficient. And so we did quite a bit of that.

03-01:01:21
Burnett: And so these are to account for the differences in molecular weight of gases, or the structure?

03-01:01:29
Prausnitz: Molecular weight is not the important point; it's the interactions between them, and that's determined more by the electrons that go around—

03-01:01:40
Burnett: Okay, so this is about energy. This is about—

03-01:01:41
Prausnitz: About energy, yes.

03-01:01:42
Burnett: Okay. And so the phrase that came up is the excess-Gibbs-energy model.

03-01:01:56
Prausnitz: No, no, Gibbs is one of the great scientists, sort of the inventor of chemical thermodynamics. Gibbs was at Yale all his life, and Gibbs died in 1903, and

he is the one who really showed that thermodynamics can be used for solving some chemical problems. This was not at all obvious. As the name "thermodynamics" implies—"thermo," of course, is heat, and "dynamic" means motion, so thermodynamics was the science of heat engines, locomotives. And you might say, well, what's that got to do with chemistry? And, indeed, nobody had thought that it had anything to do with chemistry, but Gibbs saw the connection. So Gibbs is the father of chemical thermodynamics, one of the really great men of American science.

03-01:03:00
Burnett: And so you were incorporating some of those models into your research, or adjusting them?

03-01:03:09
Prausnitz: My models for giving the excess Gibbs energy— The excess Gibbs energy means the Gibbs energy above and beyond what it would be if it were an ideal mixture. We know how to calculate the Gibbs energy of an ideal mixture—it's very simple—but, in fact, there are no ideal mixtures. Nature doesn't have them, except in very rare cases. And so the excess is what you have to add to take care of the nonideality, and we developed models for that, yes, and these models are still used a lot all over the world. And one of them that is very popular is called the NRTL model; NRTL is non-random, two-liquid. I won't go into the explanation. There's no need to go into the explanation. But when I was in Germany years ago at a meeting, there was a song. The German chemical engineers had made a song about NRTL. [laughter] Unfortunately, I don't remember the lyrics, but that was quite funny.

The other model that is very popular is called UNIQUAC—and that name came about from my children. After dinner I gave various words that applied to the ideas of the model, and I asked the children—they were then about—oh, I don't know—my son was about six and my daughter was about ten—I asked them to come up with a name, and so they came up with the name UNIQUAC. And that's, of course, a catchy name, so I liked that. And my colleague at MIT, Bob Reid, who was a good friend, and I remember he told me at the time, "Well, that's the first version, and now if you make a new version it'll be called the QUACQUAC model." [laughter] Yeah.

03-01:05:23
Burnett: Well, that was in '75 that you came up—

03-01:05:27
Prausnitz: Yeah, it was the seventies.

03-01:05:28
Burnett: Well, maybe we should talk about that—if we get there we can talk about that a little later. But we haven't talked about family too much, and so last time we talked you got married—

03-01:05:41
Prausnitz: Yes.

03-01:05:42
Burnett: —so things happened along the way.

03-01:05:45
Prausnitz: Well, we had two children, and, let's see, we were married in '56, and my daughter, Stephanie, was born in '62, and my son, Mark, was born in '66. So we were married quite a while before the first child arrived, and I think that was a good idea. You want to establish your marriage first and get that really settled before you have any children. I think it's a mistake to have children right away, you should really wait a while, but that's a personal feeling.

03-01:06:23
Burnett: That's interesting. For the time, wasn't it kind of a given, A, that you would get married fairly young—you're maybe right out of graduate school, or in graduate school—and you have kids? [laughter]

03-01:06:36
Prausnitz: Right. I think it's a mistake. You should definitely wait a while to make sure that you really have a stable home. But I was twenty-eight, and Susie was twenty-two, almost twenty-three; it was a week before her birthday. And by today's standards that's still pretty young.

03-01:06:58
Burnett: That's pretty young.

03-01:06:58
Prausnitz: People usually get married later, but in that time it was not young at all. It was quite normal.

03-01:07:04
Burnett: So you wanted to wait, and so you had your kids in the sixties.

03-01:07:10
Prausnitz: Right, in the sixties. Well, in fact, it was a little difficult, because I had my first sabbatical in 1962, and we spent sabbatical in Switzerland, in Zurich, at the Eidgenössische Technische Hochschule, ETH, one of the great technical schools in Europe. And so our daughter was born in Switzerland, and, well, in a foreign country things are a little different. Fortunately, Susie and I both speak German, so that was not a problem, but we have different customs, especially at that time. Nowadays everything is Americanized, including Switzerland, so it wasn't so different. No, I enjoyed Switzerland very much. It was a wonderful sabbatical. And when Susie went into the hospital for the birth it was wonderful. She had a beautiful room, overlooking the city, and marvelous care, at a very cheap rate. I think we paid ten dollars a day or something like that. It was ridiculous. [laughter] And what really interested me, too, was that the nurses and staff paid much more attention to me than they did to Susie, because I was Herr Professor, and at that time if you were a

professor that was a big deal in Switzerland. That was a rank that was highly respected. Nowadays, of course, it isn't anymore, but it was at that time. And so I would come to see Susie in her hospital room, and I was asked what kind of wine I wanted, red wine or white wine. Oh, yeah, it was great. [laughter] I was treated very well, and Susie stayed in the hospital ten days. Nowadays, of course, you stay in the hospital two or three days, but at that time it was ten days, and nobody was in a hurry, everything was relaxed. It was great.

03-01:09:29
Burnett: Good. And so this is the 1960s, of course, and Berkeley was kind of an epicenter for a lot of the political struggles and movements.

03-01:09:48
Prausnitz: It came a little later. It started in '64.

03-01:09:50
Burnett: Well, there's Students for a Democratic Society and so on, but there's some sparks of that even earlier. So I don't know how chemical engineering or engineering in general or the College of Chemistry was affected by the tumult.

03-01:10:12
Prausnitz: Not much. There were, of course, some students who were gung-ho and who participated in the various protests, but we had relatively little to do with it, and we didn't notice very much. The demonstrations were primarily a little bit down-campus, on the square there where the administration building is. And so that's a good distance away. We would hear noises, but I didn't really know what went on down there until the evening, when I would see it on television. We had some interaction. We would occasionally get a whiff of these gases that they used. What is the gas?

03-01:11:07
Burnett: CS gas, or tear gas?

03-01:11:09
Prausnitz: Tear gas, yeah. We would get a whiff of that once in a while, and then we would get telephone calls, people asked me how I am adjusting my course in view of the Free Speech Movement and Vietnam. They wanted me to change my course to take all that into account. And then occasionally there would be a parade of students coming through the halls, shouting some slogan, but not much. There was really very little.

03-01:11:46
Burnett: How would you be expected to adjust your course?

03-01:11:49
Prausnitz: Yeah, well, of course I had no idea how to do that. [laughter] How would I talk about chemical engineering properties—? I don't know, and they obviously didn't know, either, so it was an idle threat. It didn't really amount to anything much.

03-01:12:04
Burnett: Oh, so there was this sense that one should render every course political.

03-01:12:10
Prausnitz: We should be relevant. "Relevance" was the big word in those days. We've got to be relevant. Now, how to do that is not obvious. I didn't pay too much attention. I'm afraid I was not relevant.

03-01:12:25
Burnett: Well, chemical engineering becomes so relevant, more relevant, broadly, and we'll probably talk about this in more detail in the following session, but, of course, with the rise of the environmental movement, and environmental legislation begins to change and force changes in industry, and—

03-01:12:46
Prausnitz: Well, there was napalm. I don't know if you remember that.

03-01:12:50
Burnett: Yes, of course.

03-01:12:51
Prausnitz: Napalm was made by Dow Chemical, and so there was a connection there, but environment was not a focus point in the '60s. That came later.

03-01:13:03
Burnett: Well, I mean, there was Rachel Carson's *Silent Spring*, which was—

03-01:13:08
Prausnitz: When did that come out?

03-01:13:09
Burnett: Nineteen sixty-two. And so there was a burgeoning—

03-01:13:14
Prausnitz: That started it.

03-01:13:16
Burnett: —and Aldo Leopold's *Sand County Almanac* and that kind of thing. So there was something going on, especially at universities, but it takes—the late 1960s and early [19]70s you have environmental legislation at the federal level, and then the states are dealing with it, and municipalities are dealing with it, and they need scientists to help figure it out. And so I'm sure that had an impact on you in your career.

03-01:13:45
Prausnitz: Right, but then the Free Speech Movement, that was 1964.

03-01:13:50
Burnett: Nineteen sixty-four, that's right.

03-01:13:51
Prausnitz: And so at that time there was very little that I was aware of in the broad environment.

03-01:13:58
Burnett: Right. Well, that was very much capital-P politics, wasn't it?

03-01:14:05
Prausnitz: And the legislation came—that was under President Nixon. I know people don't like President Nixon here in Berkeley, but that was one of the good things he did.

03-01:14:19
Burnett: Well, creating the Environmental Protection Agency, and some of those institutions.

03-01:14:21
Prausnitz: Yes, yes, that sort of thing, yes. Yeah, opening up China.

03-01:14:26
Burnett: Yes, there's a lot of change in the air, and it will come to affect chemical engineering, and the research opportunities change as a result, but we'll talk about that another time. I do want to ask you about a couple of things. One is you have a text that comes out in 1969, *Molecular Thermodynamics of Fluid Phase Equilibria*, and it's quite a thorough text, [laughter] if I might say so.

03-01:15:08
Prausnitz: Well, yes, that's been a big success, and I think probably that helped my professional career more than anything else. That book was very, very well received, and that was a first edition. Second edition came out in the 1980s, and the third edition came out in 1999. So that's been a big success, and I worked on that first edition for about ten years. I did it all alone. In the later versions I had coauthors. But that's right, I started to take my course notes and write them up in book form. And one reason why it's successful is because I have lots of examples. I show a lot of pictures and tables. The old thermodynamics books generally don't do that, or don't do enough of that.

03-01:16:10
Burnett: No, it is very clear, and it covers a wide range of phenomena. And there's material, I think, from the papers that you'd worked on that you had—

03-01:16:22
Prausnitz: Yes, that's in there.

03-01:16:22
Burnett: —that you had developed. So in terms of doing the work, actual work practice, in the Prausnitz laboratory, what is the Prausnitz laboratory physically? What are you doing when you go to the Prausnitz lab?

03-01:16:46
Prausnitz: You bring a radio, [laughter] and you bring some lunch.

03-01:16:54
Burnett: And your brain, at a—

03-01:16:56
Prausnitz: Well, hopefully. Yes, hopefully you bring your brain. Well, we did a lot of experimental work. We made a lot of measurements of thermodynamic properties, and then we did a lot of correlational work where you take properties and try to bring them all on one plot. And that's, you see, where molecular science comes in: you've got to know what to plot it against. What's the independent variable that's important here? And so we did quite a bit of that. We had quite a few correlations that were established at that time. And one of the most powerful tools for doing all this is something called the theorem of corresponding states. I don't think you know what that is, and I don't expect you to know. The theorem of corresponding states. This is a very powerful idea that was introduced by [Johannes Diderik] van der Waals many years ago, and that is if you take the property of a material at a particular state—by "state" I mean a temperature and pressure—and then you can say that that property is identical for some other substance at a different temperature and pressure. In other words, you have material A here at a certain temperature and pressure, and you have material B here at a different temperature and pressure, and you find out, if you choose the temperature and pressure properly, that the properties of A are the same as the properties of B. That's generally the idea, and it's a very powerful idea. Very powerful. And we've used that a lot, as do other people. The theorem of corresponding states was well known already in the early part of the twentieth century, and it was greatly developed, first at MIT and then at Berkeley. Professor Pitzer in chemistry here, one of the men I greatly, greatly admired, he really worked it out thoroughly, and he prepared all sorts of correlations that I routinely use today, and the basis is the corresponding states theorem.

03-01:19:11
Burnett: Right. And when you're doing experiments and you're gathering data, you need these kind of standards against which to measure or to plot the behavior of various substances as you—

03-01:19:23
Prausnitz: Yes, yes. You plot it up in a generalized way, and this has been tremendously powerful. Again, we're not the first people to do that—others have done that—but we certainly contributed to this.

03-01:19:42
Burnett: Can you talk about instrumentation? When you're talking about the experiments, how do you do this work? [laughter]

03-01:19:45
Prausnitz: Yes. Well, the instrumentation is generally not particularly difficult. The main problem in measuring equilibrium properties is chemical analysis. You have a mixture, and you want to know what's the composition of the mixture. So analytical techniques are really what's important. And the one that we used

extensively is chromatography, and that's still done today. It's a standard way of doing things. There's the chromatography for the gas phase, gas-liquid chromatography, and there are chromatographies for the two-liquid phases. And these are instruments that you can buy. They're not cheap, but you can buy them, and we did that, and we used that extensively. Even today, we use this for chemical analysis.

03-01:20:42
Burnett: So you're working with theories that you've explored and found in the domain of physical chemistry, and you're doing experimental work to look at how this applies, and you can fit the data to the models that you have.

03-01:21:05
Prausnitz: Yes, yes, yes.

03-01:21:06
Burnett: Surprises? Were there moments when you had really thought you had discovered a relationship or a phenomenon and the data surprised you and took you in a different direction that you hadn't expected?

03-01:21:34
Prausnitz: No, I don't think I had surprises of that sort. What surprised me, and I mentioned this already earlier, is the deep, deep conservatism of the petrochemical industry. It was so deep that you might almost call it stubbornness. I didn't expect that. I had hoped that my colleagues in industry would be more open and more willing to welcome new ideas, and work on them, and that was generally not the case. All too often my colleagues in industry, they didn't want to do anything. They wanted a program. In fact, one of the interesting things when I worked at Chevron and I came up with this new way of calculating vapor–liquid equilibria, they wanted me to prepare a sheet—today we would call it a spreadsheet—where you give each set. Now you do this, now you do that, now you do this, and so on. And they had an interesting name for these sheets: they called them the idiot sheets. And that's exactly right. That's what they were. They were for idiots, where you tell them this, that. So I prepared idiot sheets, and that was something that, of course, I didn't particularly enjoy, so that surprised me. Now, on the other hand, there was one company that was very open to new ideas, and that was Air Products and Chemicals, and I was a consultant for Air Products and Chemicals for, oh, almost forty years. They were open to new ideas, and so there I had really success. They not only asked for my advice but they followed my advice. [laughter] They actually did something about it, and so that was a very happy situation. I really enjoyed that.

03-01:23:37
Burnett: Were they a comparatively small company? I guess since I haven't really heard of them I imagine that they were—

03-01:23:41
Prausnitz: No. Well, originally, of course, they were, and I don't know when they were founded—I think in the forties—and, of course, originally it was a very small company, but it grew and grew, and by the time I became a consultant for them—that was, oh, about 1960, I think—they were already quite large, and now they're a large company. Their headquarters are in Allentown, Pennsylvania, but they have plants all over the world.

03-01:24:13
Burnett: I was wondering if there's a cost to taking the advice for the petrochemical industry.

03-01:24:23
Prausnitz: Well, of course there is. Of course. That's exactly the point, and they don't want to do that.

03-01:24:30
Burnett: Yeah. It's an incredibly capital-intensive industry, and [laughter] it's physically set in steel and concrete. You have these distillation columns. If what you're saying, your advice involves building a new plant—I mean, I don't know if it would be that extensive, but—

03-01:24:49
Prausnitz: Well, I wouldn't do that, but yes, it would. Certainly there's a cost, and the cost is not so much in new equipment as in design costs. You have to make a new design, new calculations. No, equipment wouldn't necessarily be involved, but there's a human cost, I would say, probably more than anything else. You have to reorient your thinking, and people don't like to do that. [laughter] They like to keep the old thinking.

03-01:25:23
Burnett: Right. Well, the human capital investment, if so much is taken up by capital—

03-01:25:29
Prausnitz: Right, it's the human capital investment, not so much equipment, and people are not willing to do that. But Air Products was, and there was a very good spirit there, and they were interested in doing things. In fact, the way the contact was made, I was giving orally a paper at a meeting describing some of the things I was doing, and in the audience was a member from fairly far up at Air Products. He was not at the director level, but he was just below that. And he was in the audience, and he appreciated what I was saying, and he said, "Oh, this might be useful for us." And so I became a consultant for Air Products. The only nuisance about that, of course, is travel. I don't like to travel, and Air Products was in Allentown, Pennsylvania, so I had to first fly to Chicago, and then from Chicago to Air Products, and then take a taxi there to the motel that they had reserved for me. And by the time I got there, it was usually, oh, 9:00 or 10:00 p.m. and everything was closed. You couldn't get any food or nothing. Everything was shut tight, except the Coca-Cola

machine. That's about the best you could do. So I always had to take my supper with me, and—

03-01:27:03
Burnett: Allentown was a little sleepy in those days.

03-01:27:05
Prausnitz: Yeah. Nowadays it isn't. Of course, nowadays it's much more awake, [laughter] but it was very sleepy at that time. Then, of course, I lost three hours and I had to get up very early, so it was a little unpleasant. But, well, this is minor matters.

03-01:27:23
Burnett: Right. Well, is it fruitful for you? This is a relationship—

03-01:27:27
Prausnitz: Oh, yes.

03-01:27:27
Burnett: —over forty years.

03-01:27:29
Prausnitz: Not quite forty. Maybe thirty or thirty-five, something like that. Yes, of course it was. It was very fruitful, and I learned a lot. I learned a lot about how things are done in industry. I learned about some of their problems. And it was very fruitful, yes. Yes, I enjoyed it.

03-01:27:50
Burnett: So did that become part of your pedagogy? Because you had not such a great experience working in industry yourself. You decided fairly early on that the academy was the place for you.

03-01:28:03
Prausnitz: At Chevron. Yeah, I did not have a good experience, no.

03-01:28:06
Burnett: Right. But in terms of educating your students and guiding them, did you and your colleagues talk about the importance of, if not actual active collaboration with industry, the problems of industry, questions of industry? How did that fit into the formation of chemical engineering students, or did it?

03-01:28:37
Prausnitz: It did, but not much, and the way you form connections with industry is by working on problems jointly, and that is now slowly being done in a few places, but it was relatively rare at that time. Well, what helped was we would invite people from industry to come and talk. And I remember we had a visiting professor from DuPont, and he was a very well-known chemical engineer, one of the real pioneers in the area. He was quite elderly already, but he was here as a visiting professor. His name was Chilton. And he would lecture in the classroom, and he would give other lectures outside of the

classroom. So there was that, and not as much of it as I would have wanted, but there was some of that. And the thing I remember about Chilton was I think it was the first lecture he gave he would ask the audience, "What is the most important thing you have to consider when you build and operate a chemical plant?" Well, first, of course, nobody says anything, and then somebody would pipe up and say profitability, or something similar to that. And Chilton was very patient. He would listen to people saying things, and then he would say, "No, you're all wrong. The most important thing you have to worry about is safety." That made a tremendous impression on me, that here was a man who was working for DuPont—and DuPont, of course, was at that time the premier chemical company in the United States, and he wasn't talking about making money; he was talking about safety. That really impressed me very much. And I certainly have passed that on to my coworkers. So there's an example where industry came through. [laughter]

03-01:31:06
Burnett: Right. So if you're thinking about design and the incorporation of new techniques and methods, is that something that you have to consider? But I'm assuming that—

03-01:31:23
Prausnitz: No, the kind of work we do doesn't really come in very much. But yes, in principle the answer is yes. But where it really comes in is in the experimental work that students do. I want to impress upon them—I do impress upon them—that when you build some equipment and you operate it, safety is your first consideration.

03-01:31:46
Burnett: Did you have accidents?

03-01:31:47
Prausnitz: Fortunately not. There have been accidents in other laboratories, but fortunately nothing really terrible. It could've been much worse. But no, in my laboratory we never had any—no, we never had any accidents. I'm very grateful for that. And it's partly because I keep pressing upon the students, whatever you do make sure it's safe. Always wear glasses. Wear a lab coat. Don't have long hair that could get tangled up in the pump or something.

03-01:32:26
Burnett: Oh my God, yeah.

03-01:32:28
Prausnitz: Oh yeah, this happens. It can be terrible.

03-01:32:33
Burnett: It's impressed upon you whenever you walk into a chemistry building to see the emergency eyewash stations and the signs everywhere about, in a particular room, danger, radioactive, [laughter] you know, various kinds of hazards that people encounter, and protocols for when something bad

happens. It has a kind of industrial feel. You feel like you're walking into an industrial space.

03-01:32:58
Prausnitz: Well, yeah, it's necessary. Especially glasses. I'm always concerned about eyesight. We don't want anything happening there.

03-01:33:09
Burnett: Oh, of course. Well, we talked about Eckert, and we talked a little bit about Alan Myers. Can you talk about some of the students with whom you collaborated in the 1960s, late fifties and up to 1970, early seventies, that stand out to you as—? I don't want to have you rank people, [laughter] or if you don't mention them they're not considered. Let's assume that they're all extraordinary folks.

03-01:33:41
Prausnitz: I had very good relations with almost all students. There were always exceptions, but by and large my relationship with students was very good, and in many cases we formed friendships that last to this day. So one name I should mention is John O'Connell. He was here at the same time as Eckert, and we are still good friends to this day. And he certainly was very helpful in promoting the research program around here. He came up with many good ideas, and after he'd left here he went to University of Florida, and he was there many years, and then he went to University of Virginia, and he retired from there just a couple of years ago. One of the interesting things about John O'Connell was that when he was looking for an academic position he had an offer from University of Wisconsin, which at that time was probably the leading chemical engineering department, certainly one of the leading chemical engineering departments in the United States. And he turned it down. He preferred to go to the University of Florida. Why? I don't know if I'm correct or not, but he felt that Florida paid more attention to its undergraduates. Wisconsin, like Berkeley at that time, was primarily interested in graduate students, and I'm sure Wisconsin was a perfectly good place for undergraduates, but that was not the main focus. But he felt that at Florida it was the main focus. Whether or not it was, I can't judge. But anyway, that's what he felt, and so he was there. And he had a very distinguished career as a teacher. He really enjoyed teaching, and interacting with undergraduate students. He did research, of course, but that was not his main focus. So I thought that was rather courageous of him to do that, because University of Florida didn't have any particular reputation then. Even now, it's not considered one of the top schools, whereas Wisconsin was and is still considered. And—

03-01:36:20
Burnett: Well, people are attracted to different aspects—

03-01:36:22
Prausnitz: Different things.

03-01:36:22
Burnett: —that there's the three elements of the academic career: there's the research side, the teaching side, and the service side. And you're drawn to teaching, and perhaps you led by example.

03-01:36:36
Prausnitz: Oh, I loved it. I miss it now. I do some teaching but not very much. I do a lot of mentoring.

03-01:36:45
Burnett: Which is teaching, right? In a way?

03-01:36:48
Prausnitz: Oh, sure it is. But my most happy moments are when I get a student—an undergraduate, usually—at the board, and say, "Well, what about this? And what about that? And how do we look at this problem? Can you make a picture?" So on. I think I mentioned it to you already before, yeah.

03-01:37:08
Burnett: Yeah, we did talk about that: kind of one-on-one.

03-01:37:11
Prausnitz: One-on-one, and then the student gets this look, and then says, "Oh! Oh, that's how—!" The a-ha. That's teaching at its best.

03-01:37:23
Burnett: Yeah, you see the discovery that they have.

03-01:37:27
Prausnitz: The discovery. "Oh, that's how it works. Oh." [laughter]

03-01:37:29
Burnett: Right. Are there other students from that period that we should mention, or—?

03-01:37:40
Prausnitz: Well, many, of course, but one comes to mind was the name Sherwood, Albert Sherwood, and he was very theoretically oriented. And after he left here he went to Livermore, to the laboratory at Livermore, and he did very nice theoretical work there. He was very, very good. He was also older, and what really impressed me—he came from MIT, and I think he was married. Yes, I think so. And the first thing he did when he came here is he bought a house. I thought that was rather strange for a graduate student. Well, it turned out, of course, that was brilliant of him. He bought a house somewhere in East Walnut Creek or someplace, near Walnut Creek, that general area. He bought a house, and then he lived there for, oh, I guess four or five years, and then, of course, he sold the house at a tremendous profit. [laughter] So I thought that was very, very clever.

03-01:38:54
Burnett: Yes, yes. Well, the text comes out in '69, and I wanted to ask you about—this is an emerging subfield of research, and it's being undertaken at other

institutions. Were there conferences? Was there talk of a journal? Or was it not considered to be something that would be a discrete field of its own?

03-01:39:32
Prausnitz: No, I think it was recognized to be an important cornerstone of chemical engineering. There are, of course, other cornerstones; this is not the only one. But the other one is what we call rate processes. Thermodynamics does not talk about time. Thermodynamics cannot tell you how fast something happens; it only can tell you that if you leave things alone they will eventually reach a state of equilibrium, and then thermodynamics is applicable. But to get there, and how long it takes, that's out. That's a rate process, and that's another cornerstone of chemical engineering. So I think it was recognized to be a cornerstone of chemical engineering, and what we did here at Berkeley—we were not the only ones—was to modernize. The thermodynamics that existed—went into chemical engineering thermodynamics—that existed when I started out here—was very classical, and the intellectual ideas were all at least twenty years old. That would be much older, maybe even thirty, forty, fifty, but very old stuff. And we modernized it in the sense that we said, well, the reason thermodynamic properties are what they are is because of molecules. Molecules do things, and if you can get that in there, then you can really make some advances. And that's been accepted, and I think everybody agrees that this is the way to go.

03-01:41:06
Burnett: Were there efforts to build community among the other universities, or do you host conferences? Were you going to conferences that had a focus of molecular thermodynamics?

03-01:41:21
Prausnitz: Well, yes. There was an annual meeting of the American Institute of Chemical Engineers. There's also a spring meeting, but that isn't really attended very well. It's the fall meeting which is the annual meeting. It comes usually in November, early November. And at these meetings there are symposia on various topics, and thermodynamics was one of the topics that would have symposia, and that's still true today. So the meeting might have, oh, fifty, sixty symposia, something like that, and more every year, on all sorts of things. And now, of course, a lot of them are bio-oriented. That was not the case when I started out here. Bio was not considered part of chemical engineering. Now it is, very much so. But yes, there were these symposia, and, of course, I would meet people, and we would chat, and I would find out what's going on. We would go have a beer, and then I would learn who's doing what, and, of course, always the topic where can you get research money. That was always a topic.

03-01:42:45
Burnett: Definitely. Well, in the following sessions we're going to move into the 1970s and we're going to talk about the transformation of chemical engineering, and the expansion into new areas, but this is this key contribution that you and

others made in molecular thermodynamics, and it was tied initially to the petrochemical industry. I don't know if this is even a fair question, but did you find that this work kind of suffered from its own success, that you'd basically made those contributions and it reached a conclusion? Or was it crowded out by these other, newer areas?

03-01:43:43
Prausnitz: Well, I think we reached a point where we were no longer at the cutting edge. In other words, we reached a point where people had accepted the ideas, and industry uses them now quite routinely, and it's not a particularly important or fascinating topic anymore. It was up through about, oh, 1990, 1995. I'd say twenty years ago, maybe twenty-five years ago. But twenty-five years ago it was sort of saturated. There was nothing much more that you could do, and other problems became more important. One area that came out of this petrochemical application is polymers, and so we did quite a bit of work on polymer thermodynamics in the—well, we started already in the seventies and eighties. And that was quite successful. That worked quite nicely.

03-01:44:53
Burnett: Well, perhaps we'll take that up in some more detail in our fourth session, next time.

03-01:45:00
Prausnitz: Yes, all right.

03-01:45:01
Burnett: All right, thank you.

03-01:45:02
Prausnitz: Good, thank you.

Interview 4: December 12, 2018

04-00:00:20
Burnett: This is Paul Burnett, interviewing Dr. John Prausnitz for the University History Series, and we're here in Gilman Hall, and it's our fourth session, and it's December 12, 2018. And welcome back, Dr. Prausnitz. So we were talking at the end of session three about the beginning of the 1970s, and I'd like to ask you about the maturation of your research program in molecular thermodynamics. And there is a great paper that you wrote in 1985, "Abstraction and Reality: The Two Sources of Chemical Thermodynamics," and in it you give us a kind of history of chemical thermodynamics. And you talk about what we had talked about already: the way that a lot of work in chemical industries were kind of cut and try, and the methods were very rough, and that molecular thermodynamics provides you with a precise way not only to analyze chemical processes but to predict the behavior, to some degree, of molecules' interaction. You break this down very elegantly. You talk, just very concretely, about the process in a chemical plant, and that two basic things are going on: you need to know about chemical equilibria and phase equilibria. And last day we were talking about Gibbs, and the importance of Gibbs's research, and in here you talk about the importance of the Gibbs constant. So I'm wondering if you could just walk us through the important contributions—

04-00:02:40
Prausnitz: I don't know what you mean by "Gibbs constant." That is something I don't—

04-00:02:43
Burnett: Chemical potential—

04-00:02:45
Prausnitz: Oh, the chemical potential, yes.

04-00:02:47
Burnett: Yeah.

04-00:02:47
Prausnitz: Well that, of course, is one of the great advances in science. As the word implies, thermodynamics deals with heat and work, "dynamic" meaning there's something moving, like a wheel of a locomotive. Gibbs was born, I think, in 1839, so when he was first appointed to the Yale faculty, he must've been about thirty years old or so. And at that time the problem of phase equilibria and chemical equilibria was, of course, known. People knew that there was such a thing as having two phases which reach an equilibrium state, and, of course, they knew that chemical reactions do not go on forever; they stop at a certain point. That's chemical equilibria. So people knew all that. But nobody ever thought of the idea that thermodynamics could somehow be useful for talking about phase equilibria and chemical equilibria. They were completely separate topics. And Gibbs' great genius was to show that there is an intimate connection between the two, which nowadays everybody knows,

and it's taken for granted. But in Gibbs' day that was a completely new and revolutionary idea. We don't really know what went on in his mind, but what he probably was thinking about is how does thermodynamics work for solving problems in the heat and work area. And the way it proceeds is that you first have to think of how to project the problem into the mathematical world. You have a problem in the physical world. Well, how can you translate that into a strictly mathematical, ideal world where you can solve the problem? And that's exactly what Gibbs did when he thought, well, let's apply this method to chemical phenomena. And in the case of heat and work, the key ideas that were needed were energy and entropy. Those are the two ideas that you need. And so if you have a problem in the mechanical heat area, you first project it into the mathematical world with the help of these abstract functions: energy and entropy. They're abstract because you can't really see them. You can't touch them. You can't smell them. They're just ideas in the mind.

04-00:05:33

And so that's how Gibbs then said, well, what kind of an abstract quantity can we use which might be helpful in solving these chemical problems? And the concept that he devised was the chemical potential. And the once you've defined what you mean by chemical potential, then the problem of phase equilibria and chemical equilibria can be solved precisely, exactly, in the mathematical world. But then you have a final step, and you've got to take the result that you got in the mathematical world and project it back into the real world. It turns out that's the most difficult step. It's the most difficult because the first two steps have been achieved. Gibbs did that in the 1870s, so that's done, and everybody knows about it, and there's nothing more that needs to be done in those first two steps. But the last step is the hard one. The last step is, well, how do you take the mathematical result and interpret what it means in terms of real physics, in terms of real molecules. And that's where molecular thermodynamics comes in, in making this final step. So, in a way, it's really not much thermodynamics; it's mostly physical chemistry. And that's what I've been doing, and many other people have been doing it, too. [coughs] But we're doing it within the Gibbs framework.

04-00:07:07

Burnett: Right.

04-00:07:09

Prausnitz: There was one idea, in making this last step, that turns out to be very useful in chemical engineering. And this last step was made by Gilbert Newton Lewis, who was for thirty years the Dean of the College of Chemistry at Berkeley. This was before he came to Berkeley; he was at MIT at the time. He came up with the idea of a quantity called a fugacity. And the fugacity is helpful for making the third step. It doesn't solve the problem, but it's a useful technique to get you started. And the fugacity, as the name implies, means the escaping tendency. If some fluid, say, has a high fugacity, it means it's unhappy where it is and wants to get out. If you have a low fugacity, that means you're perfectly happy where you are; you have no particular wish to leave. And the good thing about fugacity is that the units of fugacity are pressure units, and pressure is something that we can relate to. We understand what pressure is because it's part of our daily experience. And so say the fugacity is one tenth of a millimeter of mercury; we know that's very low. On the other hand, if the fugacity is a thousand bars, we know that's very high. So we have a physical ceiling for what fugacity is, and we don't have that physical ceiling for the chemical potential. When I was in Hawaii some years ago, I was enjoying it very much. I was on the shore there. The palm trees were swaying. I had a book of poetry I was reading, and I had a glass of some drink; I don't remember what it was. It was really wonderful, and I thought, well, I better write a little postcard to my coworkers at Berkeley. And I wrote, "I am here in Hawaii and my fugacity is very low." [laughter]

04-00:09:34
Burnett: That's great!

04-00:09:35
Prausnitz: So I think that helps understand what fugacity really means.

04-00:09:42
Burnett: Well, I don't want to interrupt your stride as we're moving towards this, but this is something we should talk about perhaps later, is the way that you explain things—and maybe this is influenced, in part, by G.N. Lewis—almost anthropomorphizing, as one of your colleagues said, anthropomorphizing the molecular behavior, that something *wants* to do something, or is *afraid* of doing something, that they're almost sentient beings. And that's a really vibrant way of illustrating something that's highly abstract and highly technical, and that's, I think, the teacher in you coming out.

04-00:10:21
Prausnitz: Yes. Yes, I enjoy doing such analogies, and I agree, they're very helpful in teaching. Yeah.

04-00:10:29
Burnett: So we were talking about how G.N. Lewis developed this concept of fugacity, and that was useful. In moving towards the molecular thermodynamics of the fifties and sixties and into the seventies, so the Gibbs function or the Gibbs—yeah, it's the Gibbs function—is useful in understanding the relationships between pressure, volume, temperature, and energy, and so on, in order to understand chemical equilibria and phase equilibria. Can you talk about what you were involved in, in further developing the theory getting into the 1970s?

04-00:11:29
Prausnitz: Well, first of all, most of my work was in phase equilibria. I did some work in chemical equilibria, but that was much less. And I think the reason for that is that the phase equilibrium problem is much more important in chemical engineering. I'm not saying that chemical equilibria are unimportant, but I'm saying, relatively speaking, in designing a new plant or in making calculations for various operations within the plant, the problem of phase equilibrium

comes up again and again and again, and is a much more common problem than the problem of chemical equilibria. So I spent most of my time on the phase equilibrium problem. I don't think I did anything on the fundamental theory. I never contributed anything to that. The theory of thermodynamics or phase equilibria has been known for many, many years. There's nothing really more that can be done. I was concerned with how we interpret the mathematical results that Gibbs gave us. How can we interpret them along molecular lines so that we can make some predictions without having to do a lot of work in the laboratory? That's the main idea of a prediction: you want to make predictions so that you don't have to go to the laboratory and make measurements. Making phase equilibria measurements is not so easy. It can be done, of course, there's all sorts of equipment for doing it, but if you want to get reliable answers you have to do it very carefully. You have to have some experience, and you have to be very patient. Well, of course, this is true of any experimental work. You've got to do it right, and you'd like to avoid going to the laboratory. You'd much rather do a calculation, or, best of all, if you have a computer program, you push the button. That's what people in industry really like to do. So I spent my time accordingly.

04-00:13:41
Burnett: Well, if I may interrupt here, there's a good rationale for industry. This comes up in a number of areas where the abstract mathematical work is important. It's especially important in domains where there is a problem of massive investment of capital to build a brand new plant from scratch, billions and billions of dollars, and if it's done wrong it's costly, right? The other aspect of it is safety, which is not insignificant for the chemical industry. I think wasn't—

04-00:14:21
Prausnitz: Well, also environment.

04-00:14:23
Burnett: Yes, which—

04-00:14:24
Prausnitz: You want to make sure that you don't pollute any more than absolutely necessary, so that's another restraint that you have to work with.

04-00:14:31
Burnett: Right, right.

04-00:14:32
Prausnitz: Yes, that's exactly right.

04-00:14:33
Burnett: Absolutely. Can we talk about—if you could walk through a contribution you made, say, from some work in the sixties, or into the 1970s, because it's often said that the work that you did, you were almost an unwitting

environmentalist, [laughter] in that you made contributions that, when taken up by industry, resulted in these efficiencies.

04-00:15:05
Prausnitz: Yes, efficiency is not only with regard to pollution, but also with regard to energy. You want to minimize energy. I don't remember the exact details, but somebody once calculated how much energy is used in the chemical industry, and it is very substantial. It's by no means negligible. It's a huge, huge amount. So if you're trying to conserve energy, the chemical industry's one place where you should look to see how you can do that. And, of course, a lot of people are doing that; there's been a lot of work done in that area. So that was of concern to me. Of course, in the sixties and seventies people didn't worry much about energy. Energy was not a cause of worry like it became later. And similarly with pollution, yes, there was some concern, but not nearly as much as came in later years. So yes, I guess it's correct that I was an unconscious environmentalist. [laughter] I didn't realize it, but yes, I think there's probably a lot of truth in that remark.

04-00:16:19
Burnett: So when these methods get taken up, you can take a real example, if you know of one, or a kind of hypothetical example of the kind of gains that you get from applying some of the molecular thermodynamic principles that you're working on to a particular industry, say the petrochemical industry. How much can you get from introducing a number of these changes? How much of a savings can you make, energy-wise?

04-00:17:00
Prausnitz: Well, I don't think I can give you any numbers. I just don't have those numbers. But by using some of the correlations that I presented, you can calculate with a lot more confidence how much reflux you need. Don't ask me to explain reflux.

04-00:17:21
Burnett: [laughter] Okay.

04-00:17:22
Prausnitz: Reflux is, in a distillation column, the material that comes out at the top. Some of it is returned to the column, never mind why.

04-00:17:35
Burnett: No, that makes sense, yeah.

04-00:17:36
Prausnitz: And that is, of course, an expense, because that means that if you return a lot to the column, that means high reflux. Then you've got to keep vaporizing it again and again. Now, you need to do that in order to get a good separation, but you'd like to keep it to a minimum. Furthermore, you'd like to use as few stages as possible. Each plate in the distillation column is an equilibrium stage, and sometimes you need a tremendous number. And, of course, they're

expensive, and so you would like to keep that number as low as convenient. And I think the correlations and techniques that I published helped to do that, helped to lower the reflux, and to lower the number of plates. How much of a saving that is, I don't know, but I suspect it's far from trivial. I don't have numbers, but it's certainly significant.

Now, one of the more successful examples that I had was there was a design made by some people in the hydrocarbon area where they had a composition of the product high—it was a good separation—but when they actually built the plants and did the distillation column, it turned out to be completely opposite: they got on the top not the product that they wanted but the product that they wanted to reject. And so this was due to the fact that they were making errors in the calculation of the phase equilibria. By "errors," I mean they didn't really know what the phase equilibria were. They did their best guess on the basis of techniques that they had, and then it turned out that it went the wrong way. And if they'd used my correlations, which I guess they didn't know about, then it would've come out the way they wanted, so that was kind of nice.

04-00:19:54
Burnett: So "phase equilibria" would mean that given a certain reaction, with a given set of parameters of temperature and pressure and so on, that you're going to get this amount of a gas and this amount of a liquid of a given substance.

04-00:20:10
Prausnitz: Well, it's the composition you're worried about.

04-00:20:12
Burnett: Okay.

04-00:20:12
Prausnitz: You want the composition of the gas to be significantly different from the composition of the liquid. That's the whole basis of the separation. And then you take that gas that comes up, and then you can condense that, and you do the next stage. You condense it, make a liquid out of it, and then you vaporize that one, and you get another gas. You do this in stages, and if you have enough stages and enough reflux, then you can get a pure product—not totally pure, but, say, 99 percent pure. That's the whole basis of distillation. Of course, that's not new; people have known about that for a long time. But you have to have reliable phase equilibrium information if you want to design with confidence, and that's where my work was helpful.

04-00:21:03
Burnett: So it's not only a question of the way the industry was doing things before was kind of ballparking, or using customs: this is how you do it, and there's a procedure, and because it's worked this way in the past relatively well, it's good enough? But they can also potentially get things upside down!

04-00:21:29
Prausnitz: Yes.

04-00:21:29
Burnett: From your example it sounds like they can.

04-00:21:30
Prausnitz: Yes, yes, yes, that's right. That doesn't happen very often, [laughter] but it can happen. And what people did at that time—they still do, to some extent—is they rely on experience. They know, well, in the past such and such was done, and that seemed to work well. That'll probably work here, also. It was a lot of that idea. And nowadays we still have that, but a lot less. Nowadays people really want to make calculations, and not just sort of wave their hands and say, well, this is probably okay. As I said, there still is some of that, but a lot less.

04-00:22:06
Burnett: Now, when it comes to the 1970s, what comes to mind immediately is the energy crisis of 1973, and, again, another one, in 1979, and concerns about pollution. Rachel Carson, *Silent Spring*, is 1962. There's all of the back-to-the-land movements, and new social movements around prioritizing responsibility towards ecosystems, maintaining wildlife areas, and so on. And the government responds by creating the EPA in 1970. There's the Clean Water Act. There's a lot of legislation, and government is now being tasked with assessing the polluting potential, or actual performance, of various industries, especially the chemical industries. So DDT was the big culprit in the *Silent Spring* case, but Agent Orange and a number of other chemicals that were being dumped around towns that people lived in [were also in the limelight]. This was the political and social climate in the early 1970s. How was that received by Chemical Engineering, the department, and was there funding available? Because these are state agencies, were they able to say, well, here's a pot of money if you want to explore pollution? How did that play out in your experience?

04-00:24:04
Prausnitz: Well, it played out relatively little here in the Berkeley Chemical Engineering Department. There were quite a few chemical engineering departments throughout the country who changed their name; they are called Department of Chemical and Environmental Engineering or something like that. It's still true today. For example, the chemical engineering department at UC Riverside has the word "environmental" in its title. We never did that here. At that time, more than today, certainly, there was a certain, what I call "spirit of purity." We didn't want to be affected by such matters. We were scientists, and we wanted to do science, and if the science helps the environment, well, that's fine, but that was not really our primary goal, at that time. Things have changed, of course, tremendously. I remember there was a dry cleaning outfit, clothing dry cleaning, and they were concerned about the vapors that come from their operation, and these vapors were just dispersed into the air. They had a little chimney, and they went out into the atmosphere. And they were

told they can't do that anymore; they've got to stop doing that. And so they came around here and said to us, "Would you like to help us on this?" And we were very snooty at the time. When I say "we," I don't mean myself necessarily, but the spirit of the department. We sort of laughed, ha-ha, chemical engineering for dry cleaning. That was sort of a joke, and today it wouldn't be—at any means, we'd be very happy if the dry cleaning industry would come around and fund some research—but at that time that was considered beneath our dignity.

04-00:26:11
Burnett: Was it because the scale of that wouldn't—? You'd think that would be quite—although small business, taken in totality that's a significant industry.

04-00:26:18
Prausnitz: Yes. Oh, yeah, it would have a tremendous influence, and I think it has been picked up. There were various processes devised to do that, and really classical chemical engineering. But it was our attitude at the time. It struck me that we just sort of laughed it off. Today, we wouldn't do that. [laughter] We wouldn't laugh it off.

04-00:26:49
Burnett: Well, I guess there were—

04-00:26:54
Prausnitz: Well, let me say something about the seventies, because there was a major step forward in my laboratory at that time, and that is I had read that, oh, in the late twenties there was a chemist, well known chemist at General Electric, who suggested the idea that maybe you could correlate the information on activity coefficients by dividing a molecule into groups and then talking about group–group interactions to predict what the activity coefficient should be. This was about 1928. But the GE people never followed up on it. There was one article, and that was all. But I did know that Shell, here in Emeryville, the Shell laboratories, they were working on something like that. And the reason I knew that is because one of my crew workers had a summer job at Shell, and so I learned that they were doing this. And it seemed to me this would be a very nice idea. It would really help a lot, if you could make it work. And, of course, Shell had a program for doing it, but it was strictly proprietary. They didn't publish it, and they kept it only to themselves. So I thought I would look into that, and that work was done in the mid-seventies. And the correlation that came out of it is called UNIFAC [UNIQUAC Functional Activity-Group Coefficients]. I don't remember now exactly why this name was chosen, but AC means activity coefficient. So in UNIFAC the AC is activity coefficient, and the UNI comes from universal or something like that.

Anyway, we worked on that, and that hit the profession like a storm. They loved it. It's exactly what people loved to do in industry: you just tell the computer what your molecules are; the computer divides the molecule up into groups; the group–group interactions are tabulated; the entire calculation is done by the computer, so after you give the computer the minimum information it will predict what the activity coefficients are; and, of course, the activity coefficients is what you need to calculate vapor–liquid equilibria.

04-00:29:39
Burnett: So this is "The group-contribution estimation of activity coefficients in nonideal liquid mixtures." This is a 1975 paper in—

04-00:29:48
Prausnitz: Seventy-five, right. And that was probably the most successful thing I've ever done, successful in the sense of industry reception. From a scientific point of view it's not particularly important, but it was a great hit. [laughter] And I had a postdoctoral visitor here from Germany by the name of Gmehling, and he loved this, and when he went back to Germany he spent the rest of his career working on that, perfecting it, and getting all the various group–group interactions. So he picked that up here and ran with it. He became a professor of chemical technology in one of the German universities, and he had a laboratory, made some measurements, but his main contribution is he did a vast survey of the literature, so anybody who had ever measured anything remotely similar to vapor–liquid equilibria, he found out the data and used the data to get these group–group interactions. So that was very nice. I was very happy that he followed up on it, because I didn't want to do it. I was happy to present the original idea and show that it can be reduced to practice, but then I'd just as soon let it go, let somebody else do all the details, and that's what happened. Gmehling worked on this. In fact, he's now retired but he's still working on this. He has a small company where they're still doing this.

There's only one objection that I have to this whole thing, and that is that Gmehling made a program with all these numbers, but it's all proprietary. If you want to use Gmehling's program, you have to pay. And that rankled me. I don't think that's correct. I don't think you should use university research to make people pay, but obviously I'm a minority with that view, [laughter] and most of my colleagues don't feel that way.

04-00:32:01
Burnett: Well, we will need to talk about that shift, and we may get there in this session, where the university becomes a different place while you're working at it, [laughter] especially with respect to intellectual property. And the full name of this professor is—?

04-00:32:25
Prausnitz: Gmehling. And I have his picture up here on the wall. He's been retired now for a couple years.

04-00:32:36
Burnett: Right. Now, this paper, "Group-contribution estimation of activity coefficients in nonideal liquid mixtures," is cowritten with Aage Fredenslund?

04-00:32:49
Prausnitz: Yeah, it's pronounced "Oh-huh." It's Danish.

04-00:32:54
Burnett: Aage Fredenslund and Russell L. Jones.

04-00:33:00
Prausnitz: Right.

04-00:33:00
Burnett: Were these students of yours?

04-00:33:02
Prausnitz: No.

04-00:33:02
Burnett: No, okay.

04-00:33:03
Prausnitz: Jones, he was a student here, but he didn't work with me. I forget who he worked with. But anyway, his thesis is on a completely different area, so he did this just for the fun of it, so to speak, in order to broaden his education. And Fredenslund was a visitor, and he was a professor at the Technical University of Denmark. And he was here visiting, and he was intrigued by this whole idea, and so he worked on it with Jones and with me, and that's how this paper came about. Fredenslund then later on developed further, and there are some papers by Fredenslund in subsequent years, which talk about this. He unfortunately died early, so he's been gone now many years. I liked Fredenslund very much. He was a very, very pleasant person. When I visited Denmark, he drove me and my wife around, and he had a very interesting technique. When it was time for lunch at some little town, he would go to the hotel there, the major hotel in the city, and he would ask, in Danish of course—he wanted to show he's not a foreigner—he'd ask, "Where is the second best restaurant in town?" And I said, "Second best?" "Well," he says, "there's no point asking which is the best because the hotel will say right here, our restaurant is the best," and, of course, he doesn't necessarily believe that, so he asked which is the second best. And the second best was still very good. It was still perfectly all right. [laughter]

04-00:34:59
Burnett: And maybe the best, who knows, depending on their reporting, right? And there was another paper, and I think we started to talk about this a little bit last time, earlier that year in 1975, "The Statistical Thermodynamics of Liquid Mixtures." Is that part of the same research project? Because it involves—

04-00:35:15
Prausnitz: No.

04-00:35:16
Burnett: No?

04-00:35:16
Prausnitz: No, no, that was a different purpose. The idea there was to develop some reasonable molecular theory for representing data, with a minimum of arbitrary constants. So we developed a technique—not a technique; a method, I guess, an equation—for correlating data, and I think that was called the UNIQUAC method.

04-00:35:46
Burnett: Yes.

04-00:35:48
Prausnitz: And so UNIFAC is really sort of a derivative of UNIQUAC. And that has been used throughout the world. The UNIQUAC model is really very useful. It covers a large, large variety of mixtures, including polymers. If you have a polymer dissolved in a liquid, and you want to know the vapor–liquid equilibria, UNIQUAC is a useful method for doing that.

04-00:36:15
Burnett: You call it a "universal quasi-chemical equation."

04-00:36:20
Prausnitz: Yes.

04-00:36:22
Burnett: And just now you said "theory," and before you had said that you didn't do anything theoretical, so—

04-00:36:29
Prausnitz: Well, I didn't do anything theoretical in thermodynamics, but we did a lot of theoretical stuff on what I called the last step, the last step being how do you relate the chemical potential to physical properties? That's not thermodynamics; that's physical chemistry. Oh, we did a lot of theoretical work in that area, but the theoretical work in getting to the result of the chemical potential of a substance must be the same in both phases. That was Gibbs' result. We didn't touch that; that's been known for a hundred years, more, and we didn't do anything there.

04-00:37:06
Burnett: Well, so then these are two of your "greatest hits" in terms of the theoretical contributions to the problem of phase equilibria.

04-00:37:21
Prausnitz: Those two, and then the other one I think we talked about before, with Alan Myers, on adsorption of mixtures. That was a great big hit, too. That worked very well. I guess in those years, those were the most popular results.

04-00:37:36
Burnett: Now, do you feel that this research program, that was the peak maturation point of this set of questions and problems?

04-00:37:50
Prausnitz: Yes.

04-00:37:51
Burnett: Mid-seventies.

04-00:37:52
Prausnitz: Mid-seventies. Well, yeah, let's see. The one with Alan Myers was mid-sixties—

04-00:37:57
Burnett: Yeah, that's right.

04-00:37:58
Prausnitz: —but UNIQUAC and UNIFAC was mid-seventies, yeah. Yeah, we started to do quite a bit on polymers, and, again, we had correlating methods whereby you can sometimes predict with reasonable accuracy—not perfect—what the phase equilibria are if you have a polymer in your liquid.

04-00:38:19
Burnett: Right. Is that trickier, working with polymers?

04-00:38:22
Prausnitz: It's not trickier, but you have to allow for the fact that in a polymer you have a very large molecule, and you have to take note of the fact that it's large. That has some consequences, and you have to make sure that they're included in the theoretical result.

04-00:38:43
Burnett: And so in doing that important research, in getting the—the uptake was fairly immediate? These are citations in the thousands for some of these papers, right?

04-00:39:01
Prausnitz: Yeah.

04-00:39:01
Burnett: These had really significant impacts.

04-00:39:04
Prausnitz: Yes, yes.

04-00:39:04
Burnett: And they are apotheosized and celebrated a bit later, and there are papers in the late-eighties and early-nineties talking about the impact, perhaps because only with historical hindsight you could see how big an impact it had, or at the time was there a big splash?

04-00:39:25
Prausnitz: Well, it took a while. Obviously people don't immediately adopt something; they have to be convinced that—well, first of all they have to know about it,

and so I went to many meetings, and I talked about these ideas, and, of course, the students went to the meetings, presented papers on the basis of their PhD thesis. So first of all you have to get the word out, and then there's a period where people tested to see if they're really any good, and so on. So that takes a few years, but within a few years it was adopted quite extensively. There's one other paper I should mention, which was also very successful, and that has to do with hydrates. A hydrate is sort of like ice, except that in the ice crystal there's a central molecule, like methane, and so the water molecules form a cage around the methane. It's a solid. It looks very much like ordinary ice, and it has similar properties. Well, now, I don't know if I mentioned it last time, but—

04-00:40:40
Burnett: Was this about the pipeline?

04-00:40:42
Prausnitz: Yes, this is the pipeline.

04-00:40:42
Burnett: Hydrates forming in the pipeline.

04-00:40:43
Prausnitz: That's right, the hydrates. And so I don't remember—I guess it was early-seventies—this was a paper by one of my students named Parrish. I think it was published 1972; I'm not certain. William Parrish. And we showed there that if you take a theory that had been published a few years earlier—not to me; the theory came from somewhere else—and if you then apply that theory to the data—and the data are quite extensive; people have made measurements with hydrates many cases—you can generalize it, and correlate it, and you come up with a computer program which tells you at what temperature and pressure you might expect to form hydrates. You want to avoid that. You don't want hydrates. And that was a big success, and it's now, of course, out of date. We have a lot more information now than we did then, so the program has been revised. New constants have been put in, not by us but by others, especially there's a group at the Colorado School of Mines in Golden, Colorado. They worked on this, and there are now new methods—well, the method is the same, but the details are updated. So that's been a big success, and everybody uses that throughout the world. If you are concerned with hydrates, this is well known.

04-00:42:20
Burnett: So you've mentioned several times: are these data-intensive calculations? Do you need a computer, or is it just much easier to work with—?

04-00:42:28
Prausnitz: In most cases it would be very tedious if you didn't have a computer.

04-00:42:31
Burnett: Yeah. And you're not so interested in that side of things.

04-00:42:35
Prausnitz: No, I'm more interested in physical chemistry. [laughter]

04-00:42:38
Burnett: Right. So the higher-order thinking that goes into conceptualizing the problem and then working towards a solution, and then you partner with, it could be students, it could be other colleagues who have a—

04-00:42:52
Prausnitz: Yes.

04-00:42:52
Burnett: —real enthusiasm for the programming.

04-00:42:54
Prausnitz: Yeah, right, who like to program, [laughter] which I don't want to do.

04-00:42:59
Burnett: And then that creates something that, as you said, the industry people can plug numbers into and they can get the result at the end, and they're satisfied. So you did mention talking to someone that you knew at Shell, and posed a problem that was based on a paper that was produced out of GE in the twenties. And it started me thinking about the source of ideas, the source of problems. For you, does this typically come from industry? In this period, at least, does it typically come from industry? For example, trying to work on this question of hydrates and pipelines? Or is that where inspiration comes from, for you?

04-00:43:50
Prausnitz: Well, yes, I first find out what the problem is that people are interested in, and then I look around to see is there anything that the physical chemistry people have done that is appropriate, that is relevant. And then I take that theory and develop it. There's a big gulf between an article that you find in the *Journal of Physical Chemistry* and what industry needs. There's a lot of work, a lot of creativity in getting from one point to the next. First of all, the people in the industry had probably never heard of this particular article in the *Journal of Physical Chemistry*, but even if they'd heard of it, they see right away they can't use it. A lot has to be done before you can use it. And so that's what I do. But, again, I think I mentioned the word "translation."

04-00:44:42
Burnett: Yeah.

04-00:44:42
Prausnitz: I translate what physical chemists have done into useful chemical engineering knowledge.

04-00:44:49
Burnett: And that's a byword. Now there are whole institutes with "translational" in the title, and this is something that you were doing from the get-go, essentially.

04-00:45:00
Prausnitz: Right, right. That's right. It's popular now, translation, yes. [laughter]

04-00:45:05
Burnett: So when you're—

04-00:45:07
Prausnitz: Makes me think, though, of that movie. Remember the movie *Lost in Translation*? There was a movie about that.

04-00:45:13
Burnett: Yes, [laughter] that's right.

04-00:45:15
Prausnitz: So there's always that danger that something may get lost.

04-00:45:18
Burnett: Right. No, absolutely. Well, so when you were in your teaching mode, and advising graduate students, and evaluating theses, and talking with your colleagues, is that a refrain for you? Are you saying, where's the engineering? Where's the application? Where is the—?

04-00:45:39
Prausnitz: That's always in the back of my mind, certainly.

04-00:45:42
Burnett: Yes, because we talked about the identity of chemical engineering and how it was changing, how you helped change it in the 1950s to make it more connected to what was happening in physical chemistry, to render chemical engineering not more pure but derived from pure research in chemistry. Is there a point at which you feel it might have gone the other way, that chemical engineering takes up that mantle and is really going towards the fundamental, the basic research, and that some research begins to get lost in that, or at least it stays there, it doesn't go into the applied world? Do you find yourself becoming an advocate the other way towards applications? And if so, when does that happen for you, roughly?

04-00:46:50
Prausnitz: Well, I don't know what you mean by "the other way." [laughter]

04-00:46:54
Burnett: So you begin your career in this field that was considered to be lesser than, quote-quote, "real chemistry" or "physical chemistry" or "basic chemistry." And they [chemical engineers] were just the hewers of wood and drawers of water for the petrochemical industry, and they were just doing engineering work, old school, and you brought to the department, along with others, this eye to basic research; always thinking, what's happening in basic research, and how can we translate that or apply that to the engineering domain?

04-00:47:40
Prausnitz: That's right, that's right.

04-00:47:42
Burnett: But if it went the other way, is there a point at which non-applied research, basic research is being done in chemical engineering?

04-00:47:53
Prausnitz: Oh, yes. And, of course, what really pushed that is the book by Bird, Stewart, and Lightfoot. I don't know whether we talked about that before.

04-00:48:01
Burnett: Yeah, the "BSL." [laughter] Yeah, the—

04-00:48:04
Prausnitz: And that came out in the early sixties.

04-00:48:06
Burnett: *Transport Phenomena*.

04-00:48:07
Prausnitz: That's right. And that book made a really tremendous effect on chemical engineering education and research. And that certainly had a huge effect here in Berkeley. So I certainly did feel that way, and I'm afraid I offended many people. They would come and tell me all the wonderful things they were doing, and I'd say, "Well, you're a chemical engineer. Where's the chemical engineering here?" And they would be offended, so I stopped doing it that way. [laughter]

04-00:48:48
Burnett: But you felt—

04-00:48:48
Prausnitz: There was a spirit that if you do good science, that's what you should be doing. And I never felt that way.

04-00:49:02
Burnett: And when was this, roughly?

04-00:49:03
Prausnitz: I believed you should do science, but you should do science with a goal.

04-00:49:08
Burnett: Right, a goal of—

04-00:49:09
Prausnitz: Of chemical engineering.

04-00:49:11
Burnett: Of chemical engineering, right. And so after *Transport Phenomena* you found that colleagues and students, in the sixties, even, were moving towards this "we're going to do pure chemistry here."

04-00:49:26
Prausnitz: Or pure fluid mechanics.

04-00:49:28
Burnett: Or pure fluid mechanics.

04-00:49:31
Prausnitz: Yes.

04-00:49:32
Burnett: Right.

04-00:49:32
Prausnitz: And I didn't think that was right.

04-00:49:34
Burnett: So I want to open an area of discussion around sources of support. We've talked about industry support. Sometimes NSF was in the mix. And another way of looking at it is your support of government research, right? Helping the government figure some things out in different domains. And so one of those areas was for the National Bureau of Standards in the mid-sixties. I'm wondering if you could talk a little bit about that.

04-00:50:21
Prausnitz: Well, at that time the National Bureau of Standards, at least the particular division with which I was associated, they were pretty pure, and I did some work with them on thermodynamic properties of materials at low temperature. Low, I mean really low, not temperatures that you get in your refrigerator but much lower than that. And that was very nice. I enjoyed that. But then the National Bureau of Standards became the NIST, the National Institute of Science and Technology, and with that name change also came a change in the whole orientation of the bureau. They also were told never mind all this pure science stuff; do something useful and practical. And I was, of course, glad to see that, but after the cryogenic period I don't think I did anything more for them.

04-00:51:23
Burnett: Okay, so this is cryogenic research and—

04-00:51:26
Prausnitz: Yeah, it was cryogenic properties of materials at low temperature. And we published quite a few papers on that. There were, oh, I would say about a dozen papers in the sixties, maybe early seventies.

04-00:51:40
Burnett: One of the things that happens, and I alluded to it earlier, the energy crisis, and the state response, the federal government under the Carter administration—you've got the Department of Energy in 1977—and a lot of initiatives towards—I think especially in California. Wasn't there also the State of California was interested in that?

04-00:52:08
Prausnitz: Oh, yes, they were interested in that, but I never had any direct connection with state agencies.

04-00:52:15
Burnett: But state agencies can set the tone for research, at least a direction.

04-00:52:25
Prausnitz: That's right.

04-00:52:25
Burnett: If money is forthcoming—

04-00:52:27
Prausnitz: That's right, yeah.

04-00:52:28
Burnett: —that can steer the ship in a new direction.

04-00:52:31
Prausnitz: Right.

04-00:52:31
Burnett: And so one of the consequences was that either alternative fuels, or taking existing polluting fuels and figuring out how to do it differently so that it produces a cleaner product [became important].

04-00:52:45
Prausnitz: Right, and that's done with catalysis, and we have people here doing catalysis, and they certainly were stimulated by that, with the exhaust from automobiles, with the NOX exhaust. And they're still working on that now.

04-00:53:06
Burnett: Right. And out of some of that, they developed catalytic converters and that kind of thing—

04-00:53:11
Prausnitz: That's right.

04-00:53:11
Burnett: —in motor vehicles, as—

04-00:53:13
Prausnitz: And that's been well represented. [break in audio]

04-00:53:22
Burnett: So we were talking about the changing climate of research, looking at alternative fuels, looking at pollution, and those kinds of things, and the response from your department.

04-00:53:38
Prausnitz: Well, the response, as I mentioned a moment ago, was in catalysis, where people thought of ways to do something about the exhaust gas from car, and what kind of a catalyst you would want. And that sort of work was done here, primarily, I think, by Professor Bell. But with regard to pollution, otherwise not very much was done here. The civil engineers did most of it, are still doing it.

04-00:54:10
Burnett: Civil engineering at the University of California Berkeley.

04-00:54:12
Prausnitz: At Berkeley, oh, yes.

04-00:54:13
Burnett: And there was one other area, though, in the late seventies the Carter administration is willing to support some of this research. Can you talk about one area that was explored that you were looking at, and what happened to that research program?

04-00:54:42
Prausnitz: Well, I was looking at recovery of valuable chemicals from liquefied coal. We didn't work on the liquefaction part of it, although many people in industry did. I was a consultant for Air Products. They had a huge program in Colorado—I think it was based there—on coal liquefaction. They spent a tremendous amount of time and money on it. But then when Reagan came in, it just stopped from one day to the next, and it was very difficult for Air Products. I think they lost a lot of money on that, but I'm not sure of the details. But certainly it was very difficult for us. We had these students here who were working on research toward their PhD, and suddenly the support stopped, so that was very bad. But we published a few papers on after you liquefy the coal how do you get valuable chemicals out of it. This is what we were concerned about.

04-00:55:48
Burnett: It's often said these days that clean coal is a bit of a myth, that the economics are just not there. How do you feel about that, knowing that the research program never got off the ground fully?

04-00:56:03
Prausnitz: Well, I think there still is some work going on. I think in West Virginia, at Morgantown, there's a DOE [Department of Energy] laboratory, unless it's been shut down, and maybe under President Trump maybe they stopped it. I don't know. But there was some work, but not very much after Reagan came in. How do I feel about it? Well, I don't know enough about the economics to make any real statements, but certainly it's true that coal is fundamentally bad because the carbon-to-hydrogen ratio in coal is high. In other words, for every atom of carbon, there is a little more than one atom of hydrogen, and so when you burn coal you make a lot of CO_2, in contrast to natural gas. In natural gas,

it's mostly methane, and for each atom of carbon you have four atoms of hydrogen, so when you burn natural gas you make a lot of water, but water is harmless, water doesn't bother anybody, and you still make CO_2 but much less than you do with coal. Now, that's a fundamental fact in chemistry you can't do anything about. You can clean up the coal in the sense of removing all sorts of impurities, and getting the little particles out with a filter of some sort, but you can't change the basic carbon-to-hydrogen ratio. That's fixed. So from that point of view, coal is very bad, and I think eventually we probably will get away from it, but in the Far East, in China and Indonesia and Vietnam, they still build coal-burning plants, and, well, that's not good, but they do that because it's economically favorable, and so it's hard to argue with it.

04-00:58:15
Prausnitz: My view is that we should go much more into nuclear. I know it's unpopular, and I keep having arguments with my wife about it. It's against the political grain. But I think the advantages of nuclear energy are very high. Of course there are disadvantages—nothing ever exists that has no disadvantage—but from a safety point of view the old power plants are much more dangerous than the nuclear plants. If you look at history, nuclear plants have really done very, very well. Now, there are cases where it went bad, yes, but by and large it worked out very well. And I think more than 50 percent of the power generated in France is done with nuclear energy, and, as far as I know, there's never been any problem in France. It works very, very well.

04-00:59:20
Burnett: Yes, as they've committed to it, and they've been able to make it work.

04-00:59:23
Prausnitz: Yes, and they make sure that all the safety procedures are obeyed, and that's what you have to do. You have to, of course, be careful—

04-00:59:31
Burnett: Right. [laughter]

04-00:59:32
Prausnitz: —and protect yourself from possible tsunamis, which they didn't do in Japan. They missed out on that. But there was nothing really wrong with the nuclear plant in Japan; it was just the environmental effect of a tsunami was so severe that they had to shut it all down.

04-00:59:52
Burnett: Right, right. Well, when you do your work assisting in the improvements of designs of separation processes, you don't really consider safety. You consider safety in your laboratory, but that's not really your purview.

04-01:00:13
Prausnitz: No, no. Others do that. Yes.

04-01:00:20
Burnett: So we're in the 1970s, and there is this pivot briefly towards energy efficiency and alternative fuels, and there's some research that was undertaken, and you notice with some regret that students got cut off at the knees when they were finishing their PhDs in this area—

04-01:00:44
Prausnitz: Oh, yeah.

04-01:00:45
Burnett: —of working in coal, removing impurities from coal—

04-01:00:48
Prausnitz: That's right, that's right.

04-01:00:49
Burnett: —or separation processes, and the Reagan administration cut that. And it doesn't really come back in force until the 2000s, when the price of—

04-01:01:00
Prausnitz: Well, coal is not studied at all anymore, here. In our department no one works on coal. Now, I don't know about other places, but—

04-01:01:09
Burnett: But alternative fuels and energy efficiency—

04-01:01:12
Prausnitz: Well, alternative fuels, biofuels. There's been all this interest in that. And you talked to Professor Blanch, so he probably discussed that with you.

04-01:01:24
Burnett: Yes, a little bit. The other big change that happens in the 1970s, you have the Cohen-Boyer patent, you have *Diamond v. Chakrabarty*, the very beginnings of the biotech revolution. And it happens here in the Bay Area, and I can't imagine that the impact was small on chemical engineering. So I'm wondering if it's a good time to begin to talk about the pivot towards the life sciences, in particular molecular biology, on campus and in California, and how chemical engineering fit into that dramatic transformation.

04-01:02:33
Prausnitz: Well, it started really with Professor Wilke, who had really tremendous foresight. I greatly admire what he was able to do, that he really saw this coming long before anybody else did. And he started work in the bio area, and he got Harvey Blanch to come here. Harvey Blanch was at the University of Delaware, doing very well there, but we were able to attract him to come here. And Blanch started a program with Charlie Wilke on newspapers. Old newspapers are a source of fuel, and so they worked on the conversion of newspapers into something useful, like ethanol. And they were way ahead of other places. Other places picked that up. The difficulty is economics. That process that Wilke and Blanch came up with, as well as other processes,

including some that I developed, they work, they all work, but they're too expensive, and it depends a lot on the price of oil. If oil becomes sufficiently costly, why, then these alternative biofuels have a chance. But unless oil gets a lot more expensive, there's not going to be much promise in biofuels.

04-01:04:11
Prausnitz: Now, let me just point out there are essentially two sources of biomaterials that you can use. One is corn, and if you want to use corn to make ethanol—and ethanol is a fuel—that is okay. That, of course, has been done, and is still being done, because the farms in the Midwest just love it. This is a new market for corn, and so with the help of the US government, all sorts of processes were developed for making ethanol out of corn, and they still work today, primarily because they were government subsidized. Also, corn is a very fortunate source. It's relatively easy to make a useful product from corn. The other sources that people use, the ones that I certainly worked with, are grasses, or waste products that you get from corn, the corn cob, not the kernel. The kernels are what they use today; they don't use the cobs. And also there's a lot of greenery coming with corn. It's called stover. If you start with those materials, that's much more difficult to make something useful out of them. And that is, of course, what we would like to do. I would like to use waste material that isn't used for anything else and make something useful out of it. With corn, obviously, what you do and what has been done is it affects the corn market. Corn is grown primarily for feeding animals, and sweetcorn for feeding humans, and the corn market has been greatly affected by the fact that corn is used for making ethanol. So you don't want to use corn; you'd like to use something that nobody else wants, something inedible. [laughter] And that is much more difficult. But, again, there are processes for doing it, and we've worked out some here, but economically they just won't succeed.

04-01:06:41
Burnett: Do you think there's sometimes too much technological optimism around the capacity of engineers to work wonders? You'd mentioned the example of clean coal, that there's that four-to-one ratio. That's the ratio to beat, and you can't do it with coal, no matter how much you engineer it. Are there others where the contenders are close, where the race is closer? I remember with oil-sand separation it wasn't economic until oil hit—

04-01:07:14
Prausnitz: Shale.

04-01:07:15
Burnett: Yeah. And—

04-01:07:16
Prausnitz: Tar sands and shale, yeah.

04-01:07:18
Burnett: Tar sands and shale. And it wasn't economic until oil would hit fifty, sixty dollars a barrel, but that doesn't seem like—

04-01:07:27
Prausnitz: Oh, I think it was more than that. [laughter]

04-01:07:28
Burnett: Oh, yeah, yeah. But it's still—

04-01:07:31
Prausnitz: That's where it is now.

04-01:07:32
Burnett: But the processes got more efficient. I remember the threshold was quite high, and after scaling up, it worked out to be more economic. There's tremendous environmental costs, of course, which we won't account for now. But my question is, are these contenders really amenable to technological solutions if we give sufficient attention to them and sufficient investment?

04-01:08:09
Prausnitz: Well, that's what we hope. Now, you mentioned a moment ago about coal and natural gas. If you can find a way to do something with the CO_2, then coal really looks more attractive. To run a coal plant, and the CO_2 goes up the stack, first thing you have to do is you have to catch it. You have to somehow get that CO_2 and separate it from the other gases, mostly nitrogen, and that can be done. There are ways to do that. Again, they cost some money, but you can do it. But then what do you do with the CO_2 after you've separated it from the gas? Well, they're talking about injection into old oil reservoirs, but that has all sorts of dangers associated with it. So if somebody can come up with some way to take care of that CO_2, maybe send it to Mars or something—[laughter] that's of course not economic, but if we could somehow do something with the CO_2, then coal would be much more attractive again.

04-01:09:17
Burnett: And, of course, I guess the question of separating the oxygen from the carbon is a ridiculously energy-intensive [laughter]—

04-01:09:28
Prausnitz: Well, there are techniques, and there's some work being done here at Berkeley on this, and that is to use solar energy, to convert CO_2, to make methane and water. But somehow you have to get the hydrogen in there. I don't know the details of how they do it, but, as you point out, it requires energy, and so that's why they hook it with solar, hope to get the energy from the sun to bring about this reaction. It's pretty much in the clouds. [laughter]

04-01:10:09
Burnett: So there is a shift in your research interests. You make these signal achievements in developing the research that would assist the chemical industry, and the petrochemical industry, in the mid-1970s, the key papers that we've just talked about. But there is this larger shift toward the life sciences, federally—there's Nixon's War on Cancer in the early seventies; there's the scaling up of the NIH, the National Institutes of Health—but there's a lot more money going towards the life sciences. And once biotechnology becomes a

reality, and it is possible to patent living organisms, or the modifications to living organisms, and it's possible for universities to capture some of this stream, it begins to change the game, and it changes fairly quickly. So how do you manage that? At what point did you feel that your research in molecular thermodynamics towards the questions that you were looking at in the seventies, when did that run its course? Did you feel that it was a problem solved?

04-01:11:44
Prausnitz: Oh, no, not at all. I was not really much affected by this, except through Harvey Blanch. Harvey reported on his research, primarily through his students, and it struck me that it was all very empirical. In other words, he'd measure something and he'd plot it up, this versus that, he'd get a nice curve, and that was sort of the end of it. There was very little analysis of the results. The results were presented, and the results were probably useful for some particular purposes, but there was very little science there. There was no methodology. And this was particularly called to my attention by Professor Clay Radke, who mentioned you before. Radke was very unhappy with this biotechnology. There was no technology. There was a lot of bio, but not much technology. [laughter] And so I talked to Harvey about it and said, well, maybe we can use some thermodynamic ideas to put the results on a firmer basis, perhaps correlate them in some way. And Harvey was very open to that. He said, "Yes, that would be great if we could do that."

04-01:13:12

So Harvey and I started to work together, and we published a lot of papers in this general area of how one can use a more rational, more theoretical, more mathematical way of presenting the data. And that worked out very well for many years. We worked on this together for I would guess as many as twenty years. How much of an impact it had, I don't know. I can't say. The part that I found most interesting was the separation of proteins. You have a bunch of proteins in water, usually; you have some salt in there, possibly; and now you want to make a separation. So coming out of a bioreactor, you don't just get the product you want; you get also some other products, too, that you don't want. And so the separation problem comes up there, as it does in chemical plants. And so I worked with Harvey on that, and we did, I think, some reasonably good work.

04-01:14:24

But, again, did it have an effect in industry? I really don't know. I think maybe it did. I remember hearing from the people at Amgen, one of the large pharmaceutical houses, and they chatted about the work that we had done here. They were doing similar things in separations of proteins. That's the only one that I really know of, that ever contacted me. You never know; people might use it without telling me about it.

04-01:14:55
Burnett: Yeah. Well, I'm also thinking of the impact on chemical engineering here at the University of California, Berkeley.

04-01:15:01
Prausnitz: Oh yes, so that was quite profound, because we hired more people, and we have, I think—I don't know—something like four or five of our faculty are in the bio, or closely related, area. What really gave this whole thing a push was this institute that was founded on the campus. Do you remember? EBI, I think it was called, Energy Bio Institute or something like that. EBI. And a lot of money was given to us by one of the major oil companies, British Petroleum. We're talking about roughly $15 million a year. It was really big bucks. And a whole building was built, very nice laboratory here. It's on Oxford Street.

04-01:15:57
Burnett: Is that the Energy Biosciences—

04-01:15:58
Prausnitz: Energy Biosciences. That was the word, yeah.

04-01:16:00
Burnett: Institute, yeah.

04-01:16:02
Prausnitz: And so that had, of course, influence. If you have money you influence research, and they threw a lot of money into it. And so I worked in there for a number of years, and we had graduate students and visiting scholars from all over the world who came to work in this area. And it was fine. What we did, I think, was perfectly good, but, again, the economics was against us. So everything we came up with worked, yes, but it cost too much money.

04-01:16:42
Burnett: Well, persistence is required, I think. You have to keep trying these different avenues, and—

04-01:16:51
Prausnitz: Yeah, well, you have to have some good ideas, and I guess our ideas weren't good enough. [laughter] One of the tools that was popular—it still is, I guess, in some cases—was ionic liquids. An ionic liquid is a salt, but it's not solid; it's a salt that's liquid. In other words, an ionic liquid is a salt with a low melting point. If it melts, say, around room temperature or below, then you have a liquid of ions. And that became a very, very popular topic in chemical engineering about twenty years ago, and there are lots and lots of papers on ionic liquids. And ionic liquids have their nice property: they will dissolve some of these materials, these biomaterials, like a grass, or the core of a corncob, or shavings from a corncob. So ionic liquids are really nice. You could get this material into solution. I think it'll even dissolve wood, so that's good. And then you can work with it, do some chemical reactions. The problem, though, is that after you're all through, how do you recover the ionic liquid? Again, can be done, but it's not cheap, and you've got to recover it. If

you don't recover it, you've got a waste problem on your hands. And furthermore, ionic liquids are expensive, so you can't afford to throw it away. You've got to use it again. And that's the big problem with ionic liquids. We worked with ionic liquids for a while. I know Harvey Blanch was a great enthusiast of using ionic liquids in the bio area. But then one day the director of the EBI said no more ionic liquids; it's useless in the sense that it's too expensive; don't use ionic liquids anymore. Well, Harvey Blanch was very upset by this, but he then did his work at what's called JBEI—

04-01:19:16
Burnett: Right, the Joint BioEnergy Institute.

04-01:19:22
Prausnitz: And they did not prohibit working with ionic liquids. The EBI director said no more ionic liquids, but at JBEI they did use ionic liquids, maybe still do today. I'm not up to date.

04-01:19:34
Burnett: EBI was Cal Berkeley and two other institutions?

04-01:19:44
Prausnitz: Well, Illinois got some of the money.

04-01:19:47
Burnett: Okay, but oh, so EBI was just University of California Berkeley?

04-01:19:51
Prausnitz: Yes, right.

04-01:19:52
Burnett: And then the Joint BioEnergy Institute had Iowa State and UC San Diego—

04-01:19:59
Prausnitz: I don't know much about that, but that goes through LBNL. That's where it came from. And one of our faculty is the big boss there; Jay Keasling is his name. And I don't really know as much about what they do as I would like, but I do know that getting support for the work that they were doing was very difficult. We couldn't get any money from JBEI. We didn't get any money from EBI. Even though they had $50 million, we never got much.

04-01:20:33
Burnett: Interesting. Why do you think that is?

04-01:20:38
Prausnitz: Well, what we were doing was mostly classical chemical engineering, and the director there, he was not an engineer; he was a scientist. And he was more interested in funding fundamental work, to see if there was some brand new way of doing things. Classical ways were not particularly popular. Now, the assistant director was an engineer—I think he came from British Petroleum—and he was very much application-oriented. So if you had something that

looked good financially, he was for it, but he didn't think—and he was right—that what we were doing was financially useful. So the director wanted fundamental, new, really new work. The assistant director, the engineer, he wanted something that you could use right away, would make money. We were in between.

04-01:21:41
Burnett: You had this desire to make applications, or to apply principles to useful products, but, ironically, more fundamental research was required. You needed a longer timeline to be able to make something happen.

04-01:22:02
Prausnitz: Yeah, that was the constant problem we have in chemical engineering. The people in industry think we're much too fundamental; we're too remote from economically useful application. And the other group, the physical chemists, they said, well, we're much too applied; we're not doing really fundamental work. We always fall in the middle. This has been true in my career, and the career of many of my colleagues, for years.

04-01:22:30
Burnett: But you had a good run of developing useful tools for industry.

04-01:22:42
Prausnitz: Yes.

04-01:22:42
Burnett: And so you had that credibility. Obviously it didn't necessarily work in this case, but—

04-01:22:48
Prausnitz: No, it didn't.

04-01:22:48
Burnett: —did you have a line of credit, or a line of credibility, when it came to research funding? Or did that not pan out?

04-01:23:00
Prausnitz: Apparently it did not.

04-01:23:01
Burnett: [laughter] Apparently not, yeah.

04-01:23:02
Prausnitz: No, apparently not.

04-01:23:03
Burnett: Now, you mentioned LBL. You were a PI—

04-01:23:07
Prausnitz: Yes.

04-01:23:07
Burnett: —for biochemistry there. I can't remember what the exact title was.

04-01:23:13
Prausnitz: No, no, it was in their Physical Chemistry Division. And yes, I did get some money from them, never very much. The last project that I did there had to do with krypton. Krypton is useful in headlights in cars. You get these very bright lights in a car, which is so annoying if the car faces you, and these lights use krypton. Now, krypton is a rare, very rare gas, and getting it out of the air can be done, but, again, this is expensive. And so we worked on an alternate process, using ionic liquids as an absorbing medium for krypton. And, again, what we had was okay, it worked, but it was too expensive.

04-01:24:19
Burnett: I think Jud King talked about drawing some funds out of LBL, that Chemical Engineering was able to marshal some of the resources of LBL. Can you talk a little bit about that? It was true in your case. I don't know what the role of PI there means. It means you have a chair there? You have some kind of—?

04-01:24:53
Prausnitz: No, it just means that you have a research program that you head. You're the head of it. So that's what PI means: principal investigator. And they give you a budget, and then, if the budget is big enough, you can support students or postdocs or whatever. I never had any space up there. I never got a laboratory space. That's just as well. I don't like the fact that our students are up there, because the educational experience is very narrow.

04-01:25:33
Prausnitz: I want students to be on campus, but, again, the economics is against it.

04-01:25:41
Burnett: Well, I was going to ask you about how chemical engineering, notwithstanding the lack of success in the project in ionic liquids, there is biochemical engineering. There is something called biochemical engineering, and it happens here. Is Harvey Blanch the kind-of toehold, and he's able to build resources and a program around it, and attract students? How does the rest of the department react or adjust to the kind of incursion of life-science questions?

04-01:26:37
Prausnitz: It's a problem.

04-01:26:38
Burnett: Yeah?

04-01:26:38
Prausnitz: It's definitely a problem, because it means that the department is, in a way, split. [coughs]

[break in audio]

04-01:26:54
Burnett: So we were talking about the effect of the incursion of biotechnology interests, and the enthusiasm for that within the department, and the dangers of that for the identity of the department, and I was wondering if you could elaborate on that.

04-01:27:12
Prausnitz: Well, yes, as I indicated, it sort of splits the department into two factions. We all get along all right, there's no enmity, but the intellectual interests are different. And we also have some problem with the graduate students. The graduate students who go into bio resent having to take courses in more classical chemical engineering, including applied mathematics. They don't want to be bothered with this; they just want to concentrate on their biology. And we think that that's not good, that they are, first of all, chemical engineers, and they should know the things that chemical engineering PhDs should know, so we make them take these courses that they really resent, in a way. So that's not nice. We don't like that, of course. We want students to take courses that they enjoy, and that they feel are important to them. We see it, really, at the seminars. Every week we have a departmental seminar. We invite a speaker from outside, or we have graduate students reporting on their theses. And the seminars are well attended by the bio people when the speaker is a bio person, but then the others don't come, especially the catalysis people. We have a large program in catalysis. Three of our faculty are leading catalysis people, and have big programs. And then they don't come when it's bio. But then when the lecture is about catalysis, then the bio people don't come. And when I say "people," I mean not only the students but the faculty. The bio faculty doesn't show up, and vice versa. [coughs]

04-01:29:15
Burnett: Ah, that's too bad.

04-01:29:18
Prausnitz: Yeah, that's really bad. Yes, it's very bad. And this is true at other departments. We're not the only one. I was mentioning Rice University. It's a very good place, in Houston, Texas, and they finally split into two departments.

04-01:29:38
Burnett: Yeah, that can happen. And I suppose you could see the biochemical engineers going to biochemistry, and going into the Valley Life Sciences Building if it's part of Integrative Biology, and that the chemical engineers could be—so the fate of the department—how big is the department now? How many faculty, roughly?

04-01:30:03
Prausnitz: I think it's nineteen, eighteen or nineteen.

04-01:30:05
Burnett: Not a huge department to start with, right?

04-01:30:08
Prausnitz: No.

04-01:30:08
Burnett: So department identity. You can see the cost increasing: if you've got a smaller department, the cost of splitting interests becomes quite high. And I understand that the name has also changed.

04-01:30:24
Prausnitz: Yes.

04-01:30:25
Burnett: I don't know exactly when that happens, but it becomes—

04-01:30:28
Prausnitz: Oh, it must be ten years now, something on that order. Yes, the title now is Chemical and Biomolecular Engineering.

04-01:30:42
Burnett: And so it has this dual identity, or it caters to two communities, but there's not that crossing.

04-01:30:50
Prausnitz: Absolutely.

04-01:30:50
Burnett: Your story, which we'll talk about next session, is that you adapt, and you become interested, and I'll ask you about this another time, but you cross over, and you are fluent in both languages, to the sense of—

04-01:31:15
Prausnitz: "Fluent" is a little exaggerated.

04-01:31:17
Burnett: You acquired some fluency, I'd say.

04-01:31:19
Prausnitz: I got some. Not enough, but yes, [laughter] I did learn a little about the bio world.

04-01:31:25
Burnett: Let me put it this way: if there is a talk in the seminar, and it's a bio person, do you go?

04-01:31:34
Prausnitz: Oh, I go. I religiously go to all of them. But if you ask me did I understand what the bio person was doing, well, it varies, [laughter] and depends on the bio person. If the bio person makes an effort to communicate to non-specialists then of course I can understand it, but sometimes they don't do that. Sometimes these bio people are very narrow, and they use a lot of jargon and a lot of abbreviations, and then I have trouble.

04-01:32:06
Burnett: And what's your reaction in that case? Do you ask questions, as a kind of quasi-outsider?

04-01:32:13
Prausnitz: Yes, I try to, but I don't want to be too embarrassed. [laughter] But yes, I do try to ask some questions.

04-01:32:24
Burnett: Okay. Well, we'll talk more about that, kind of your adaptation and your reaching out and your enthusiasm for biochemical engineering, in addition to your other researches, next time. Before we finish today, I did want to talk about visiting professorships. You've had several, and some of them we'll talk about in the eighties period next session. But there was one in 1973, in the University of Karlsruhe. Can you talk about that experience?

04-01:32:57
Prausnitz: Yes, it was a very good experience. I took my wife and two children. We were very nicely housed on the campus. There was a guest house, and we had an apartment in the guest house, which was very convenient. And I was stationed in the Physical Chemistry Department. They don't really have chemical engineering the way we know it here. There was a professor in the Chemistry Department who was a professor of applied chemistry, so there was some appreciation for chemical engineering, but not really in the way we have it here. It was not really a separate department.

04-01:33:42
Burnett: Interesting. You did say in an earlier session that chemical engineering wasn't a discipline in many other countries.

04-01:33:53
Prausnitz: That's right.

04-01:33:53
Burnett: But Germany was an exception, is that right?

04-01:33:55
Prausnitz: No. The German tradition was you had chemistry, including applied chemistry, and you had something which is called *Verfahrenstechnik*. *Verfahrenstechnik* means "processing technology": fluid mechanics; heat transfer. But these were separate institutes. They don't have departments; they have institutes. And if you wanted to do chemical engineering, you took courses in both areas. But the idea of chemical engineering is still somewhat alien in Germany. Some universities have it, but many don't. And when I was there—this was a number of years ago—they didn't have chemical engineering in the sense that we have it here. They had mechanical engineering. We were taught processing, fluid flow, and heat transfer, particles, and then they had chemists. But one difference is that many of the chemists were favorably inclined toward applications, and at that time—it's

different now—that was not true at Berkeley. The Berkeley chemists looked down on us. We were not a brother; we were a brother-in-law.

04-01:35:31
Prausnitz: It's different now, but at that time that certainly was true.

04-01:35:35
Burnett: But you didn't do a lot of visiting. There are professors who do a ton of visiting professorships. It's quite astonishing how many years they spend at other universities. And for some it's quite fruitful. Is doesn't seem something you were inclined to do, unless you got an invitation, or is it something that you got something out of, being in Karlsruhe, for example?

04-01:36:03
Prausnitz: I don't remember now how I happened to get there. I think there was one professor—his name was Professor Franck—who was here, visiting here, and I got to know him. And I think he was then responsible for having me invited to his place. I'm not sure that's correct, but I think it's close. And he was a physical chemist, a very good physical chemist, and one who was interested in applications. He knew quite a bit about chemical engineering, and he worked with people in industry, so he was favorably inclined. And when I got there, I gave a series of seminars about thermodynamics in chemical engineering, and I think I influenced some of the younger people there—

04-01:36:58
Prausnitz: —who then went on to teach at other universities. So I think I contributed something. Now, did I get anything out of it? Well, yes, I think so. I learned quite a bit about spectroscopy, which was one of the main tools that was used in the institute where I was. And I learned something about refrigeration, which was a major point of interest. There was an Institute for Thermodynamics, and they worked on refrigeration, among other things. I don't think I got too much out of it, technically, but culturally it was a very interesting place. The Supreme Court of Germany is in Karlsruhe. The building was right next to the campus. So I learned something about the German legal methods, the German government, and they had some very nice art museums, and there were concerts. So culturally I got a lot out of it, but technically, a little, not very much.

04-01:38:14
Burnett: [laughter] Wonderful. Well, let's pause for now, and we'll return—

04-01:38:18
Prausnitz: Okay.

04-01:38:18
Burnett: —next time. Okay.

Interview 5: January 28, 2019

05-00:00:20
Burnett: This is Paul Burnett interviewing Dr. John Prausnitz for the University History Series, and the project on chemical engineering here at UC Berkeley, and this is January 28, 2019, and this is our fifth session. Last time we were talking about years at different institutions, sabbaticals or years for research work, so we talked about one in the seventies, and there's another one in 1981 at the Technical University of Berlin. Can you talk about that year abroad, and what kind of work you were doing there?

05-00:01:10
Prausnitz: Yeah, it wasn't a whole year. It was somewhat less than a year. It was essentially the spring and the summer. And my family was with me, and we had a very good time. We enjoyed Berlin very much, and the children went to German school, which was exciting for them, and we saw a lot of the culture life in the city. And also, we took various excursions into the surrounding area, although that was a bit of a problem because we were in West Berlin, and all around us was the East German Police, but we managed to do it, and we didn't really have any difficulty. One thing you had to be very careful about, in East Germany there was a speed limit, ninety kilometers per hour, which is quite, quite low by German standards, and so you had to be careful not to exceed that, because the East Germans saw that as an opportunity to get Western money. If you exceeded the speed limit, they would immediately catch you and fine you right there, and, of course, you had to pay in the Western currency, not the Eastern currency. But we managed to avoid that. There was no real problem.

05-00:02:35

Now, at the university, in my particular area, I found out very quickly that the United States was way ahead. I don't think that's true anymore, but at that time the German work on chemical engineering, thermodynamics was very old-fashioned. It was highly experimental. There was no attempt, really, to interpret the data along molecular lines. They just weren't thinking that way. And so I think I contributed quite a bit. I was at this institute, Institute for Thermodynamics or something; I don't remember what it was called. And I had quite a lot of influence on the graduate students there, and also on the professor in charge of the institute. He and his wife were lovely people, and Susie and I got to be very good friends with both of them. Professor Knapp is his name. He died about ten years ago, but his widow is still alive and we're in touch with her quite frequently.

05-00:03:51

So, all in all, I enjoyed being there, but I was primarily a giver, rather than a receiver. Nevertheless, I learned something about the history of German universities, about how they run things. The thing that really bothered me most—and, again, this is just at that time; today it's different—is the isolation of the different institutes. If you were a student in a particular institute, that's where you were, and you didn't budge outside. Your confinement was very narrow. There were a variety of consequences, and that is that if a student had some sort of a question, he would go to Professor Knapp to see if the question could be answered. Well, Knapp was a very good man, he knew a lot, but of course he didn't know everything, and so, naturally, the American way of handling a question that a professor can't handle is to say, "Well, go to such-and-such a professor and ask him," but that was considered bad form at that time. The professor of an institute, he's supposed to know everything, and so the idea that one of his students would have to go elsewhere to get information reflects badly on him, that he didn't know that particular question, the answer to that question. This is, of course, terrible, and I think today there's much less of that, but this was really very striking, this provinciality. And it's not that there was any enmity between the institutes, but they guarded their borders very carefully, and so the students had to conform in that isolation.

05-00:05:47
Burnett: And that's something that you've written about before, in general, with respect to some of the problems that you saw in science. So was this your first encounter with that, or was it something that was striking to you particularly in Berlin that made you think this is something wrong with how science is done generally?

05-00:06:07
Prausnitz: Well, again, at that time; today it's much better. But no, I had no reason to believe that Berlin was any worse than any other place. It was just a feature of the old German system, and it certainly held back progress. I'm a firm believer that progress comes from the outside. The usual image is a balloon. The balloon rises and the balloon expands because it has contact with the outside, not because of anything happening inside, but it's happening outside. And I firmly believe that's true, and at that time, in the German university, there was very little of that. At the same time, I was impressed by the fact that there was lots of money. Compared to the way we lived at Berkeley, there was lots of money there. The state provided assistantships, so each professor had a certain number of graduate student assistants, typically five, and they were paid by the state, from the state budget, so the professor didn't have to go out and raise money. And also, he had at least one secretary, possibly two, and he had a metal shop and a wood shop. It was all paid for by the university budget. So with regard to materials, they lived very well, very well indeed.

05-00:07:43
Burnett: Would you prefer that system, in terms of the funding of science, in terms of it not being spending a lot of time writing grant proposals? You can just get the work done?

05-00:07:50
Prausnitz: Well, of course. Of course I prefer it. It makes life a lot easier, and allows you to spend time on research rather than on begging. On the other hand—there's always an "on the other hand"—on the other hand, if you have to apply for

grants, the advantage of that is it makes you think. You've got to really think hard and figure out what is it I want to do, and how can I do it, and so forth. You have to really prepare your case. If the money comes automatically, you don't have to do that. So, as always in life, there are advantages and disadvantages. One of my colleagues in chemistry was on a fact-finding committee for a university in Switzerland, in the chemistry department, and the report that he wrote to the administration is that the professors were getting too much money, and that the professors didn't have to think about what they were going to do; the money was coming anyway, and so they were not being challenged. Now, needless to say, this colleague of mine in chemistry was not very popular with his Swiss colleagues, but he had a good point: if the money comes anyway, you don't have to really think very much about what you're doing. So there must be some happy intermediate position. In Switzerland, again, at that time, the professor was sort of king. He could do anything he wanted, and he was never questioned on any financial matters, and he was a real big deal.

05-00:09:37
Burnett: Was this at ETH?

05-00:09:38
Prausnitz: This is ETH in Zurich. That's where I spent my first sabbatical. So it was even more extreme in Switzerland than it was in Germany at that time, but it certainly is true that the German professors live very well. I mean, when they wanted a piece of equipment, they would just write something, and it would come. So, from a budget point of view, it was much easier for them. I think, again, this has changed, no longer the case.

05-00:10:09
Burnett: Well, I mean, would you make a case that there is an American way of doing science at this time that then gets exported to other places, or—?

05-00:10:21
Prausnitz: Oh, yes, absolutely. The old European continental system—and England, too—is that a given department would have one professor, one professor only, and he was there to— It was called the "god system," and—

05-00:10:39
Burnett: The god system?

05-00:10:40
Prausnitz: God, G-O-D. He was the god, see, [laughter] and all the other people were dependent on him. And that is pretty much gone now. The departments have several professors, and there's much more diversity and independence for the various what I might call sub-professors. They can have their own projects, whereas in the olden days they had to work on the project that the big boss told them to work on. So yes, there has been a big influence from America in the system. But another thing is that they don't have the tenure system that we have, where people have to prove themselves as assistant professors before

they get tenure, although, again, I understand that is changing in Europe, but certainly at that time when I was there, there was no such thing. If you were appointed to a position, you had tenure. Unless you did something terrible, you would keep that job as long as you wanted. Another thing I noticed is that once you were a professor, you were never checked. In other words, there's no review. In American university, especially a place like Berkeley, you're reviewed every few years to see, well, now, what have you done recently, so on. That question is never asked in Germany. If you're a professor, that's it, and there's no such thing as a salary raise. Here, of course, as you move up the ladder you get a higher salary. They don't really have that system. The only way you can really improve your salary is if you get a so-called *ruf*, which is a call. If some other university calls you, then you can start negotiating. But other than that, your salary is fixed, although, of course, you're a public official, and so there are inflation raises. As the cost of living goes up, your salary goes up, too. But that's for everybody.

05-00:12:57
Burnett: Right. You're a state public servant, effectively.

05-00:12:59
Prausnitz: Yes that's a public servant; it's not on an individual basis. So a professor is never really checked like he is here. Here, he's under constant surveillance. [laughter]

05-00:13:10
Burnett: And I understand there were some— By the late sixties, I think, salaries at UC Berkeley for faculty were pretty low, compared to other comparable, say, private universities.

05-00:13:28
Prausnitz: Still true, I'm afraid.

05-00:13:29
Burnett: Yes, but the gap has closed.

05-00:13:31
Prausnitz: The gap is less, yes.

05-00:13:33
Burnett: Was that in the early seventies that that happened? Did you remember when things—? Is it [George Deukmejian, Governor of California, 1982-90] who approved salary raises?

05-00:13:45
Prausnitz: I'm sorry, I don't remember that. But yes, it's true that with time salaries improved at Berkeley. When I first came, my salary was $5,100 per year, and I thought that was a lot at the time, and, of course, that's no longer the case. A starting assistant professor now has at least ten times that much. But I don't remember any particular moment in history when that changed dramatically.

05-00:14:12
Burnett: Okay. So there's an American system of merit and surveillance and evaluation, and also some multiple mechanisms of external justification, not just your department, the campus, or the budget office at Berkeley, but also to any external assistance in terms of National Science Foundation funding. So you're applying for grants; you're always seeking support—

05-00:14:43
Prausnitz: Well, they have that in Germany, too, of course, but you're not as dependent on it. You have a certain base that the state gives you. If you want more than five people—five is approximate—why, then you have to go out and get grants, but you have a certain base to begin with. And not only that, you have all sorts of other assistance—shop people, secretaries, and what not—and that we just don't have in this country, and that is unfortunate. A professor spends an awful lot of time doing things that a secretary could really do much better.

05-00:15:18
Burnett: So administrative support would be much, much better.

05-00:15:22
Prausnitz: Right.

05-00:15:27
Burnett: So you are returning to Germany after—

05-00:15:36
Prausnitz: In a sense, yes.

05-00:15:36
Burnett: In a sense.

05-00:15:37
Prausnitz: In a sense.

05-00:15:37
Burnett: Were there any memories or anything? Was it just such a completely different place?

05-00:15:45
Prausnitz: Well, when I left Germany I was still quite young and I don't remember too much, but I did look for my old school, and that was gone, had obviously been torn down or lost in the war. The house where I lived—where I was born, in fact—still exists, and looks essentially unchanged, and, of course, we went to see it. So that was, essentially, the way it used to be. I didn't notice much change there at all. And there was sort of a playground in front, and a big Catholic church. All that was still there, so that was kind of reassuring, that there's some continuity. But, well, it depends. There's some parts of Berlin that were not damaged nearly as much as others, and that were restored, but, of course, there are other areas that were completely demolished, and so the houses that were put up are usually pretty low-quality, something quick,

quick, quick to help people get a place to live. So there's a big variety of things.

05-00:16:59
Burnett: Right. Did you go to the Wall?

05-00:17:01
Prausnitz: Oh, yes, of course I went to the Wall, and at that time it really was a wall. It was quite forbidding. And we went to East Berlin a few times, just to look around, and my grandparents are buried in a cemetery in East Berlin. So that was kind of interesting.

05-00:17:27
Burnett: Was it a different environment, I imagine?

05-00:17:30
Prausnitz: Oh, East Berlin at that time was completely different. Oh, yes.

05-00:17:32
Burnett: Yeah. And a lot of places were not even rebuilt. There was still rubble.

05-00:17:37
Prausnitz: There was still quite a bit of rubble, yes. And also, you would see many walls with holes in them, where obviously some bullets had gone through that had never been plugged. But there was a rich cultural life. There was the opera, which was very good, and there were theaters. So there was a lot going on there, but it was all drab. No neon lights to speak of. People walking around were sort of quiet, and the dogs didn't bark. It was—

05-00:18:16
Burnett: Wow. [laughter]

05-00:18:16
Prausnitz: It was— I'm exaggerating, of course, but it was sort of— It was not joyful. There was no joy. People were just drudging around in old clothing. It was sad. Now, of course, that's all gone. Of course, it's completely different now.

05-00:18:37
Burnett: Did you go back and visit after—

05-00:18:40
Prausnitz: Oh, yes, I've been back to Berlin several times. Oh, yes.

05-00:18:43
Burnett: And so you've watched the changes and the transformation.

05-00:18:45
Prausnitz: Oh, yes, the changes. It's quite dramatic. There are parts of Berlin now where you think you're in New York or Dallas or someplace, these big skyscrapers and neon signs, and very loud traffic. And so much of the charm of Berlin has been sacrificed, as it has everywhere in the world.

05-00:19:10
Burnett: Right, of course. So I wanted to highlight another time abroad for you, and this was in 1985.

05-00:19:21
Prausnitz: Well, that was quite different.

05-00:19:23
Burnett: Can you talk about that year?

05-00:19:25
Prausnitz: Yes, 1985 I was at a place called *Wissenschaftskolleg*. *Wissenschaft* means "knowledge" or "science," and *kolleg* means gathering, togetherness. And the *Wissenschaftskolleg* was sort of an institute for advanced study. It was a private organization, but they did get federal funds to help. And the way it works is they invite forty scholars from around the world, and they provide an office for everybody, and a library, and most important is a dining room. There's also a living room, newspapers, magazines, but most important is the dining room, because the only requirement of these forty scholars is that they show up for the midday meal. And, of course, in Germany the midday meal is the main meal. It was 1:30, and it was a very good meal. We got very good food: soup and meat and potatoes and wine, nice desserts. And so it was very, very nice. And, of course, the purpose of this requirement that you show up is to encourage you to communicate, talk to people. And I did. I had good luck. First of all, I speak German. Many of the scholars there spoke German, although some only a limited amount, so that there was a lot of English. A lot of the conversations were in English. And, of course, I could speak either way. It didn't matter to me which it was. And I had a talent for asking good questions, and so I would ask—there were psychologists there, and literature people, and historians, and sociologists, and so on. And so I have a talent for asking questions, which, essentially, "What are you doing?" and "Why are you doing it?" and so on. And, of course, people love to talk about themselves [laughter] and what they're doing, and so I had no problem making friends, sort-of one-way friends—they did most of the talking; I did the questioning—but that didn't bother me, because I learned something. And I learned a lot about different things, different areas of knowledge. And so I had no problem getting their confidence, and they thought I was a most unusual chemical engineer. It felt very unusual that I should know something about history or literature.

05-00:22:13
Burnett: Is there a prejudice? Is there—

05-00:22:15
Prausnitz: Oh, there's tremendous prejudice, and I managed to break through that. And so I found out what they think about us being engineers and scientists. And I guess the quick answer is not much. [laughs] Many of them are quite convinced that what they're doing is really important. What we do in science, and especially applied science, is important, in the sense that it provides

comfort of living. We have nice cars and nice clothes that are made by applied science people, and, of course, medicine. So they appreciate the comforts that applied science provides, but they don't think of applied sciences as being intellectually equivalent to where they are. By "they," I mean primarily literature people, or philosophers. Again, I'm generalizing, which one should not do, but they tend to feel that they are doing the really important intellectual work. They're concerned about the ultimate questions of life. What is the good life? What are correct ethics? What should be the correct form of government? How do we bring together love and justice? The big—

05-00:23:58
Burnett: The big questions.

05-00:23:59
Prausnitz: The big questions of life, they deal with them in various ways, whereas we—"we" meaning chemical engineers—we just provide comfort, but we don't provide any intellectual contribution. So, again, I shouldn't generalize, but most of them had thoughts along those lines. And the analogy that I use, and I think it's probably correct, is that these humanists tend to think of us applied scientists the way that we applied scientists think of car mechanics. We know that we need a car mechanic. If our car doesn't work, we've got to go to a car mechanic, and he'll fix it. And we're very grateful for that. We want our car, and we're very happy that there's a car mechanic who's able to do this, but we don't think of him as our intellectual equal. He performs an important service, the car mechanic, but he's not one of us, in a different category. So I use that analogy, and I think it's probably a correct one.

05-00:25:06
Burnett: I think so. It's this notion of applied science as technical work. It's procedural. Once you understand the fundamentals, and the fundamental research has proven X or Y, then it's simply a matter of translating that to an application.

05-00:25:25
Prausnitz: Yeah, but the word "simply" is totally incorrect.

05-00:25:27
Burnett: Of course. [laughs]

05-00:25:28
Prausnitz: It's not simple at all, and it takes quite a bit of intellectual imagination, and often very hard work, to make that transition.

05-00:25:37
Burnett: Well, I think your career speaks to the truth that neither is it simple, nor is it merely applied. So this notion that there's just this fundamental research over here, and an application done by technicians over there, you have done this fundamental work to understand processes, and that's part of the story, too. That division between pure and applied seems to have dissolved.

05-00:26:09
Prausnitz: Yes, it's quite a jump. You read some paper in the *Journal of Chemical Physics*, which is full of equations and full of all sorts of abstract stuff, and you ask yourself, "Well, now, what can I do with this?" And often the conclusion is "I can't do anything with it," [laughter] but if you then try to do something with it, it's not simple and it's not automatic, it's not, "Oh, one, two, three, here's the application." That's certainly not what happens. There's a lot of intellectual, really intellectual work in going from what the theorists give us to something useful. Big, big jump.

05-00:26:49
Burnett: We haven't really talked about false starts. You've said that you go to the journals, the *Journal of Physical Chemistry*, and you're scouting, I think, for ideas that might be productive, right?

05-00:27:11
Prausnitz: That's right, that's right.

05-00:27:12
Burnett: How much of that goes on, or is that a phase that you do when you're starting a new project, or you're like—

05-00:27:17
Prausnitz: Oh, you're doing it all the time.

05-00:27:18
Burnett: All the time. Okay.

05-00:27:20
Prausnitz: And that's where Berkeley is particularly good, because we have the very powerful chemistry department right next door. And so I would, as I still do today, I go to chemistry seminars. I find out what they're doing. And sometimes, by no means always, I see, oh, this is interesting. Maybe we could do something with this. So, much of my career is a consequence of what I heard in chemistry seminars. Not only seminars, but also the lunch table at the Faculty Club. I think maybe I mentioned it already once before: there's this nice, long table where chemists—not anymore, unfortunately—used to congregate, and I would go there just about every day and listen in on the conversations, and I would learn a lot, not only about science but about policy and government and university administration, and how to get around the latest rules. It was a wonderful experience, that table.

05-00:28:26
Burnett: And one of the stereotypes of the scientist, if they're doing empirical work, they're in the lab testing something, and it's this repetitive work, and monotonous. Or, on the theoretical side, there's someone alone in a garret, [laughter] scribbling down formulas and dropping them on the floor, and forgetting their bodies, and that kind of thing, living in this rarefied air. And what you describe is fundamentally social: that you are going to hear people

talk, and you are talking to them, and asking questions. So, science is a social practice—

05-00:29:14
Prausnitz: Absolutely.

05-00:29:15
Burnett: —and we don't think of it that way, often.

05-00:29:19
Prausnitz: Of course. Well, there is the occasional lone scientist, but they are relatively rare. How many Einsteins are there? Einstein was a lone scientist, although not as "lone" as you might think. He did talk to people. But that's, of course, very rare. So yes, I think most science is social in the sense you have to talk to people and find out what they're doing and what they're thinking about. Yes.

05-00:29:49
Burnett: And we started to talk about false starts. So there's a lot of combing and a lot of seminars that you need to attend before it sparks, and you're always—

05-00:29:59
Prausnitz: Oh, yeah.

05-00:30:00
Burnett: —and you're always thinking, what can I do with this?

05-00:30:03
Prausnitz: The efficiency is not very high, no.

05-00:30:05
Burnett: No. So in the eighties you had done this incredible body of work that had reached a kind of peak of maturation in the mid-1970s. You'd done all of this work, and it has this incredible utility for the petrochemical industry, for example, giving much greater efficiency to look at their processes from using these new tools to be able to predict these chemical relationships. In 1985, you returned to Berlin as part of this group. Did it serve a purpose for you in—

05-00:31:04
Prausnitz: Yes.

05-00:31:04
Burnett: —jumpstarting your understanding of your future?

05-00:31:06
Prausnitz: Yes, it gave me a chance to just sit down and think, what do I want to do now? Which is a rare opportunity, because, of course, here, you're constantly caught up in daily problems. So that is one of the great advantages of going to the *Wissenschaftskolleg*: they leave you alone. They don't bother you. In fact, they had a wonderful library service. There are many good libraries in Berlin. Berlin's a big city. And if you wanted a particular book or a journal, you just wrote it down on a piece of paper and you went to the local library there, and

they had many couriers. The couriers each had a car, and they would go to the appropriate library to get the book for you, and within twenty-four hours you got the book you wanted, no matter how obscure, no matter what it was. It was a really wonderful, wonderful service. You just didn't have to do any work at all. You just wrote it down and in twenty-four hours it appeared on your desk.

05-00:32:11

So I started to do a little reading in the bio area, in the biothermodynamics area, and I soon found there is a lot there, but little that I could use for chemical engineering purposes. And I thought, well, maybe here's an opportunity to do something in the biothermodynamics area. And, fortunately, we had a professor here—he's now retired—Harvey Blanch is his name—and Harvey and I became friends. And he was very welcoming. He was very happy to have my contributions. So we had quite a few students together. We must have had, oh, I don't know, twenty or so PhDs that we supervised together. So we did quite a bit of work in that area.

05-00:33:14

And earlier you mentioned Charlie Wilke. He was sort of a model to me. Charles Wilke was one of the early chairs of this department. He was the first chair, actually, and before that he was head of the division. Chemical engineering was a division within [the College of] Chemistry, not a separate department, and Charlie headed that division, and then when chemical engineering became a department he was the first chair, and he was chair for ten years. So he really built up the place, and, of course, I always think of him as being a brilliant man, because he hired me. [laughter] And I greatly admire Charlie Wilke. He had been very successful in the area of mass transport. He'd done very nice work in that area. And then, when he was in his late fifties, I guess, something like that, he decided he'd done enough in that area and he was going to do something else now. And I thought that was very courageous. I really admired him for that. So he's the one who started biochemical engineering here. When? Oh, I guess the seventies, late seventies, and we started hiring people. One of the first people we hired in that area was Harvey Blanch, and he and Charlie Wilke worked together for a number of years.

05-00:34:37

So he was a model, in a sense, and Harvey and I had some very good students. How useful our work is in industry is hard for me to say. I don't know. Some of it certainly is, and some of it certainly is not. [laughter]

05-00:34:58

Burnett: Well, we'll talk about that in some detail. I am interested in this process of starting over. You spoke of the professors in Berlin who were deeply siloed in their institutes, and they were afraid, I think, from what you said, of venturing beyond that, because they didn't want to be seen to not know something.

05-00:35:31
Prausnitz: I think that's right.

05-00:35:32
Burnett: What is your feeling when you were starting over? What was your approach?

05-00:35:43
Prausnitz: "Starting over" is too strong. I was still doing thermodynamics, so it was sort of a branch or side way. It was not a totally new highway, whereas Charlie Wilke—he really made a radical change. I didn't make a radical change. I said, well, how can I use the things that I've learned in the last twenty or thirty years? How can I use those to make some sort of contribution in the biochemical area? So I guess a motivation for us is to see your colleagues. You see what your colleagues are doing, and you think, well, gee, what am I doing? Maybe what I'm doing is really old-fashioned stuff, and I really should be doing something that's exciting, that is somehow morally equivalent to what my colleagues are doing. The presence of colleagues is tremendously influential.

05-00:36:46
Burnett: So you have this kind of transformative year in Berlin, and you return, and you recognize—had you started conversations with Harvey Blanch before that, or was it really going to Berlin was—

05-00:37:09
Prausnitz: No, I think it was afterwards. I couldn't do it alone. I needed somebody who knew some biology. I didn't know anything about biology, and Harvey, of course, did, so we were a good team. We matched up quite well.

05-00:37:24
Burnett: And did you just open textbooks? Did you attend lectures? How did you train up to learn that world?

05-00:37:34
Prausnitz: Well, mostly by reading and talking to people.

05-00:37:38
Burnett: Yes. Again, the social aspect of things.

05-00:37:41
Prausnitz: The social aspect is very important. But there was another experience that really pushed me. There was a seminar, chemical engineering seminar, where Harvey talked about his work, and it was all empirical. He measured this and he measured that, he measured something else. There was no intellectual framework that I could see. And I was sitting next to my good friend and colleague, Clay Radke, who'd been my PhD student years ago, and Radke said to me, "This is very strange. This is just making measurements. There's no framework here. There's no there there." And he said, "This really should be changed." And that made a big impression on me, what Clay said, and it was at that point that I approached Harvey and said, "Well, now, what about it?

Can we use the science of thermodynamics? Would that be a helpful tool here?" And Harvey, to his credit, said, "Oh, yes, it probably would." So that started a collaboration.

05-00:38:46
Burnett: That's wonderful. So, venturing into this area, what was first for you? When we talk about you did the same combing process, right, you're scouting for something that you could hang thermodynamics on, or that you could frame with your thermodynamics models or ways of approaching things, can you talk about the first thing that took root for you?

05-00:39:13
Prausnitz: What do you mean by "take root"? I don't—

05-00:39:15
Burnett: [laughs] Well, first thing that catches. Let's say you're going out there and you're exploring, and the first time you said, this is an area where thermodynamics is going to be useful in understanding the processes.

05-00:39:35
Prausnitz: Well, the main application always is separations. You have mixtures, and there's some things in a mixture that you want and there's some things that you don't want. So the question now of separation, you have a liquid mixture, and now in the liquid mixture you have proteins, or other biomolecules. And so that was one of the main things that I worked on with Harvey—techniques for making separations—and that worked out quite nicely. And the other thing we looked into, which was more academic, is the folding problem. You have a big, big, long molecule, and somehow it folds into a globular shape as a protein, and does its function in that shape. In other words, it's not denatured. Well, many, many people, many biologists have been concerned about folding of proteins. It's a huge literature. And we did some work in that area with computers. We just took a large molecule—that's by molecular simulation—and tried to find out under what conditions it would fold. So we made some contributions there, but that's strictly scientific, and how much of a contribution, well, it's hard for me to say. There's such a huge literature, but I think we got some results that were of interest.

05-00:41:08
Burnett: So you're trying to understand why chemically proteins fold the way that they fold—

05-00:41:17
Prausnitz: Yes, or unfold.

05-00:41:18
Burnett: And you were looking at the thermodynamics of that, and other people had just been looking at strict sort of more—

05-00:41:25
Prausnitz: Oh, no, some other people had been doing similar work, but we did it primarily by molecular simulation on the computer.

05-00:41:35
Burnett: So this is a rich area—

05-00:41:42
Prausnitz: Oh, very.

05-00:41:42
Burnett: —in a couple of senses: rich in that there's an incredibly dense world of research taking place, but it's also well-financed, at least with respect—

05-00:41:57
Prausnitz: Yes, if you're in the club.

05-00:41:58
Burnett: If you're in the club.

05-00:41:59
Prausnitz: You have to be in the club, which, of course, we were not. So when you come to the question of finance, chemical engineers are at a tremendous disadvantage, because on the one hand people say, "What you're doing is much too theoretical, truly none of your business. You should be doing applied work." And then the applied people say, "Well, what you're doing is not really applied. It's much too theoretical. We can't use any of your stuff." So the two ends both don't like us, because we don't do enough of what they want. And so we're caught in the middle. And getting support for the kind of work we do has always been very difficult.

05-00:42:45
Burnett: Can you talk about this work with Monika Prange and Herbert Hooper on water-soluble polymers?

05-00:42:53
Prausnitz: Yes. Well, the main work that I think you're referring to is the gels. If you take a polymer and cross-link it so that you get a three-dimensional network—polymers are those chains, but how you get the chains now to link, then you form what's known as a gel. And so we looked at the thermodynamics of gels. And, again, people had been looking at that before, but not from an engineering point of view. So we looked at it from an engineering point of view, and especially asked the question, what can we do with a gel? Can we use it as a separation device? Turns out you can, but it's not necessarily as useful as we had hoped. Monika Prange was a German visitor. She already had a PhD when she came, so she came here for a postdoctoral year. And Herbert Hooper was a very, very good graduate student. I was very happy with him. And so we worked together, and I think we had some nice results.

One of the interesting things about gels is that a gel can, under the right conditions, go through a phase transformation. In other words, it means that a gel may have a certain density, and then you heat it, or you cool it, or you shine some light on it, or you do something, and suddenly it will collapse. That's the phase transition. And, of course, when it collapses it ejects all the liquid that's in there, so that a gel can be used in drug delivery, and it is used in drug delivery. So we looked at that a little bit. We looked at phase transitions and gels, and we came up with some very nice, nice results.

05-00:44:57
Burnett: When I was reading this paper, it discusses the conventional approach, the way that studying gels had been done in the past, and there's a lot of empirical work—

05-00:45:13
Prausnitz: Oh, yes.

05-00:45:14
Burnett: —and there is a work of E. A. Guggenheim's, his quasi-chemical approximation.

05-00:45:20
Prausnitz: Yes.

05-00:45:21
Burnett: And that's from a monograph called *Mixtures* in 1952, so this is—

05-00:45:26
Prausnitz: That's right. Old stuff.

05-00:45:28
Burnett: It's fairly old stuff. And so the position of this paper is that's not good enough, and this quasi-chemical approximation fails to explain how observed order/disorder transitions lead to lower critical solution temperatures.

05-00:45:47
Prausnitz: Right.

05-00:45:49
Burnett: Can you—

05-00:45:49
Prausnitz: You're learning something here. [laughter]

05-00:45:51
Burnett: Well, I'm swimming in this, and it's fascinating to me. So this might actually take another two hours to explain, but can you talk us through, perhaps, just basically how this is a departure or a contribution that renders the old way of doing things a bit more accurate, or—

05-00:46:24
Prausnitz: Well, we made some improvements there, yes, and it's always been a bit of a puzzle why you get so-called upper and lower critical solution temperatures. Now, a critical solution temperature means that when you are at that temperature you get a phase split. And in polymer systems you can have one at low temperature, and you can have one at high temperature. So in between those two temperatures you have two phases, but if you're above the upper one, or below the lower one, you have only one phase. And we tried to work out the thermodynamics of the situation, and we were partially successful—not totally—and we found that the quasi-chemical approximation is not nearly good enough, and it goes in the right direction, but it's not strong enough. So we devised some models which improved on that, and how useful? Well, yes, I think it's useful in some cases.

05-00:47:32
Burnett: Well, I can imagine. It's mentioned in this paper that this is hydrophilic gels. We're talking about gels that absorb water, and then, presumably, can release water if there's a phase transition, and that's where you're talking about the importance of drug delivery. So drugs, perhaps biochemical separations, and you were talking about that earlier, and the mixed success on that latter case.

05-00:47:56
Prausnitz: Yes.

05-00:47:57
Burnett: And this research was apparently supported by the Department of Energy.

05-00:48:05
Prausnitz: Yes. I managed to get an appointment on the hill here at the—

05-00:48:12
Burnett: Right. We talked about that earlier.

05-00:48:13
Prausnitz: At the Lawrence Berkeley National Laboratory. Well, that was also a little interesting, how that happened. I was offered a named professorship at MIT, and that impressed people.

05-00:48:38
Burnett: I bet. [laughs]

05-00:48:39
Prausnitz: And so they said, "Well, we better do something for Prausnitz." So I was given an appointment at the laboratory. And so I managed to get some money. That's what it amounted to. I didn't actually have a physical space up there, but I had the title of principal investigator in the Chemical Sciences Division, and I managed to get some money for research. And, of course, that whole laboratory there is supported by the Department of Energy, so that's how I got to the Department.

05-00:49:19
Burnett: That's the connection. I see.

05-00:49:21
Prausnitz: It was through the laboratory.

05-00:49:23
Burnett: Okay. So others in chemical engineering have talked about the importance of getting money from these other areas that wouldn't necessarily be supportive of chemical engineering, per se, but you have to kind of capture some of this money. So is that an example of that, that getting this appointment permitted you to redirect some funds?

05-00:49:52
Prausnitz: That's right.

05-00:49:53
Burnett: Were there others doing that, as well? Were there others in chemical engineering—

05-00:49:55
Prausnitz: Oh, yes, we have several people there. By all means, we have several of our faculty who have appointments up there, and some have their laboratories up there, but I never had a laboratory there. I didn't need that. I had enough space right here, and I much prefer that. I think it's very unfortunate to have students up there. They're sort of marooned. It's sort of like sending people to the colonies in the old imperial days.

05-00:50:28
Burnett: Really? [laughter]

05-00:50:29
Prausnitz: Well, it's like a colony, you see, up there, and—

05-00:50:31
Burnett: Well, with respect to chemical engineering, yes. But we were speaking about this American system of science where boundaries are not so strict, and there are multiple institutional arrangements, and multidisciplinary sites; is LBNL like that, in a sense, that you can bring scientists together? Do they not talk with each other up there?

05-00:50:55
Prausnitz: Yeah, they probably do. I don't really know too much about that, but yes, I'm sure they do. On the other hand, we have here the campus, and the campus has all sorts of seminars and lectures and whatnot. And if you take students away from the campus, you, in a sense, deprive them of what the campus has to offer. Now, it's true up there they get other advantages. As always in life, there are pros and cons. But I would much prefer to have the students right here.

05-00:51:27
Burnett: Well, we've talked about the importance of physical space, and it makes a difference whether or not you can be down the hall from someone, or you can have a conversation about science on the way to the lunchroom, or the cafeteria, or something like that, that people talk about that over and over again, how important it is to be collocated.

05-00:51:55
Prausnitz: Well, there's a physical, cylindrical building that Professor Calvin built. I don't know if the name Calvin is familiar to you.

05-00:52:06
Burnett: Yes, Melvin Calvin.

05-00:52:07
Prausnitz: He was a Nobel Prize-winning chemist. He did a lot of photosynthesis. And he got money somewhere—I don't know where—to build the cylindrical building that we have. It's just a little bit south of here. And this was all done on purpose. It has only one central staircase in the middle. Then they have these spokes coming out of it. And Melvin thought that this would encourage mixing; people have to go up the same staircase. I don't know whether it worked or not—there's no way to really know—but he was very well aware of the need for mixing, and so he thought this particular architecture would help him to do that. Now, of course, it's used by computer scientists. There's no more chemistry being done there. It's very unfortunate, but that's the way the world works. The most extreme case of that, incidentally, is something I encountered at Rensselaer Polytechnic Institute. It's a very nice, high-quality science school in Troy, New York, near Albany, and it was built many, many years ago. It started out as, I guess, a religious group. Well, you go there and right in the middle of the campus is a beautiful church, public church building, with a steeple, nice windows. And when you look closer, and when you go in there, you see it's the computing center. So that tells you something: the church turned into a computing center. [laughter]

05-00:53:51
Burnett: So you're talking of the repurposing of space for these things. But I think there's quite a long history of design, of design of space, in order to facilitate research, and part of it is by accident. I think in Melvin Calvin's case, the Rad Lab, the old radiation—the Donner Laboratory was an open space by chance, or it was just a barn-like space, and that inspired him to then go and say, can we build a space like that, deliberately, to encourage people to talk to each other?

05-00:54:25
Prausnitz: Did Melvin not do one of these series, these oral histories?

05-00:54:31
Burnett: Yes. It's not as full as we would like, but yes, I can get it for you. And there is certainly— I think one of the biggest ones is about his assistant. She did an

interview, and she told a lot— Let me verify that for you, but I can hook you up with those stories. [laughs] So there's another piece which I really enjoyed, which is "Thermodynamics and Separation of Mixtures." It's a 1994 article that you wrote.

05-00:55:08
Prausnitz: Yes, this was a celebration of the Bunsen-Gesellschaft, the Bunsen Society, in Germany, which is a society for physical chemistry. And they had a hundredth anniversary in 1994, and they had invited me to give a talk at that occasion. And the talk is essentially what this manuscript is about. So it was on separations, which had always been a topic of major concern to physical chemists, at least in the olden days; not so much anymore today. In a way, this was an unfortunate experience for me, because there were three speakers invited to the celebration, and I was the third one. And the other two were both Nobel Prize winners, so I was greatly honored to be in that company, but the trouble was that one of them wouldn't stop talking. It started at about 9:30 or so, and I was the last speaker, and so by the time I got to the lectern, I gave my talk, people were itching to go to lunch. The master of ceremonies didn't have the guts to stop the previous speaker, who had gone way over time. So I started late, and people wanted to get out, [laughs] so I had a great difficulty in keeping the attention of the audience. They wanted to go to lunch.

05-00:56:55
Burnett: Did you abbreviate or adjust your talk, or—?

05-00:56:59
Prausnitz: Yes, I tried to, but, of course, it's not so easy to do on the spur of the moment.

05-00:57:03
Burnett: It's terrible when that happens, because it makes it look as though it's your fault, [laughs] because they're waiting, and they—

05-00:57:10
Prausnitz: Yeah, but the previous speaker went over by at least a half hour.

05-00:57:14
Burnett: Oh my goodness.

05-00:57:17
Prausnitz: Yeah, that was unfortunate.

05-00:57:19
Burnett: Well, one of the things that you talk about, you talk about the importance of chemical potential, and Gibbs we've talked about already. So you've talked about the importance of Gibbs, and the fact that Max Planck and [Francois-Marie] Raoult and [Pierre] Duhem and [Ludwig Edward] Boltzmann were all thinking and working on these things in the late 1870s. And then you shift to talk about the importance of separations, and this Sherwood plot.

05-00:57:52
Prausnitz: Oh, yes.

05-00:57:52
Burnett: Yeah. Can you tell me a little bit about the Sherwood plot, and—?

05-00:57:57
Prausnitz: That's been quoted over and over again. Well, I first found out about the Sherwood plot from [Thomas K.] Sherwood. Sherwood was one of the great chemical engineers of his time, and a man with tremendous personality and a sense of humor. It was just a delight to know him. And he was professor of chemical engineering at MIT. All the great chemical engineers of the early period were at MIT. And so he retired—at that time, of course, retirement was a requirement; you had to retire at a certain age—and he came here, happily for us. We managed to attract him here, and he was a visiting professor here for, oh, I don't know how long, maybe ten years, when he died of cancer. And he made this plot, which is very instructive, and the plot is the price of a particular chemical versus its concentration in nature, or in a chemical solution. And he showed that the price goes up dramatically as the concentration falls. And the point is that when something is present in very small concentration, you have to do a lot of work to get it out, and the smaller the concentration, the more you have to do. And so he showed this in this graph. It's been quoted many, many times. And so I used it there also to show how important separations are.

05-00:59:45
Burnett: And that importance is just going to increase as we get to lower and lower concentrations or proportions in nature. In the mining industry they talk about this all the time.

05-01:00:00
Prausnitz: Well, the most dramatic example people are familiar with is uranium isotope. Most uranium is 238 mass [U^{238}], and before fission work, before building nuclear pants, we need uranium 235. You need an isotope of uranium which is present in very small concentrations, and so you have to take the uranium that you mine and separate the isotope that you want from the isotope that you don't want. And this is a very elaborate process, and it can be done, but it's not very efficient. It costs a lot of money. It costs a lot of energy. And there are various ways to do it, and we hear about it all the time in connection with Iran. They have centrifuges for doing that. They need many, so you have to do it in stages. So that is, perhaps, a particularly well-known example, a very small concentration. You've got to get it out somehow.

05-01:01:10
Burnett: And they used to find copper in ore of 4 percent [concentration], and now it's 0.1, 0.01. [laughs] And they just mine massive quantities, and do massive ore crushing and separation processes. And you can see how chemical engineering is just going to be more and more important as separation becomes more of a challenge.

05-01:01:36
Prausnitz: Yes, you want to do that as efficiently as possible, because it's expensive.

05-01:01:41
Burnett: Absolutely. So, continuing to talk about collaborations and work that you were doing, we've talked about proteins a little bit, and I think there was this work on the globular nature, and working on understanding protein aggregation, and so on, but there was also this work with Jianzhong Wu. I don't know if I have his name pronounced correctly.

05-01:02:14
Prausnitz: I have given up trying to pronounce his name, and whenever I see him or talk to him he's just Wu. [laughter] I cannot pronounce his first name. He knows that. He's one of the best students I've ever had. He was very, very good. And he is now Professor of Chemical Engineering at the University of California Riverside. And he's primarily a theorist, but he does very, very nice work.

05-01:02:47
Burnett: And he started here in the early nineties? Is that right, or—?

05-01:02:49
Prausnitz: I don't remember. That must be about right. Well, it's, again, an interesting little detail: he had written to me, and he said he would like to come here. And so he sent all his credentials, and so on. And the powers that be at the time said, "Well, no, we don't really want to take him. He seems to be pretty good, but it's going to be expensive." Foreign students pay high tuition. And so they didn't want to take him. So then I decided, okay, if they won't take him as a student, I'll take him as a laboratory assistant with LBL funds. The funds came in very handy. So he became a laboratory assistant, or something like that, and he was paid from LBL, and then he was here for about a year as a laboratory assistant, and by that time he was able to convince everybody that he's really very, very competent, and should be a graduate student. And so then he was admitted as a graduate student. I had to do a lot of pushing. And he came as a graduate student, and he got his PhD thesis in a short time. Then he stayed as a postdoc for another year.

05-01:04:11
Burnett: Sounds like you have an eye for good students.

05-01:04:16
Prausnitz: Well, yes, I think so, although, of course, you take a chance. Everything's a risk, but with him I hit the jackpot. There are others I had that didn't work out nearly as well. But yes, by and large I have a pretty good nose. [laughter] I can smell talent.

05-01:04:39
Burnett: This work was on bovine serum albumin solutions.

05-01:04:46
Prausnitz: Yes, this was just one thing he did. He did many other things, too, theoretical stuff, but that was the one experimental project that he worked on.

05-01:04:55
Burnett: So this is osmotic pressures of—

05-01:05:00
Prausnitz: Yes.

05-01:05:00
Burnett: Okay. And so part of it is measuring this, using osmometers.

05-01:05:08
Prausnitz: Osmometers. An osmometer, as you might guess, measures osmotic pressure. Yes, that worked out very well.

05-01:05:17
Burnett: Yes. And there's another area that you worked in, as well, and this was with your former student, Clay Radke. And you were working on the activity coefficients of lithium salts.

05-01:05:37
Prausnitz: That's more recent. That's only in the last couple of years that we worked on that, and we're continuing that. That project continues. The idea there is to get some fundamental information on properties of lithium ions, because of batteries. And lithium-ion batteries are huge business, and, of course, we would like to improve them. And so we're hopeful that this information that we're getting would be useful, and I'm told that it will be. We have here a young man named Bryan McCloskey. He's an assistant professor, about to be promoted. He's been here about four or five years. And he's an electrochemistry person, and he has a huge program here in batteries, together with LBL. And so we're giving him this information, hoping that he can do something with it.

05-01:06:37
Burnett: And can you talk a little bit about electrochemical engineering in the department here?

05-01:06:43
Prausnitz: Oh, yes.

05-01:06:43
Burnett: I know it's not something that you worked in so much.

05-01:06:44
Prausnitz: No, no, I've done very little in that.

05-01:06:47
Burnett: But this is another field.

05-01:06:48
Prausnitz: No, this was an area that was started by Charles Tobias. Tobias came here in the early days of the department—he was one of the very early professors here—from Hungary, and in Hungary he had specialized in electrochemistry. And the idea that one could do electrochemical engineering is really something he thought of, and he developed. He was very successful, and he had many students who are now electrochemical engineers throughout the world, including professors of electrochemical engineering. And he was really very influential. I got to know him quite well—we became quite good friends—but I never really worked in that area.

05-01:07:39
Prausnitz: But I, of course, knew about it. That was unusual. There are very few chemical engineering departments who do work in that area. There are more now, thanks to him, thanks to Charles, but at the time he started it was almost unique—not quite, but almost unique.

05-01:07:57
Burnett: Well, you would think it would be a massive area because of not just batteries but integrated circuits, the chemical—

05-01:08:07
Prausnitz: Well, there were electrical people, electrical engineers and physical chemists, but not chemical engineers.

05-01:08:18
Burnett: So it's kind of opening a door into that world, and it becomes a somewhat multidisciplinary—

05-01:08:24
Prausnitz: Oh, yes, definitely, definitely.

05-01:08:26
Burnett: —aspect. In the same sense in which you looked at molecular thermodynamics, and that became a really important framing tool, did Tobias have a similar, or did some of his students have a similar entrée into—

05-01:08:43
Prausnitz: Yes, he and Charlie Wilke played a role in this. He paid attention to something that others had not paid attention to: namely, you have an electrode; the molecules have to get to that electrode, and leave it. So there's a mass transport problem, and most others had completely neglected that. They didn't pay any attention to that. And Charles Tobias showed that under certain circumstances that's crucial. You've got to understand how the molecules go there and leave. So it was his background in mass transport, aided by Charlie Wilke, that he brought to electrochemical engineering, and now it's standard. Now everybody knows about it, but he started it. This was brand new. Well, not brand new, but not focused on—

05-01:09:36
Burnett: Well, we're here in the eighties and the nineties. We jumped a little bit ahead to talk about lithium-ion battery work.

05-01:09:44
Prausnitz: Oh yeah, that's very recent.

05-01:09:47
Burnett: But one of the things that you're doing is acting as the editor of journals, and there are a couple of journals.

05-01:09:57
Prausnitz: Yes.

05-01:09:57
Burnett: I'm wondering if you could talk about that work.

05-01:09:58
Prausnitz: Yes. Well, I was on editorial committees, several journals, and I found that work to be quite important. The way we communicate science is through publications, and the publications have to be checked. You can't just publish anything. Someone's got to look at it, and comment on it, and make sure at least superficially it's correct. So that's a big job, and professors do this all over the world, and, of course, they do it for nothing. We don't get paid for that. This is just part of our professional duty, to aid in communication of science. So I did a lot of that. I still do some. I get articles for review. And editorial boards, well, the editorial board is primarily like the Supreme Court. If various reviewers don't quite agree on a paper—some say yes, it's very good; others say no, it's bad—it has to go to a higher court, and that's what the editorial board in many cases does. It is a super-review.

05-01:11:20
Burnett: Yeah, yeah. So you were editor of the *AIChE*, the journal of record for chemical engineering—

05-01:11:29
Prausnitz: No, no, I was never editor, no.

05-01:11:30
Burnett: You were not? You were just—

05-01:11:31
Prausnitz: No, I was on the editorial board.

05-01:11:33
Burnett: Oh, so that's '73 to '77?

05-01:11:34
Prausnitz: The editorial board, but I was never editor, no.

05-01:11:37
Burnett: Oh, okay.

05-01:11:38
Prausnitz: The only editorship I have had is there is an outfit called the *Annual Review of Chemical and Biomolecular Engineering*. And I started that. I'm the original editor. This was about ten years ago. And now, just fairly recently, when I turned ninety years old I became editor emeritus and let some younger people take over. So I edited that for ten years, but no longer now. I'm still invited to the meetings. Everybody's very nice to me, and we always have a very good dinner, so that's all very pleasant, but I'm not the editor anymore.

05-01:12:22
Burnett: Okay. And in addition to being the sort of higher court, you'd be writing introductions to each issue.

05-01:12:35
Prausnitz: In the *Annual Review*, I had an opportunity to sound off on whatever I wanted to publicize.

05-01:12:42
Burnett: Right. And you were on the editorial board of the *Journal of Physical Chemistry*—

05-01:12:47
Prausnitz: Oh, yes.

05-01:12:47
Burnett: —from '76 to '81.

05-01:12:49
Prausnitz: Yes, and there was another one, the *Journal of the Industrial Engineering Chemistry*, and then the main one is a journal called the *Journal of Fluid Phase Equilibria*. I'm still on that board.

05-01:13:03
Burnett: But that's really your—one of your—

05-01:13:05
Prausnitz: That's my area.

05-01:13:06
Burnett: That's your area. The *Journal of Physical Chemistry*, that is—

05-01:13:12
Prausnitz: Well, they have various divisions. And I think it's B that I'm associated with. It's A, B, C. I think I was with B. Yes, that's right. I had a rather interesting experience with one of them, not the *Physical Chemistry* but the other one, *Industrial Engineering Chemistry*, and that is the way that they list their publications in a particular issue of the journal is they have various topics. They have all the papers on one topic together, and then they have papers on

another topic, and so on. And I suggested that we shouldn't do that; we should just mix them all up. And that was a shocking idea. The reason I wanted to mix them all up is because people browse, and so they look in their area, they find an article, and, okay, then that's the end, there's another article. Well, the way it is now, when they do that the other article's in the same area, because all the areas are grouped together, but under my scheme the next article would not be in this area. And, of course, that's what I want. I want the browser to see something other than his area. Well, I had no luck with that. I suggested this, and some of the other people on the board didn't even understand what I was talking about. They didn't get it. So it never happened.

05-01:14:47
Burnett: You missed your calling in designing shopping malls and supermarkets, because that's exactly the principle. You don't want a quick route from one place to another. You want to force the shopper to go through all the departments, [laughs] or as many of the departments as possible.

05-01:15:05
Prausnitz: Yes, and not only that, but I find in these departments that a given group will be subdivided. I happen to like a particular cracker called Triscuits; perhaps you know what it is, Triscuit. There are various kinds.

05-01:15:18
Burnett: Yes.

05-01:15:19
Prausnitz: There are various kinds. So if you go to the Safeway here, the big one in Berkeley, they have different flavors, and you find some Triscuits in one place, but then you have to go to another place to find other forms of Triscuits. They're not all together. And, of course, that's deliberate.

05-01:15:37
Burnett: Yes, but this is kind of your—

05-01:15:47
Prausnitz: It's my *leitmotif*. [laughs]

05-01:15:48
Burnett: It is. It is. It really is a theme, isn't it? And did you organize some of your teaching in that way?

05-01:15:59
Prausnitz: Well, yes, I try to relate whenever I talk about something, try to relate it to other areas, show where it can be used. And, again, I'm afraid many other teachers don't do that.

05-01:16:16
Burnett: You are really nourished by looking at the broader context. You don't subscribe to the two-cultures frame.

05-01:16:28
Prausnitz: "Context" is my favorite word. I love the word "context," yes.

05-01:16:34
Burnett: So you are thinking about applications. You're thinking about— One of the celebratory articles that I found, two people wrote it, and I think it was in the '98—so a while ago, twenty years ago already. And it's quite a celebratory article of you, and it talks about the contributions, or what goes into chemical engineering, and there's the empirical work, and there's the experimental work, the work in statistical mechanics, the theoretical work, and the modeling work. And you have had an impact in each of these domains. And—

05-01:17:32
Prausnitz: I don't know what article you're referring to.

05-01:17:35
Burnett: Now I have to go dig it up. [laughs]

05-01:17:38
Prausnitz: It's not one that I wrote. Somebody else—

05-01:17:39
Burnett: No, it's not one that you wrote, and you didn't point it out to me. It's "A Short History of Molecular Thermodynamics and a Tribute to John M. Prausnitz."

05-01:17:52
Prausnitz: And who are the authors?

05-01:17:53
Burnett: And it's in the *Industrial Engineering Chemical*—

05-01:18:00
Prausnitz: *Chemistry*, yeah, *Research*.

05-01:18:01
Burnett: —yeah, for the ACS [American Chemical Society].

05-01:18:02
Prausnitz: Oh, I see. I see, yes. I see. That's where it appeared.

05-01:18:05
Burnett: Helmut Knapp.

05-01:18:07
Prausnitz: Helmut. He's a German—

05-01:18:08
Burnett: Right. And Stanley Sandler from University of Delaware.

05-01:18:12
Prausnitz: And this is in '98?

05-01:18:13
Burnett: Yeah. So you didn't know about this?

05-01:18:15
Prausnitz: Oh, I'm sure I knew about it, but it's from twenty years so I forgot about it, yes. Well, that's nice.

05-01:18:23
Burnett: Well, it was apropos of chemical-process simulation, and it's complaining about the questionable, or even incorrect, thermodynamic models for the process under consideration. There would be garbage in, garbage out, and in this case the play on words is garbage in, gospel out. [laughter] So—

05-01:18:49
Prausnitz: Better than the opposite.

05-01:18:51
Burnett: And it has drawings in it, and it has these different types of—

05-01:18:56
Prausnitz: With stick figures?

05-01:18:57
Burnett: Yes.

05-01:18:57
Prausnitz: Yeah, that was Helmut's specialty: making stick figures.

05-01:19:00
Burnett: [laughter] Different types of scientists. So there's *homo collector, homo experimentor, homo corollator, homo inspirator,* and *homo simulator*.

05-01:19:13
Prausnitz: Yeah, that's right. That's Helmut. He loved to do little things like that.

05-01:19:18
Burnett: So it has all of these types of research in chemical engineering, all have their contribution. And so what about all of these methods? Which one should you use? And then the last one is: "You could dispense with that process and just ask the advice of John Michael Prausnitz, [laughter] who's thoroughly practiced in all of the above methods."

05-01:19:44
Prausnitz: Oh, wow.

05-01:19:46
Burnett: So it's quite a celebratory—

05-01:19:48
Prausnitz: Yes, very nice.

05-01:19:48
Burnett: Yes, it's quite a complimentary—

05-01:19:50
Prausnitz: I had forgotten about it. Yes, that's lovely.

05-01:19:53
Burnett: Yes, it is a lovely tribute. But, we're kind of talking about the truth of that, that you have explored—and you've said your contributions in simulation, computer simulation, you've depended on others who know the —

05-01:20:15
Prausnitz: Yes, I can't do it myself, but I've always had others who do it with me.

05-01:20:20
Burnett: Right. But I think this article's pointing to the fact that you can learn to run a simulation, but if you don't understand why you're running the simulation, or how the simulation is connected to the object of its description, the object of what you're describing, then it's not useful.

05-01:20:43
Prausnitz: Right. Many people simulate just for the sake of simulation, not to answer some question.

05-01:20:49
Burnett: Right. So you're proceeding from the intellectual challenge that you were talking about, that you were saying there is intellectual content to chemical engineering.

05-01:21:00
Prausnitz: Yes. A lot of people don't appreciate that.

05-01:21:02
Burnett: So if we wanted to turn to, say, the editorial work, in evaluating things, were there controversies ever about a paper where it was a kind of epistemological schism among the board, where you felt that we're having a debate about what we can know about a particular—?

05-01:21:31
Prausnitz: No, I don't remember anything like that, no. No, it was usually interpretation of data, and people tend to overinterpret. They tend to think that their data are telling them all sorts of things, which it really isn't telling them. Much of what they call interpretation is imagination and hope. [laughter] So that would usually be the reason, in other words, they were saying, "These data show such-and-such," and then more rational analysis would say, "Well, these data suggest that perhaps, but it's not really proven."

05-01:22:14
Burnett: So there is a—

05-01:22:15
Prausnitz: Modesty.

05-01:22:16
Burnett: There's an institutional brake on the claims, because it makes sense that a scientist wants to claim as much as you can for their results.

05-01:22:26
Prausnitz: Yes, that would usually be the problem: modesty. [laughter] People are not modest enough.

05-01:22:34
Burnett: Does science make progress in part through an ethic of kind of epistemological conservatism, or modesty, or is that a value that's extremely important to science?

05-01:22:53
Prausnitz: Well, it is very important that people leap to conclusions, but they should be careful about it, and not make claims that are not justified. But to say perhaps this shows such-and-such, that's useful, or could be useful. But to say this absolutely shows such-and-such, that's generally not useful. [laughter]

05-01:23:16
Burnett: And if it's suggestive, then it leads to— And that is a part of your rhetorical style in your papers, right? This is—

05-01:23:27
Prausnitz: Suggestive, yeah.

05-01:23:27
Burnett: In the conclusions, this is suggestive of further research.

05-01:23:30
Prausnitz: That's right. It leads to the next step.

05-01:23:33
Burnett: And so things like the journals, the seminars, these are institutions whose function serves to act as an adjudicating force that evaluates and reduces the claims to their sort of proper place—

05-01:23:59
Prausnitz: Yes, yes.

05-01:23:59
Burnett: —in the larger context of the literature. Because that's one of the things that happens, too, when people submit to journals: they don't know the literature the way that you do.

05-01:24:10
Prausnitz: Well, not anymore. It's become horrendous. The literature is so large that I cannot claim to know the literature anymore. There was a time when I thought, yes, I really did know it, but now it's just huge.

05-01:24:24
Burnett: Is that having an impact on the nature of journal publication in general?

05-01:24:27
Prausnitz: Oh, yes, it does, because people make so-called discoveries without knowing a discovery was already made long ago by somebody in Bulgaria. There are many examples of that, where things are rediscovered that had already been known long ago.

05-01:24:49
Burnett: There are some complaints, even on campus—Randy Schekman has been very vocal about this—the changing values surrounding journal publications in science, that *Science* and *Nature*, in particular, are guilty of printing the splashy conclusions that are going to generate lots of controversy but have questionable science behind them—

05-01:25:16
Prausnitz: Well, there's a corollary to that, and that is that in appointing new faculty the committee wants to know not only what papers you published but how many have you published in *Nature* or *Science*, which they make a distinction between a paper in *Nature* or *Science* and in some other journal, and if you don't have *Nature* and *Science*, you're not really first class. And Randy has correctly pointed out that just makes no sense.

05-01:25:49
Burnett: Yeah. [laughs] Impact factor being this metric of excellence, and— Do you agree with that, to some extent, or—?

05-01:26:00
Prausnitz: Well, it's one measure, but it's by no means the only thing, and in this particular case it's the impact factor of the journal, not the impact factor of the article that you submitted to the journal.

05-01:26:12
Burnett: Right, right. And also, if you're looking at numbers of publications, that doesn't necessarily tell you—

05-01:26:18
Prausnitz: No. I think Einstein published thirty papers in his lifetime, something like that. And we have chemists around here who've published close to a thousand.

05-01:26:31
Burnett: Yes, so there're expectations. There are metrics we have for excellence in science, and those have evolved over time.

05-01:26:46
Prausnitz: Well, this is the curse of the administration. They want numbers. They like numbers, because then they can justify their decisions and say, "Well, I promoted this guy but not this one because he has higher numbers." I saw this on the Budget Committee. I was on the Budget Committee here.

05-01:27:12
Burnett: Yeah. When were you on the Budget Committee?

05-01:27:13
Prausnitz: They like numbers.

05-01:27:14
Burnett: Yeah. When were you on the Budget Committee?

05-01:27:18
Prausnitz: Mid-eighties, but then—I served the usual period, but then later on I served as a substitute. Somebody would be on the Budget Committee, but go off for whatever reason, sabbatical or illness or what, and then I was called in to be a substitute, and that happened twice. I was twice a substitute for somebody who had to leave. So I've had quite a bit of experience with the Budget Committee, and wonderful, I loved it, I learned tremendously what's going on, on this campus, and it's absolutely stupendous, the subjects that are being investigated. It's incredible.

05-01:28:03
Burnett: There's nothing really like it at other universities, from what I understand.

05-01:28:07
Prausnitz: Apparently that's so, yes. Other universities don't really have that.

05-01:28:11
Burnett: Yeah, the trope is that you're evaluated by your peers, both within your department when you're coming up for review—

05-01:28:20
Prausnitz: And outside.

05-01:28:21
Burnett: —and outside, within your field, but the Budget Committee is this group of faculty chosen from across, and so you're evaluating performance in history. You're evaluating performance in—

05-01:28:33
Prausnitz: Yes.

05-01:28:34
Burnett: —in ethnic studies, for example. So—

05-01:28:37
Prausnitz: Oh, yeah, that's a tough one. Yeah, but—

05-01:28:41
Burnett: That has a history, too.

05-01:28:42
Prausnitz: But, yes, that's right, and I think this is extremely, extremely valuable. And many people claim, I think right, that the distinction of the Berkeley faculty is at least in part due to this procedure, where there's a budget committee that looks at you and say, "What have you done recently?"

05-01:29:02
Burnett: Right. It's a lot of work, isn't it?

05-01:29:05
Prausnitz: Tremendous amount of work, but I enjoyed it, and you meet interesting people.

05-01:29:11
Burnett: I bet.

05-01:29:11
Prausnitz: Everybody on the Budget Committee, they pick them very carefully, and I was quite honored to have been picked for it, and I met interesting, interesting people, yeah.

05-01:29:23
Burnett: Well, it satisfies a particular curiosity that you have: you're interested in learning about— It's almost like that year you had in Berlin, but brought to campus, where you can go and do those kinds of explorations.

05-01:29:41
Prausnitz: One thing I also learned, and I've learned this also in our recruiting efforts here at the department, is not to rely so much on letters of recommendation. Everybody writes a good letter of recommendation. It's very, very, very rare that you get a letter which says, "Well, this guy's not really bad but he's not really up to your standard." That doesn't happen. The letters are always wonderful. And, of course, that's in part due to the fact that they're not private anymore. They're not confidential, so people are reluctant to write anything that might conceivably get them into trouble. So the letters, I find—and this gets worse every year—aren't really worth a damn. You can't really tell.

05-01:30:30
Burnett: Right. [laughs] Yes. So you need— Well, you look at a number of factors—you talked about numbers; that's something that's important—but I imagine there are these conversations that take place.

05-01:30:47
Prausnitz: Yes. If we really want to know something about it, we would use the telephone. But a colleague is unlikely to put on paper anything that's considered not so good, because he might get himself into trouble.

05-01:31:03
Burnett: Right. [laughs] There's a reason why the telephone has persisted as a technology in this day and age.

05-01:31:12
Prausnitz: Thank heavens. Thank heavens, yes.

05-01:31:15
Burnett: You prefer, speaking of technology, to rely on—you're not as enthusiastic about electronic, or, sorry, digital communications. Is that a fair statement? Would you rather conduct business by telephone and by—

05-01:31:34
Prausnitz: No, I like email. No, email's okay. I don't do it myself. I always have somebody do it for me. It's ridiculous, but that's what happens. [laughter] No, email, I think, is very good, but when it comes to letters of recommendation, they're all the same. Some more so, some less so, so that you really have to read between the lines. And often, you can't even do that. There's nothing between the lines.

05-01:32:08
Burnett: There's nothing to interpret. It's such a form letter. Well, we've talked about the eighties and nineties. I wanted to ask you if there were particular students or postdocs from this period, apart from the ones we've already talked about, who stood out for you, or are worth remarking on with respect to the history of the department.

05-01:32:37
Prausnitz: Well, I did have some very, very good students, there's no doubt. One of them was a student from Mexico, whose name was Juan de Pablo, and he was one of the best students that I've ever seen here. And he is now Vice President of the University of Chicago, and I'm afraid he's becoming more and more of an administrator and less of a scientist, although he claims otherwise. I just saw him a few days ago. He was out here. We had dinner together, and he claims he's still doing science. Well, I'm happy to hear that, but I'm afraid it won't last much longer.

05-01:33:19
Burnett: Oh, at that top echelon, that's a real challenge.

05-01:33:23
Prausnitz: Anyway, he was one of the top students. He did very, very well. He first went to University of Wisconsin, where he was a very big success, but then things at Wisconsin deteriorated—and you may know about Wisconsin. They had Governor Walker, who had all these problems. So when he had a chance to go to Chicago he took it, and I think he did the right thing.

05-01:33:50
Burnett: His name came up when I was looking at "The Thermodynamics of Aqueous Systems," the 1989 paper. There's a paper that came out the year before that

you wrote with him, which involved a semi-theoretical correction to take into account the fluctuations of composition around mean equilibrium values. So this is another story about the complexity of composition, and—

05-01:34:27
Prausnitz: That's right.

05-01:34:27
Burnett: —there are these stand-ins or variables that are—I don't know if they're not constants, but they're these mean values that are—

05-01:34:40
Prausnitz: Mean in time.

05-01:34:41
Burnett: —mean in time, okay—to stand in, but there are—

05-01:34:43
Prausnitz: And you're looking at a point, and you say, "What's the composition?" Okay. Then you're looking again later. It's a little different. And so there's a distribution of compositions, of fluctuations. It's not always the same. And the typical models that we use in our work here, we don't pay attention to those fluctuations, but as we showed in that paper, and as other people have shown, too, for certain kinds of calculations we must take fluctuations into account. You can't just forget about it.

05-01:35:15
Burnett: Right. Well, especially if they fluctuate to a very, very large degree—

05-01:35:18
Prausnitz: That's right, if they fluctuate—

05-01:35:19
Burnett: —and that's why you want a kind of—is it a coefficient?

05-01:35:22
Prausnitz: As long as the fluctuations are small, hey, don't worry about it, but when they become large you've got to take them into account.

05-01:35:28
Burnett: Right. So this seems to be part of a way of working. You want to develop some representational theory that does some of the work, has some predictive value, but you need to constantly adjust for the complexity of nature. You start with the Gibbs constant; that's very good, but nature doesn't behave that way, and so you're constantly looking to refine that.

05-01:36:03
Prausnitz: Yeah. Well, depends on the circumstances. In some circumstances it does behave that way, but in others it doesn't. [laughter]

05-01:36:11
Burnett: Yes, absolutely. Are there other students that are from that period that are remarkable for you?

05-01:36:17
Prausnitz: Well, I had a very good postdoctoral fellow from MIT whose name was Leo Lue, and he was certainly very, very good, a theoretical type. He's not experimentally oriented. And he got his PhD at a very early age. He was sort of a boy genius. And as so often with boy geniuses, he was intellectually top notch but socially he's still pretty much of a child. And so he worked with a professor at MIT, whom I know reasonably well, and this professor at MIT said to me, "All right, Leo Lue is getting his PhD." I think he was twenty-two.

05-01:37:07
Burnett: Oh my goodness.

05-01:37:09
Prausnitz: "And so he's very good," et cetera, et cetera, "but I'm sending him to you so that you can make a real man out of him," or something, words to that effect. [laughter] Those were not the exact words, but something. In other words, he's behind in social skills and so on. So I noticed that when he came here; he is indeed intellectually very brilliant but his social skills, interpersonal skills, were still quite immature. So I put him in the lab on the second floor of Gilman Hall, but I also put him in the same lab as a lovely Italian girl who came here as a—did she get a PhD? No, she didn't get a PhD. She was a special student from Italy, very good, who came here just to do research, and she then used the research she did here to get her degree in Italy. Anyway, I put them together intentionally, and that did wonders. It really worked. He flowered with her, and they finally got married, and they're living now in England.

05-01:38:24
Burnett: You hooked them up?

05-01:38:25
Prausnitz: I hooked them up. [laughter] I didn't know it would go to that point. I just wanted them to get together. I didn't necessarily want them to get married, although it was okay, but that was not in my plan. My plan was just to give him something to become socially more at ease, shall we say. And now they have two children. He is at the University of Strathclyde in England, in Scotland, I guess it is. He's doing very well. And she's also doing some sort of professional work; I'm not sure what. So I feel very good about that. And I wrote back to this professor at MIT, "Mission accomplished." [laughter]

05-01:39:08
Burnett: We were saying earlier that science is social.

05-01:39:13
Prausnitz: Yes.

05-01:39:14
Burnett: All right, well, perhaps we should pause for now, and we'll take up next time.

05-01:39:18
Prausnitz: All right.

Interview 6: February 13, 2019

06-00:00:14
Burnett: This is Paul Burnett, interviewing Dr. John Prausnitz for the University History Series, and this is our sixth session, and it's February 13, 2019. So, Dr. Prausnitz, we were talking last time about the move towards modeling these chemical interactions, and a lot of that, of course, involves computers and computing power, increasing computing power. And I'm wondering if you could talk a little bit about, perhaps, some examples of some of the work you did, perhaps with Monika Prange and Herbert Hooper, on water-soluble polymers, how modeling is changing into the 1980s and into the '90s, and perhaps commenting about what it's like now, in 2019, and what it's likely to be like in the near future.

06-00:01:22
Prausnitz: Well, what we've always done—and with Monika Prange, that was an example—is we've tried to make simple mechanical models. In other words, we have a little imagination, and we picture a box, and various things are in the box, and we picture how the material in this box interacts with the surroundings, and we try to build a mathematical model that reflects the physical reality to the extent that we know it. Now, the important point in the model is that we neglect all sorts of things, and the choice of what you do not neglect is very crucial. You want to know what is really important, what is primary, and what is secondary, and to build a model which includes secondary or tertiary effects is very difficult, almost impossible. You want to build a model that talks about the primary effects and forgets about the others. So it takes some imagination to do that, and I always remember a quote from Einstein; I don't have the exact words, but somewhere he says that in research the most important thing is imagination. Knowledge is, of course, useful, but imagination is essential. Einstein kept thinking about what he called *Gedankenexperiment*, which is thought experiments. And that's a very useful idea in building a model. So let me just continue for another moment.

06-00:03:10
Burnett: Sure.

06-00:03:16
Prausnitz: As computing power increases, you can include more effects. When you have limited computing power, you just do the primary effects and don't worry about the secondary, but as computer power increases, and also as our knowledge of physical effects increases, you can include the higher-order effects. And I expect one day that, through simulation, using quantum computers, we can do a pretty complete job in getting the properties of materials from what I would call semi-fundamentals; in other words, semi in the sense that you have to know how the particles interact, but then, with a good computer program, that's all you need to do. At the moment, this can be done in principal, but it takes much too long. The computing power isn't high enough. That'll change. Computing power is going to increase. And I can see

the day where we won't really need models at all. Models will disappear, because we will simulate everything, and this will be especially important in controlling a process. There has to be a sensor, which senses what the composition, temperature, and pressure are, and then that information goes through the computer, and the computer figures out what will happen. And then, depending on what will happen, the control elements will step in and do whatever needs to be done to make sure the plant doesn't blow up, and that the process is working properly. And the important question there is time. How much time is there between sensing the information and coming up with a response? Today, by simulation, that's much too much time. You don't have enough time to do that. In the meantime, the plant may have blown up. So you've got to reduce that time, and computers will do that. And I can see in the future where a computer can give a response to the input signal within seconds.

06-00:05:38
Burnett: And is it true recently they've been able to image in real time a molecule, that there's work on that? But that's a different order of business from— [laughs]

06-00:05:51
Prausnitz: That's something else, with sort of a super microscope, and certainly there's a lot you can do with microscopy, but that's usually for large molecules. You can see polymers, and I don't know enough about that to know what the future's going to do there, but there are, of course, these studies where you have optical pincers. Using optical methods you would hold on to a molecule—this is a big molecule now—and then see what happens to that molecule as you change the conditions. And one of them is flow. You hold on to this molecule, like one end of the molecule, you hold on to it, and then you flow fluid over it at various velocities, various temperatures, and you can see how the molecule reacts. So that's very useful in hydrodynamics and fluid-mechanics problems. And that's something that was a Nobel Prize-winning research about, oh, fifteen years ago or so, and certainly that's a wonderful technique. But that's really fundamental science. It's hard to know, but at the moment I don't see much connection between that and chemical engineering, but that may change.

06-00:07:17
Burnett: No, but just as an example of processing power that is now coming online, but with chemical engineering you're talking about the thermodynamics of the interactions among various molecules, what it takes to separate two mixtures that are together or combine—

06-00:07:36
Prausnitz: Well, that's half of it. The other half is reaction. After all, we want chemical products, and we get them from reactions, and you want to know not only what the reactions will produce—thermodynamics can tell you something about that—but you also want to know how much time does it take to run the reaction, and how does it change with conditions, like temperature. I have not

worked in that area, but I don't want to overemphasize separation. Separations are one important part of chemical engineering, but there's, of course, other parts.

06-00:08:16
Burnett: Right. Well, and there's different techniques. There's adsorption, and you're looking at these different chemical interactions, let's say, as a body of things, rather than just one. There are all of these different elements to it.

06-00:08:29
Prausnitz: Yes, things get more difficult when you have lots of molecules, rather than just one or two.

06-00:08:36
Burnett: Yeah. And we were talking about oil, for example. So crude oil, this is hundreds of different compounds, right?

06-00:08:47
Prausnitz: Yes, yes.

06-00:08:48
Burnett: Yes, different components of a compound. And so the modeling for the relationships among these molecules is very data-intensive now, and to get a more accurate picture, there's going to have to be a lot more advances in computing. Now, chemical engineers here at Berkeley, do they get time on the so-called supercomputers on campus?

06-00:09:21
Prausnitz: Oh, yeah. Yes, they have to apply for it, but yes, not only on campus but also elsewhere. I think San Diego has quite a few supercomputers that are used by Berkeley people. So, oh, of course. Of course chemical engineers use them, yes. But they're still not fast enough for many applications.

06-00:09:43
Burnett: Right. That's incredible. After all we see in the news about the exaFLOPS of the processing power of the latest supercomputers.

06-00:09:54
Prausnitz: Oh, it's impressive, yes.

06-00:09:55
Burnett: Yes. But it's still not enough.

06-00:09:58
Prausnitz: Well, for certain problems. It's enough for some problems, but not enough for others.

06-00:10:02
Burnett: Now, you mentioned you had an office up on Lawrence Berkeley Laboratory. Are there other relationships with other national laboratories among the—?

06-00:10:16
Prausnitz: I didn't have an office up there. I was never there physically. I had an appointment so that I got some research funds, but all research was done here in Gilman Hall. Well, no, that's not quite true. There's also a building called the Biological Sciences—oh, I don't remember.

06-00:10:40
Burnett: Bio Science Building?

06-00:10:41
Prausnitz: Yeah, I don't remember the exact name of it, but, anyway, there's a building about a quarter of a mile away from here where we did some research. But no, I never had any physical space up at the Lawrence Berkeley Laboratory, no.

06-00:10:56
Burnett: And are chemical engineering faculty working with other national laboratories nationwide?

06-00:11:06
Prausnitz: Yes, especially when it comes to neutron diffraction, scattering of neutrons. We don't have anything like that here at Berkeley, and so the place where you get that is in the laboratories of the National Institute of Science and Technology, NIST. They have a laboratory near Washington, D.C., in Gaithersburg, Maryland. And I know at least one of my colleagues, probably more than one, goes there occasionally for neutron scattering work. That's one of the few instruments that we do not have. We have just about everything else, but we don't have that. So there is cooperation there, and I think my colleagues have their various forms of cooperation, their various laboratories that they cooperate with.

06-00:12:05
Burnett: I want to turn to talk about more of your students and your colleagues and people you collaborated with. I wanted to ask about the work with Jianzhong Wu on bovine serum albumin solutions, and some of the challenges of modeling proteins. And I'm curious about this, the many-body effects of small ions and water molecules.

06-00:12:36
Prausnitz: Yes, Wu was one of my most successful coworkers. I'm still in touch with him. He is now a professor at UC Riverside, and he's been very successful there, and he knows a lot about theory, much more than I do, and he and I are working on a book on applied statistical mechanics, so I'm still very much in contact with Wu, and there's also a small personal friendship. When he came here, I asked him to do some experimental work on osmotic pressure of protein solutions, and he built an apparatus for doing that, relatively simple apparatus. The big problem that we face—it's not anything new—when you do osmometry, you need a membrane, and the membrane has to be selective. In other words, the membrane has to let certain molecules go through, and other molecules should not go through. And so that's an important point. You

have to find the right kind of membrane, and we did. We had a good membrane, which allowed water and salt ions to go through, but it prohibited a protein from going through. And we built an apparatus. It was nothing unusual—it was rather classical—but it worked for a large concentration. We wanted to cover a wide range of protein concentrations, because we were interested in predicting when the protein would precipitate, what is the solubility. And these osmotic pressure measurements give you information from which you can calculate solubilities.

So that worked out very well. And the way you would describe the osmotic pressure of a protein solution—again, this is nothing new—is you write what's called a virial series. It's a power series in concentration of a protein. And the first coefficient is just unity, and that expansion, and the second one is the second osmotic virial coefficient, and you keep going, you have a third osmotic virial coefficient, and so on. So we used the virial series—and, again, that's the classic way of doing it—to interpret the data, and once you have the second virial coefficient then from that you can figure out what the potential is between protein molecules. This is, again, nothing new; it's a standard way of doing things. And once you have the potential, you can then use statistical mechanical theory to describe the properties at various concentrations, even very high ones.

So there are theories for doing that, and that's what we did. We measured as carefully as we could the second virial coefficient, and then from that we got a potential function, which tells you how two protein molecules interact, and with that and some statistical-mechanical relations we got an equation of state, and from that we were able to calculate phase equilibria, and from that we were able to predict at what point a new phase would form, phase splitting. So that was a very nice piece of work. I certainly enjoyed that. And Wu is not primarily an experimentalist—he is definitely a theorist—so it was good for him to do some experiments, and I'm not sure he liked it too much, but he agrees that it was a good experience for him.

06-00:16:46
Burnett: We talked about your role in the fifties in bringing physical chemistry and bringing some theory into chemical engineering and formalizing a lot of it, and—

06-00:17:06
Prausnitz: That's right. This was an example of that.

06-00:17:08
Burnett: Yes, and you're also shepherding theorists into the experimental world, as well, and that seems to be—

06-00:17:19
Prausnitz: Right. Well, that's an educational objective.

06-00:17:22
Burnett: Yes, and do you think that the pendulum, for you, at a certain point, say, in the 1980s, did that shift for you where you felt that that was more of what you were doing? You were finding—I think we talked about this a little bit—you were finding that you had chemical engineering graduate students who were really theoretically-oriented—

06-00:17:45
Prausnitz: Right.

06-00:17:45
Burnett: —and that you were trying to push them more in the experimental direction.

06-00:17:51
Prausnitz: Well, and vice versa. I've had students who were afraid of theory, and wanted to do experiments, and usually did them very well, and I pushed them a little to learn some theoretical material. I'm always unhappy if we give a PhD to a student who does only experiments, or only theory. Unfortunately, that does happen, but I tried in my group not to do that. I don't always succeed. I sometimes find that some students are so clumsy that if you put them in the lab they'll break the equipment and possibly hurt themselves, so there's no point in pushing it. But, fortunately, that's rare. But I have had a few where that was the case. And, no, I try to get a thesis topic where you have some of each.

06-00:18:48
Burnett: You mentioned dexterity as kind of a basic minimum, [laughs] so if you're clumsy with chemicals, and some of which are dangerous, this might be a problem—

06-00:19:01
Prausnitz: And glassware.

06-00:19:02
Burnett: —and glassware, right?

06-00:19:03
Prausnitz: You can cut yourself, yeah.

06-00:19:04
Burnett: Right. So that's a kind of minimum. But can you talk a little bit about the skills that you really want to cultivate in an experimentalist and a theorist, physically and mentally? What is the kind of agility that you want to cultivate in a student?

06-00:19:23
Prausnitz: Well, the main thing is to be critical. In other words, what [Alfred North] Whitehead called "Seek simplicity but distrust it." In other words, if you go and measure something, you should ask yourself: do I really believe this? Is it really right? Have I forgotten something? Did I take all the precautions? Are

we sure that I didn't forget something here? In other words, be very suspicious of your results. That's one of the things I try to teach, and that's hard to do. If a student, with much work and much diligence, measures something, he wants to believe, oh, this is okay, this is the right answer. Well, maybe. Maybe it is, but maybe it isn't. And so I think students should be made aware of the fact that you have to be extremely suspicious and careful. And the same thing in calculations. There's a story about a Princeton physicist—I can't remember his name at the moment, but he's a very famous Nobel Prize-winning physicist—who had a student who wanted to compute everything on the computer, and so he comes in to see the professor with reams of paper, what the computer has spit out, and the professor says to the student, "Well, I'm very happy that the computer knows how to solve this problem, but I would like to know it, too. And so, tell me: what does all this mean?" And that's a very good attitude. If the computer understands a problem, that's fine, but that doesn't mean that I understand it. And there are some students who believe the computer, and the computer is sort of the standard. If the computer says so, it must be right. Well, of course, that's utterly wrong. It may be right, but you've got to be very critical. So that's the kind of attitude. I like to encourage students to be very critical of what they do.

06-00:21:43
Burnett: Well, I imagine it's an increasing problem as computing power increases, as techniques are elaborated, and as bodies of knowledge become somewhat ossified; that is, that they become extremely complex and highly specialized, and standardized such that graduate students kind of run to the edge—I've talked about this with someone else I was interviewing—you run to the edge of the frontier of knowledge, based on all of these layers of trust that the existing body of knowledge is correct—

06-00:22:17
Prausnitz: Yes.

06-00:22:17
Burnett: —and you go to the frontier and you say, okay, now what's this tiny problem here at the edge, and how does that confirm or refute what I've learned so far? There would seem to be a kind of danger there, and you've pointed to this moment where there are these results, but you're saying there's an interpretive or hermeneutic problem. You have to say what is the significance of those results, and the results are extremely complex, and you have to sort out and identify. So, on the one hand, you're worried about reductionism, being overly simplistic in your conception of nature, and the problems that you're looking at. And, on the other hand, just being awash in a sea of complexity, and just as a graduate student throwing your hands up and saying, "Well, this is what the computer said; I'm done," [laughs] and leaving it at that. And so your role seems to be you've got this wealth of experience—and real experience, not just elaborating theory, but experience as an experimentalist, designing experiments, wrestling with them, finding that things can go wrong. And so is that how you encounter the graduate student?

06-00:23:51
Prausnitz: Well, you question. You keep questioning. How do you know this? How do you know that's right? What have you forgotten? What assumptions did you make? Are those assumptions valid? Did you check that at all? In other words, to be very critical. And that's often very difficult. A student is proud of his achievement, and he doesn't like it when I say, "Well, how do you know that's right?" You have to do this very diplomatically. You don't want to insult the student. And, of course, this criticality applies not only to what the student does but what I do. I constantly have to remind myself, is this really okay? Did I forget something? Did I make some unreasonable assumptions? On the other hand, in order to get a result you have to make assumptions. Without assumptions you cannot get anywhere. And I remember the wonderful statement of Joe Hildebrand. He said, "If you're going to make assumptions, don't just make one or two. Make lots of assumptions, because there's a good chance that they will cancel each other in the final result."

06-00:25:07
Burnett: Really?

06-00:25:07
Prausnitz: And I think that is true. We know, for example, if you get some result making a lot of assumptions and it turns out experimentally it could be okay, not perfect but pretty good, so then you say, well, now, this one assumption is probably not right, so I will recalculate, not making that assumption, and the answer that you then get is very bad, because you've just fixed one assumption but not all the others, which have probably canceled that assumption. So there's a lot of truth in what Hildebrand said. It's sort of a joke, but I think there's a lot of truth in it.

06-00:25:49
Burnett: Well, there's a sort-of gold standard of experiments—and maybe this is wrong, but thinking about holding all other things constant, controlling the environment so that there's the one variable, and you can test that one variable.

06-00:26:11
Prausnitz: That's very hard to do.

06-00:26:13
Burnett: Isn't it?

06-00:26:14
Prausnitz: Very hard to do, yeah.

06-00:26:16
Burnett: Because they're not held constant in nature.

06-00:26:18
Prausnitz: No, that's right. That's right. You cannot hold everything constant. It's not physically possible.

06-00:26:26
Burnett: And it doesn't reflect what actually happens in nature.

06-00:26:30
Prausnitz: But then, if you compute something, and then you measure something—well, somebody else may have measured it—and you see there's a difference, significant difference, which one do you believe? The general tendency is to believe the experiment, and, well, maybe, and I've run into big trouble when I was an assistant professor. There were some results that were published in the University of Michigan, some vapor liquid equilibrium results, that I figured cannot be correct. My calculations, theoretically-based, showed that that is just not right; it was way off. And I had the temerity to publish this, and the people in Michigan were not happy at all that I questioned their data. But, finally, I think I convinced them. I think I found the experimental error that was made, and I think they finally begrudgingly agreed that the calculations were probably more correct than the experiment. But that's rare. Usually one gives preference to the experiment, but one shouldn't always do that.

06-00:27:49
Burnett: So, going back to your undergraduate training and your exposure to philosophy of science, is that partly that training, or is that an orientation that you had that drew you to that coursework where you think about epistemology, you think about the theory of knowledge, going into your work?

06-00:28:18
Prausnitz: Yes, yes, I'm sure. No, the history of science came first, I think, and then my attention to critical thinking came after that. You say epistemology, and if I use that word around here hardly anyone will understand what I'm talking about. You have a training, so for you the word "epistemology" is common. It's just like "bread" or "salami." [laughter] But around here, if you say "epistemology," people go, "What is that?" They don't think about that. And that's been one of my hopes and efforts, to get people here in the College of Chemistry to be more broad, and to read history and philosophy and so on. It makes them better scientists. And as I say in one of my articles, it makes me a better conversationalist at dinner parties. [laughter]

06-00:29:28
Burnett: This is very true. This is very true. Well, I wanted to ask you. I don't know how much this was a part of your life, but there was something called the Bronowski Project?

06-00:29:43
Prausnitz: Yes. [Jacob] Bronowski, he's no longer living; he died about twenty years ago or so. He was, I think, a mathematician originally, in England, and he had various positions in scientific enterprises, but then he started to think about philosophy, think about it a little bit, and he wrote some lovely short books. You should really look at that; as a historian of science, you would enjoy them. And he—

06-00:30:16
Burnett: Well, he had a television program, didn't he?

06-00:30:18
Prausnitz: He had a television series, yes. It was on Channel 9 here in the Bay Area. Wonderful. It was a wonderful series. I guess you could still get it, maybe on YouTube. I don't know. And he showed how—I think it was called *The Ascent of Man*.

06-00:30:36
Burnett: Correct.

06-00:30:36
Prausnitz: Not the descent, but *The Ascent of Man*. And he showed the interaction between science and other forms of knowledge. I don't know if he coined the phrase, but we now say "branches from the same tree," which I think is a very apt description, the various branches of knowledge joined at their roots to the same tree. And so I worked here on what I call the Bronowski Project. We worked up case studies, which discuss the intersection of science and society, or science and art, science and philosophy, and so on. And the idea was that these case studies should or could be used in existing courses by professors of chemistry and chemical engineering, so that the idea was that a professor, in his regular course, when he talked about something, would take ten minutes maybe to discuss one of these case studies that was relevant to what he was talking about, to make students aware that science does not exist in a vacuum, but exists in society and interacts with other areas, and so on. That was the idea. And so we worked up—I think we have sixty case studies now we worked up. I had students do this, not just in chemical engineering; students from all over the campus came. And I still think it's a good idea. To what extent it's used, I don't know. The case studies are available. Anybody can download them from the computer without any cost. So they're around. To what extent they're used, I don't know.

06-00:32:32
Burnett: When did you undertake this project?

06-00:32:34
Prausnitz: Oh, this was a multiyear project. This went on for several years. We finished it, well, it's really never finished, but we stopped working on it about three or four years ago.

06-00:32:47
Burnett: Oh, so fairly recently.

06-00:32:48
Prausnitz: It's relatively new. And I call this the Bronowski Project, but now I ran into some strange difficulty. I thought that to protect myself legally I better contact the Bronowski family and see if it's all right with them. And I did, indeed, find one of Bronowski's daughters is a professor in England. I don't know what subject, but anyway, she's a professor at some English college. And so I

contacted her, and I told her what the Bronowski Project is, and so on. And I asked for permission to use the name. And she said no, she didn't want it done.

06-00:33:29
Burnett: Interesting.

06-00:33:29
Prausnitz: I was very surprised. And she checked with her sisters—I think there were several daughters—and the final result was no, they didn't want that. Well, it's very strange. It seems to me it's an honor for their father, and something that I'm sure they approve of. How can one not approve of it? But for reasons that are unknown to me, they said no, they didn't want that. So I had to change the number—the name, rather—and it's now called the Leonardo Project, [laughter] in honor of Leonardo da Vinci. And maybe that's a good thing, because people know who Leonardo was; only a few people know who Bronowski was.

06-00:34:16
Burnett: So it's worked out for the best.

06-00:34:17
Prausnitz: I think so.

06-00:34:18
Burnett: So this is meant to be used at the undergraduate level in science courses.

06-00:34:22
Prausnitz: Yes. Well, it could be used in graduate level, too, but the point is that I want very much to introduce what I call non-science knowledge, non-science thinking, into our regular curriculum. And classically, the way people have done this is they say, okay, we'll add a course in philosophy, or a course in history, or whatever. That does not really work. Furthermore, there's no room. The curriculum is full. And so I much prefer that topics in philosophy, etc., should come up in the regular courses that we already have, not much but, say, two, three, maybe four times in a semester, that one of these case studies be talked about, and just to make the students aware. Now, I've done this, of course, in my own undergraduate teaching, and I find that whenever I do that, the students really perk up. They just love it. And I usually do it after about a half hour, in the middle of the lectures, because people tend to become a little sleepy, and so when I do this they wake up tremendously, and there are all sorts of questions afterwards, and so on. So I think that's the way to do it, because that way you don't need an additional course. It doesn't cost any money, so the dean will like it. But it's, of course, a new idea, and new ideas take hold very slowly. Academia is tremendously conservative. They don't want to change.

06-00:36:11
Burnett: It's right with the times, if not ahead of its time, because the Next Generation Science Standards, which are the national standards for K-12 education, that's a hallmark of them: it's these cross-cutting concepts of thinking about the context of learning, of knowledge production, the context of science, putting it in the world, in history, so that people understand why something was undertaken, and what its purpose was, and what the problems were, what went wrong and what went right. And that is considered to be a best practice pedagogically, that science is not in a vacuum, and that maybe accounts for some of the sleepiness, right? [laughs] Is that you're getting this denatured, rarefied essence of knowledge that is not rooted in anything real or human. And I think that that's where the education experts have landed with respect to K-12 education, and I guess it's up to individual faculty members, because of the nature of higher education. If you don't have that orientation, students will not get that exposure.

06-00:37:31
Prausnitz: Yes, as you get older the connections between the various branches becomes more obvious, but for a typical undergraduate it's very hard for him or her to see that. You need to help him or her to see the connection. They don't see it yet. They will later on, perhaps, but not at the time they're students. No.

06-00:37:53
Burnett: Yes. I will say that when I was at a liberal arts college, we offered courses in science and technology studies, and down the hill, at the University of New Brunswick, there were engineering students who would come up the hill, and they drank it up, and they were excellent, and they were really—they found it really eye-opening—

06-00:38:16
Prausnitz: What's the hill? I don't understand what you— up the hill and down the hill. [laughter] What hill are we talking about?

06-00:38:20
Burnett: Forgive me. In Fredericton, New Brunswick. And so I was teaching at the St. Thomas University, and down the hill was the University of New Brunswick, and they had an engineering school, and they would send students up there. Students would opt to take courses in science—

06-00:38:34
Prausnitz: St. Thomas is a liberal arts school?

06-00:38:37
Burnett: Yes, that's correct. And so they would take science and technology studies courses, and they were tremendously enthusiastic and excellent critical thinkers, and you can sense that there's a thirst for that among the undergraduates.

06-00:38:51
Prausnitz: Oh, yeah, absolutely. I've certainly sensed that.

06-00:38:54
Burnett: So it's a way forward, perhaps, down the road. But for now, the case studies have been completed, and they're available. Are they online?

06-00:39:12
Prausnitz: Yes. They're available online. You just have to get the right connection. And I put a little article, a very short article, in the *Journal of Chemical Engineering Education*, where it's been announced. So I don't know to what extent people have responded. I suspect very few responses. Professors don't like to be told what to do. Professors don't like to step outside their comfort zone. They want to talk about their particular thing, and they don't want to take the time to discuss other ideas. They feel uncomfortable doing that. So it's an uphill battle.

06-00:40:01
Burnett: Well, we were speaking of the importance of critical thinking, and reevaluation, and the need for assumptions, and yet the problem of assumptions, the thorny problem of assumptions, I think you published a short article in 2015 which was about a paper in 2012 that had challenged a kind of hallmark of some of the research that you and others were working in. And it was about the demise of the colloidal-like theory of globular protein solutions.

06-00:40:43
Prausnitz: That's right.

06-00:40:46
Burnett: Can you set up the history of the development of that theory of globular protein solutions, and then what happened in 2012?

06-00:40:58
Prausnitz: Well, we have some data—by "we," I mean the scientific community—on how proteins behave in solution. Usually, it's an aqueous solution with some salt in it. So there's quite a bit of information available. And in order to describe this, people—and I've done this, too—people use the theory of colloids. And the assumption then is that a protein molecule behaves like a colloid particle. A colloid particle is round, and it usually has a charge on it, an electric charge, and so that's why we say globular. Globular is almost round, [laughs] not quite round. And so there have been quite a few theoretical papers, and I contributed to that, where you discuss the thermodynamic properties of protein solutions through colloid theory. There's quite a bit known about colloids, theoretically. And this works sometimes, but as this article in 2012 showed it really doesn't always work, because calling a protein a colloid particle is just not right. A protein particle is much more complicated than a colloid particle, especially in its dimensions, which, unfortunately, change with concentration. You get a certain amount of denaturation out of it, the protein molecule sort of opens up. The folding is not as tight under some conditions as it is under others. And, of course, colloid theory doesn't allow for that. And so I pointed out in this little article that statement, mentioned now several times, of Whitehead's: "Seek simplicity, but

distrust it." So the first part is simple, and in this case the "Seek simplicity" means, well, okay, we'll use colloid theory to discuss this. But then people forgot the second part: they don't distrust it. And if you do distrust it, you find out that—again, depends on the circumstances—that colloid theory is just not applicable. Sometimes it is, but sometimes it's not, and people have been overdoing it. So that was the point of that little article. It got a lot of attention.

06-00:43:43
Burnett: Yeah. And you brought in Alfred North Whitehead, and you brought in this kind of—

06-00:43:46
Prausnitz: Yes, and I'm sure most people never heard of Whitehead. [laughter] He was—

06-00:43:51
Burnett: And protein folding is a huge area of research, because people are interested—

06-00:43:56
Prausnitz: Yes, yes.

06-00:43:56
Burnett: —in curing Parkinson's disease, and protein folding is an important component of that, as with many other diseases. And so this is a very critical research area to be focusing on. So it was the Sarangapani group that came up with the research that—

06-00:44:16
Prausnitz: Well, it was just experimental. I shouldn't say "just"; that's terrible.

06-00:44:21
Burnett: But it was a challenge, yeah.

06-00:44:22
Prausnitz: But it was a challenge to interpret it, and it showed that colloid theory is, for those conditions, not applicable.

06-00:44:33
Burnett: I want to turn to talk a little bit about—we did discuss this—the work of Clay Radke, who's interested in lithium salts. And—

06-00:44:51
Prausnitz: No, there's one correction. No, he really isn't. I'm interested in lithium salts, [laughter] and I'm interested in working with Clay Radke. He's a wonderful collaborator. He was one of my own PhD students, and I think he is probably the brightest faculty member we have in the Department. So I want to work with him, and I'm interested in lithium fluoride, and I'm trying to get him involved. So I'm the guy who started all this, and I'm pushing him to help. [laughter]

06-00:45:21
Burnett: Well, I think what we left off with that was the more recent work on separation processes for, I think, Yanjun Sun and Clayton Radke—I think it's 2018, so this is really recent. Can you talk a little bit about where this research is going, and how it's going?

06-00:45:42
Prausnitz: Which paper is this?

06-00:45:45
Burnett: Now I don't have that.

06-00:45:46
Prausnitz: Who was the first author you named? Jansun?

06-00:45:49
Burnett: I think it's Yanjun Sun.

06-00:45:53
Prausnitz: Oh, Sun, yes. Oh, yeah, yeah, he's a Chinese visitor. Yeah, well, he measured some solubilities. We don't really know much about solubilities of lithium salts in various solvents. And so that work was primarily experimental. We have experimental equipment here for doing that. And there's nothing really very exciting about it, but it's just some fundamental data. We also made some other measurements. We measured conductivities, and I think that was the only other property we measured. And then, with Sun, we looked at the surface-tension effects on an electrode. You would like a fluid, you would like it to wet the electrode. In other words, if you have a surface and you put a liquid on it, the liquid may either wet the surface or may form little droplets. If I have, say, a steel surface, like in cooking, and I put in water, the water spreads, but if I put in a heavy oil it forms little drops. So this depends on surface tension effects, and that was one of the things we studied with Sun. When you have an electrode, and you have various liquids in contact with electrodes, you get wetting, or you don't. So we had some measurements that were made of that. So that was that work.

Actually, Radke is primarily interested in oil recovery. He spent some time—twice he went to Arabia. He was the guest at the KAUST University, which means King Abdullah University of Science and Technology. It's a wonderful place in the sense that there's oodles of money—King Abdullah put a tremendous amount of money into this university, and it's a modern university in the sense that, first of all, you have women there, which is unusual, and you have a lot of foreigners there. It's sort of an oasis, you might say, in Arabia. It's on the Red Sea. So from the point of view it's a research heaven, because the facilities are limitless, and if you want some equipment, or you want something, they'll get it for you. The one thing you cannot have is alcohol. You cannot get a drink, at least not legally, so that's one limitation. This was a matter of some concern, because one of our colleagues here in Chemistry, Professor [Jean M. J.] Fréchet, a very famous polymer chemist, he's also a great connoisseur of wines, and he has a huge wine cellar with literally thousands of bottles, huge reservoir of wine. And he went to KAUST as a very high executive and could not take his wine with him, and so this caused all sorts of interesting stories, but as far as I know the wine is still stored here somewhere in the United States.

But anyway, Clay Radke was there twice for several months, and he's been interested in oil recovery, and so we're doing some work together with him on oil recovery. So I don't want to go into details, but we've also published a few papers in that area.

06-00:49:56
Burnett: Well, I think just getting a sense of the range of the things that you have been involved in, not just serially but concurrently, your concurrent interests—so we could talk about bioconversion, the work that you did on ionic liquids with Waheed Afzal and Brian Yoo.

06-00:50:16
Prausnitz: Yes.

06-00:50:17
Burnett: There's a 2011 paper on the recovery of glucose with Maria Francisco and Anton Mlinar.

06-00:50:22
Prausnitz: Yes, we worked on various methods how to get glucose out of various plants. Well, we worked with a particular plant called miscanthus. And then, once you have the glucose, you're on your way to making ethanol, and ethanol, of course, is a fuel you can put in ethanol with your gasoline. In fact, that's being done all the time in the United States. And the problem is: how do you get ethanol cheaply and sustainably? In other words, you keep growing the plant so you're not using fossil fuels. So it's attractive to do that. And, like many others, we worked on processes for doing it, and ionic liquids come in in some of the processes. And it was primarily experimental work. There was very little theoretical work in that area. You can't do much theory.

06-00:51:24
Burnett: I can't really engage on the science of this, but one of the striking things from the recovery of glucose article is one of the separation processes that was being discussed is actually something that's used in flotation processes to separate ore from waste rock. And so just the notion that you and your colleagues are thinking about existing processes in other domains, and importing them, adapting them trying them out—

06-00:52:00
Prausnitz: Oh, sure.

06-00:52:02
Burnett: —and seeing what we would have to tweak to make this work for biofuels.

06-00:52:06
Prausnitz: That's right. No, no, we do that all the time. If we know about something that works well in another area, we ask ourselves, well, maybe I can use that in my area. So that's a common procedure that we do all the time. We came up with several processes for getting ethanol from miscanthus, and they work, it's okay, but they're not economic. Oil has to get a lot more expensive before these methods become economic.

06-00:52:39
Burnett: Well, can you put a ballpark number on that, just to get a sense of how uneconomic?

06-00:52:45
Prausnitz: Well, if oil goes to, say, 100 or, even better, let's say, $150 a barrel, why, then these methods might become economical, but oil is now in the vicinity of, what, fifty, sixty dollars a barrel, and we can't compete with that.

06-00:53:06
Burnett: And so miscanthus is a grass. It grows in Southeast Asia, I think. And I guess it's wild grass.

06-00:53:15
Prausnitz: It's a wild grass, and the advantage is it grows easily. It does not need a very fertile soil. It can grow under particularly poor conditions. And it grows pretty high, and it gets up to six feet, or more. So when you harvest it, you have quite a bit of stuff, but it's only once a year, apparently. I kept asking the life science people, "Can't we get more than one crop a year?" But apparently that's very difficult. They don't know how to do that.

06-00:53:49
Burnett: So the ecological concerns and the economic concerns, would you farm it? Would you just sort of cull wild grass terrain? How—

06-00:54:04
Prausnitz: Yes, you would probably want to farm it. I really haven't thought much about that, but yes, you would probably want to have it centralized as much as possible, because transportation costs are not trivial. If you harvest all this material, you put it on a truck, you've got to send it to the chemical plant, which will operate on that material, and that cost is not trivial at all. So you want to have the chemical plant as near as possible to the source of the raw material. But that is, again, something that we haven't paid much attention to, just paid attention to the process, and it's too expensive. And as a result, the work in this area has completely died here at Berkeley. There were lots of groups working in this area. There was a whole laboratory built on Oxford Street, big building, to look into this, as well as other problems, related problems, and it all stopped. It was supported by the British Petroleum company. They gave us quite a bit of money to do this. And it went on for a number of years, but nothing really came out of it, nothing in the sense of

practice. A lot of science came out of it, but British Petroleum stopped, said, "Well, nothing's happening here that we can use." So they cut off the support.

06-00:55:40
Burnett: Well, also, the price of oil dropped precipitously. [laughs] Does that have something to do with it?

06-00:55:44
Prausnitz: We've got to double the price of oil.

06-00:55:48
Burnett: Yes. That's interesting. There's different techniques you had adopted or explored with Fuxin Yang.

06-00:55:59
Prausnitz: Yes. Well, I had some very good people there, primarily Chinese visitors. I never got any money for them; we had to do this all on the cuff, but we managed to do that, and we published, oh, must be six or seven papers, something like that, maybe even more. And it was nice. I enjoyed doing it. But, of course, it's sad that it cannot now be used.

06-00:56:29
Burnett: Yeah. Well, there's been several phases of this, is it coal liquefaction in—

06-00:56:39
Prausnitz: Yes.

06-00:56:40
Burnett: —in the seventies, and then it was stopped cold, and then—

06-00:56:43
Prausnitz: Well, coal liquefaction, a tremendous amount of money was put into that, and I worked on that a little, too, some of the problems there. And this was encouraged, primarily, by President Carter. He thought this would be a good thing to look into. And then, when President Reagan came, it just stopped cold. From one day to the next, just cut off, which, of course, made it very difficult for us, because we had students who were graduate students working for a degree, and they were paid from these funds, and the funds suddenly stopped, and we still had the student. So we had some tough times keeping ourselves afloat.

06-00:57:32
Burnett: Yeah. And that was pretty abnormal, though, because I think Reagan was pretty notorious for just shutting things down as a kind of political move to score points. Is that sudden turning off the spigot common?

06-00:57:51
Prausnitz: Oh, yes, it happens. Well, "common" may be too strong, but it does happen. What, of course, they should do is phase it out slowly, over a period of a year,

or two years would be even better, but sometimes they don't do that. Sometimes they just chop.

06-00:58:08
Burnett: So science operates as any kind of human endeavor: [laughs] it takes a while to get a project underway; and if you want to wind something down, it's hard to bring it to a full stop without losing— because you'd think there would be some kind of remediation effort. You could actually take care to wind something down, to record processes that—the tacit knowledge that's not recorded so that if someone wants to pick it up in the future you could do so. In other words—let me put this as a question—was information lost in the coal liquefaction case? Was it lost forever, because—

06-00:59:04
Prausnitz: No, I don't think it was lost forever. I think reports were written, so there is a record. Now, it may be that people were working on the project, and were about to get some results, and had to stop it suddenly. I don't know that for sure, but I think that's probably likely. But no, certainly the work we did here was all recorded, articles were written, so there is a record.

06-00:59:34
Burnett: Okay. And there's other work. So we've talked about protein work, the work on lithium salts, we've talked about bioconversion with miscanthus, and hydrogels, and contact lenses. Can you talk a little bit about that research?

06-00:59:56
Prausnitz: Yes, that's Clay Radke. Clay Radke is heavily involved in vision problems. He's done a lot of very nice fundamental work about how the eye operates, and he especially has been interested in dry eye. Millions of people suffer from dry eye, and they have to keep taking drops. So he's looked into that, and come up with all sorts of useful results. And he's interested in contact lenses. He's done quite a bit of consulting for contact lens firms, so knows a lot about contact lenses, and he wants to make better contact lenses, and contact lenses that are more comfortable. And one of the things that a contact lens must do, it must be permeable to oxygen. The eye gets oxygen, like other organs, from the blood, but, in addition, it needs oxygen from the air. So if you put goggles on, tight goggles, so that no air gets through, your vision will deteriorate, badly.

06-01:01:16
Burnett: Permanently?

06-01:01:17
Prausnitz: Well, that depends on how long you wear the goggles.

06-01:01:19
Burnett: I guess so, right. [laughs]

06-01:01:24
Prausnitz: So, of course, that's been known for years—this is not new knowledge—but that is one requirement. So when you make a hydrogel into a contact lens, you have to be sure that oxygen can get through. And some of the papers that we published together addressed that point.

06-01:01:45
Burnett: Oxygen consumption rates, for example.

06-01:01:47
Prausnitz: The oxygen consumption rate, right. And then Clay was interested in what happens after the oxygen gets to the eye surface, how is it used, where does it go, and so on. I haven't been much involved in that, but that's his major interest. But I have been involved with the hydrogel. How do you make a hydrogel that has various properties? Oxygen permeability is, of course, only one. It has to have mechanical integrity. It can't just dissolve or collapse. On the other hand, if you make it too tight, then it's not comfortable in the eye. So there are various things you have to look at, and we did some research in that area, which I think has been useful.

06-01:02:37
Burnett: So there's one paper you published fairly recently about chemical engineering thermodynamics in biotechnology, and it points to a number of things on the horizon, right? And so—

06-01:02:52
Prausnitz: Yes, but I quote some work that's been published, not necessarily my own, but work that's been done by others that I think has promises, and should be developed further.

06-01:03:06
Burnett: So there's a number of things: preventing cataracts by studying liquid-liquid equilibria.

06-01:03:14
Prausnitz: Yes.

06-01:03:14
Burnett: That's one domain.

06-01:03:15
Prausnitz: Yes, that's been very successful by people at MIT. There's a Professor Benedek. He's actually in the Physics Department. And he's looked at what are the conditions for a protein solution to separate into two phases, and he finds that if you just tweak the solution a little bit you can prevent it from doing that. And that's very important for the eye. You don't want two phases of the eye. You get little droplets in liquid, and that's very bad, because they will diffract the light. So he's done a lot of work in that area. I don't think it's been applied, for the simple reason that the eye surgeons have been so tremendously successful with cataract operations. I've had it in both eyes.

They just go in there and pull out the material in the eye, the lens, I guess it's called, and they put in an artificial lens, and that works beautifully. So the surgical method has been so successful that the sort of work that Benedek and others have done, it's just not used. Now, why not? I can only speculate, but, again, it's conservatism. People don't like to change. The surgeons have developed this very successful and universally-used technique—it's used all over the world—so why should they change? Especially since surgery collects a lot more money than putting a droplet in the eye. So there are some economic factors there, and there's just conservatism. There's really no need to change. The present situation of surgery is good.

06-01:05:18
Burnett: Right. And I guess, yeah, that's the question of following the science where it will go, but sometimes there's not an immediate, quote-quote, "market" for something. It's not going to get taken up by a particular industry, or any industry, necessarily, because it doesn't yield anything for them, or the cost of retooling— I think we talked about that with respect to the petrochemical industry in the fifties. They saw no need for years to consult with university professors about new techniques, because what they had was working fairly well, and they had a pretty solid monopoly on the production and supply of oil, so it wasn't a concern.

06-01:06:08
Prausnitz: Yes, my colleague, now retired, Professor Scott Lynn, he is a process design expert. He worked for years for Dow Chemical, so he really knows how to design a process. And he's come up with all sorts of very clever designs, and he's got them patented, but no one ever wants to be bothered, because, while it's true that he shows that economically it would be worthwhile to do this, it takes an effort to make the changes, and, of course, it costs a lot of money to scrap the old equipment and buy new equipment. So industry, by and large, looks at his patents and says, "Well, yeah, that's very nice, but we don't want to be bothered. We're doing it now. It may not be the most efficient way, but it works, and we're happy with what we're doing now." So he's not had any practical success from his patents, although his science and engineering is really superb.

06-01:07:15
Burnett: Well, a few years ago I interviewed Dr. Les Benet, at UC San Francisco, Department of Biopharmaceutical Sciences, and part of his story is that he studied chemical engineering as an undergraduate, and—

06-01:07:31
Prausnitz: He lives here in Berkeley, I think.

06-01:07:33
Burnett: I think he lives in Belvedere.

06-01:07:36
Prausnitz: Well, maybe I'm thinking of a different person.

06-01:07:39
Burnett: Maybe. So he talked about importing some new principles, and this was early sixties, mid-sixties, of chemical engineering to what was then pharmacology, which he and others transformed into the field of pharmacokinetics, which is studying how drugs move through the body, how they're eliminated, and so on. And so the concept of the black box in chemical engineering was applied to this problem, with great results. So where I'm going with this is drug development, drug testing, the interactions of drugs in the body, that's a huge area for what is, in effect, chemical engineering research. And this is something that you identify in this paper, isothermal titration calorimetry for AIDS drugs. Can you talk a little bit about that research, and—

06-01:08:44
Prausnitz: I've never done that, myself, but other people have, and the purpose of this article was to call attention to some of these ideas, not necessarily my work, but other people's work, and say, "Look, this is a useful idea. Maybe we should pick up on this, and work through this." Yes, it's a huge area, and it's very active at MIT. They have a lot of money there from industry to work on pharmaceutical problems. One of them is the isomorphism. If you take a drug and you have it in solution, you try to crystallize it, make it so you can put it into a bottle and store it. But there are various kinds of crystals that can be formed. That's why it's called isomorphism. In other words, there's various morphing shapes. There are various crystals you can get, and some of them are desired and some are not, and that's really a thermodynamic problem. And there's a lot of work going on in that area at MIT, also in England, at the Imperial College. And there's a big project at MIT on continuous processing, rather than batch processing, and I don't know how much success they've had, but I know that they're working with very, very good people, including the director, who's a Berkeley alumnus. Get that in there. [Bernhardt L. Trout, a former student of Alexis T. Bell, Director of the Novartis-MIT Center for Continuous Manufacturing]

06-01:10:16
Burnett: [laughs] Of course. We'll get the name and put that in there.

06-01:10:18
Prausnitz: So, yes, I agree that is a very active field, and it's one that I think chemical engineers should participate in. Yes, absolutely.

06-01:10:28
Burnett: So this is the problem with freeze-drying drugs: you have to understand the phase separation of liquids, and understanding when that occurs during the freeze-drying process.

06-01:10:40
Prausnitz: Yes, yes. We have a freeze-drying colleague here, Professor [C. Judson] King, who did a lot of work on that years ago. I mean, he's been an administrator now for a long time. But he did it with food. See, here's an interesting fact here: he loves to go into the mountains, camping, and, of course, you have to

take food with you, and if you can take dried food it's a lot lighter than taking wet food. So he was interested in drying food, especially turkey meat, I think, was one of his interests. [laughter] And he figured out a good way to dry foods, and freeze-drying is one way to do that, so that you keep the food's shape and its structure by freeze drying, and then you take it with you into the mountains, and you add some water, and you get the turkey meat. I've never tried this. I've never tasted this reconstituted turkey meat, but apparently it works pretty well.

06-01:11:53
Burnett: Well, the work on AIDS drugs is to help identify which drug molecules are more flexible, and could therefore bind more readily to the rapidly mutating surfaces of HIV molecules, right? So that's one of the things that shows how chemical engineering research can be really fundamental to these kinds of innovations. So I want to turn now to talk a little bit about recognition. You've received a number of—

06-01:12:34
Prausnitz: I've been very fortunate, yes. I received a lot of awards, and if you come to my study at home you'll see the wall is full of all sorts of papers, framed, signed by all sorts of people, including President Bush. Yes, I've been very fortunate. I've got lots of awards. I don't really know why that's happened, but I've been very lucky. People have nominated me for all sorts of things. And I got into the National Academy of Sciences fairly early. I was, I think, forty-three or forty-four years old at the time, which—

06-01:13:15
Burnett: Well, it was '73 when you got it.

06-01:13:17
Prausnitz: So I was forty-five. Yeah. And that's because I had two good friends here on the faculty, one, Professor Hildebrand, and then Professor Pitzer. And so they both are big names, and they've been active in the Academy. And so when they nominated me, I had a good wind blowing at my back, and I was elected. I was completely surprised. I did not expect it, but, as I say, I think I was greatly helped by having two people who were well known and nominated me.

06-01:14:00
Burnett: Well, yes, but they proposed your candidacy, and you could have been rejected, had they not found that your accomplishments were—

06-01:14:17
Prausnitz: Yes, I'm sure there was some basis for it, yes.

06-01:14:20
Burnett: Yes, [laughter] right, let's say that.

06-01:14:22
Prausnitz: But there are lots of people, lots of people who have done very good work, but because nobody nominates them they're not elected, although they should be.

06-01:14:33
Burnett: Right. Well, there's something significant, I think, about this, and it's that you are elected to the National Academy of Sciences in 1973, and you were elected to the National Academy of Engineering in 1979, so after, and so—

06-01:14:50
Prausnitz: Yes. It's usually the other way around. Yes.

06-01:14:55
Burnett: So how did that feel? Is that—?

06-01:15:00
Prausnitz: Well, I was very pleased, of course, but totally surprised. In 1973 I was on sabbatical in Germany, and I suddenly get this telegram—we still used telegrams in those days—and heard about this. I couldn't believe it. I was very surprised.

06-01:15:20
Burnett: And to be recognized for the Academy of Science prior to being recognized at the National Academy of Engineering, it is, to some extent, a nod to the scientific work that you were doing, and your connection to the physical sciences, the ways in which you would range over literatures across a number of disciplines to bring ideas into chemical engineering, and I think that that nomination six years before your induction into the Academy of Engineering is emblematic of that.

06-01:16:06
Prausnitz: Yes, I think that's right. I did spend quite a bit of time on what you might call fundamental science, and I used a lot of the ideas of Pitzer and Hildebrand, and so they, I guess, were very pleased to see their ideas developed and used, and so that resulted in my nomination from them.

06-01:16:31
Burnett: Right. And the award that you were conferred by President Bush, can you tell me about that?

06-01:16:40
Prausnitz: Well, that was much later, and this is the National Medal of Science. And I don't really know who was primarily responsible for that. Hildebrand and Pitzer had both died already, so I don't really know who was the chief person there nominating me. But anyway, that was also a total surprise. The award was for 2003, but it wasn't actually awarded until two years later. Why it took two years, I don't understand, but anyway I was invited to the White House, and not only that, they allowed me to bring my family. So my wife came, and my two children came, and they were there in the White House with me, which, of course, was very nice, and I thought, well, now, maybe, after all

this, maybe my children will finally come to respect me a little more, [laughter] but no, it didn't do that at all.

06-01:17:53
Burnett: No. They're as irreverent as ever?

06-01:17:55
Prausnitz: Yeah. I have no complaints. My children have treated me very, very well. But I thought maybe this would bring about a change in their attitude, but no, it didn't. [laughter]

06-01:18:10
Burnett: Can I ask you about—? I don't want to interrupt your story about the conferring of the Medal of Science. And does George Bush, does the President of the United States, actually put the medal on you?

06-01:18:24
Prausnitz: Yes. Now, the way it works is that there were, I guess—I don't know how many; there were six or seven of us who got the award. There's also a Medal of Technology awarded at the same time. And the way it works is that you meet the president in the White House, in one of the many rooms. It's a beautiful place. This is all due to Jackie Kennedy. She really fixed it up, and it's all in the Empire style, so really lovely room, beautiful furnishings and rugs and so forth, mostly antique. So the president comes in and shake hands, hello, and so on, but it was quite clear to me that for a president—this is the second Bush, Bush, Jr.—he was very uncomfortable. I mean, he obviously was not comfortable in the presence of PhD scientists. You could tell he didn't really like it.

06-01:19:31
Burnett: He wasn't cracking jokes and things? [laughs]

06-01:19:33
Prausnitz: No, he wasn't cracking jokes. He saw this as a duty he had to perform, and didn't particularly enjoy it. One of my close friends got the award from President Obama, and it was a completely different story. Obama felt very comfortable with scientists, and joked with them, and was a little familiar with what some of them had done, and a very congenial atmosphere, I'm told, but with President Bush it was pretty stiff. And this was during the war in Iran—

06-01:20:11
Burnett: In Iran, or Iraq.

06-01:20:12
Prausnitz: —Iraq, rather—and, of course, the scientists were all opposed to this. No one brought this up, but I think it was on everybody's mind. So then you go into the East Room, which is one of the official rooms where things happen, and one of his assistants reads the award, reads your name and what you've done, gives a little history about you, and then you step up to the podium there, and President Bush puts the award around, and you have a conversation. The

President says first, "Congratulations," and then you say, "Thank you." That's the conversation you have. [laughter] So that was all very nice, and there was a lunch afterwards, and that evening there was an official dinner. The lunch was at the White House; the dinner was at a nearby hotel, very formal, black tie, but the President didn't show up for either one. He didn't come to the lunch.

06-01:21:25
Burnett: Yeah. Well, it's national recognition. It's not your peers, but it's the nation that recognizes you, right?

06-01:21:31
Prausnitz: Well, yes, it was very nice. I obviously enjoyed it very much, and I managed to go to the restroom in the White House, where there were a lot of paper towels which said "White House" and "President," so I took some home [laughter] as a souvenir.

06-01:21:52
Burnett: Oh, we're going to have to edit this and seal this part of the interview.

06-01:21:56
Prausnitz: I have paper towels from the White House. [laughter]

06-01:22:00
Burnett: All right, your secret is somewhat safe with us.

06-01:22:02
Prausnitz: The secret is out.

06-01:22:06
Burnett: And honorary doctorates?

06-01:22:11
Prausnitz: Yes, I got four honorary doctorates, and that's, of course, very nice. Two are from Italian universities, and they really play it up. They put special gowns and special hats, and it's—

06-01:22:28
Burnett: So you went in person to receive this.

06-01:22:30
Prausnitz: No, no, they give you all that when you get there in Italy. They give you all that. So that was pomp and circumstance, and that was all very nice. One was in Padua, and the first one was in L'Aquila. Most people have never heard of L'Aquila. L'Aquila is a rather small town, about an hour's drive from Rome, north of Rome, and it's in the mountains, so the climate is very good there. It's not as hot as it is in Rome. And there's an old, old university there. The Romans had their baths there. That's why it's called Aquila, water. And only fairly recently does this university have engineering. The engineering there is

not very old, although the university is very old. Can I tell a story about how this came about?

06-01:23:31
Burnett: Yes, certainly.

06-01:23:33
Prausnitz: Let me tell you how this came about. Some years earlier, oh, four or five years before I got this honorary degree, I was reading one of the journals, American journal, and there was an article, a short article, by a professor—what was his name? Vincenzo Brandani was his name. And I looked at this, and it was a nice little article. I don't remember what the subject was, but something in the thermodynamics area, and I thought, oh, this was a nice little piece of work. And I thought a moment about it, and I said, well, this fellow, Brandani, is at the University of L'Aquila. No one's ever heard of the University of L'Aquila. This poor guy's probably all alone. No pays any attention to him probably, no one shows any recognition. So I thought I'd send him a letter, tell him it was a nice piece of work, so on. So I did that. I wrote a letter to him commending him, and so on. And then, at the end of the letter, I wrote, as one usually does in such cases, "I hope someday you'll come visit in California. Please be sure to look me up when you come to the Western United States." And I sent it off, and I completely forgot about it. I just barely remembered.

Well, one day—this was about, oh, must have been four or five months later—there's a knock on the door. And I said, "Come in," and this Italian-looking man with a mustache comes in and he says, "*Io sono Vincenzo Brandani*." His English was limited. It was not terrible; it was limited. Well, it turned out that he had come—never bothered to tell me—to spend a sabbatical here, and he had brought his wife and three sons, and so he sort of said, "Well, here I am; now, take care of me," with no previous warning whatsoever. Well, thanks to the secretary I had, who was very good, we managed to find a place for him to live, and so on, got a desk for him in Gilman Hall, and we started to work together, and his three boys all went to El Cerrito High School, and so it all worked out very well.

And he was here for about a year and a half. And we published some articles together, including one in the proceedings of the National Academy of Sciences. And it worked out very well, and they just loved being here. And so when he had to come home, his boys didn't want to go home, and they made some arrangement for the boys to live with a family that they had befriended here, and they all graduated from El Cerrito High School, and, of course, learned English. And they eventually went back, and then they all became chemical engineers, like their father. It's very interesting. And one of them is in the United States, I think in Houston, and he's the oldest one, and the youngest one is a professor of chemical engineering at one of the universities in London. The second son, he's also a chemical engineer, but I don't know where he is.

Well, as a result of this happy meeting, Brandani arranged for me to receive an honorary degree, and so that's what happened. I was invited to go to L'Aquila, and my wife came with me, and my son came, also, and there were various festivities. They had a show for me at the theater, an old, old theater, rococo style, and, of course, magnificent food. In fact, one of the interesting things was that after the ceremony we went to a restaurant, excellent, excellent dinner, but then, in order to get a really excellent dessert, we had to go to a different restaurant, because the first restaurant, while very good, didn't have a dessert as good as the second restaurant. So we had to walk a block or two to another restaurant, where we got dessert. So, you see, Italians know how to do things. [laughter]

06-01:28:10
Burnett: That's such a sweet story.

06-01:28:11
Prausnitz: And so this was very nice. I've kept up connections with Brandani for many years. He died of emphysema, unfortunately. He was a very severe chain smoker, and that eventually killed him, but for years we kept in contact, and he would occasionally come here to visit. Yes, it is a sweet story, and it teaches you to show recognition. If somebody does a nice piece of work, you should speak up and send them a letter and say "I appreciate what you've done," and so on. I think that's very important, and we don't do enough of that.

06-01:28:56
Burnett: No, we don't. We really don't. So there's been the recognition of your peers. There's been recognition of the nation. There's been recognition of other universities with whom you had presumably some contact, and, in this case, almost random contact. This gesture of generosity brought you into contact with this professor, and it yielded all of these wonderful benefits.

06-01:29:29
Prausnitz: Well, the one from Padua came about by students. There's a professor at the University of Padua; his name will occur to me in a moment. Anyway, he would send students here to do research, and there were several who came, and they've all done very well. They were very good students, and we worked very nicely together. So, as a result of that connection, I received the honorary degree from Padua. Can I tell a little story about that, too?

06-01:30:04
Burnett: Of course, yes.

06-01:30:07
Prausnitz: One of the places where Galileo taught was the University of Padua. It's one of the oldest universities in the world, Padua. And so I was shown around, and I got to a room where there was sort of a pedestal and a lectern, made of very old wood, and I was told that this had been used by Galileo. He had used this lectern in his lectures. And my wife was with me. We were looking around, and she wanted to take photographs, which, of course, she did. And then we

saw a sign which said "No photography allowed" or something like that, so we both froze up a little, and Susie put the camera away quickly. But the professor who'd invited—Bertucco was his name, Alberto Bertucco—and he looked at us and smiled, and he said, "Well," he said, "that sign there about photography, that's only a suggestion." [laughter] I just loved that remark. That's the Italian way of living. Susie and I are both from Germany, and we're sort of Prussian, and the idea of disobeying is very disconcerting. "It's only a suggestion, don't take it so hard." So that was a very good lesson in life.

06-01:31:45
Burnett: Absolutely, absolutely. Well, I wanted to circle back to talking about family, and you have a daughter and a son. That's right.

06-01:31:59
Prausnitz: Yes.

06-01:31:59
Burnett: Okay. and I'm wondering if you could talk about Susie's role in your life, and in your career.

06-01:32:10
Prausnitz: Well, she was tremendously helpful—still is—in taking care of things at home. She loves to garden, which I don't love to do, so she takes care of all that, and, of course, she takes care of the house and the shopping and the meals and all that. She doesn't really do that much anymore; she's now eighty-five, so she can't do as much as she used to, so we have more help. We have a gardener, and we have housecleaning help. But in her younger years, she was absolutely essential, and was very encouraging. And now she still helps me a lot by driving. When I turned ninety I had to give up driving. My eyesight just wasn't good enough anymore. So she drives. She's a very good driver, and her eyesight is fine, so she drives me around. So she's a big help in that sense. And my daughter is a research supervisor at Kaiser in Oakland. Kaiser hospitals do a lot of medical research, and one of their research areas is how to get new results from research to the patient—I thought that was called translation, translational medicine—and how to inform the doctor about what's new on the front, and what medications have worked, what medications haven't worked. So that's a big research area at Kaiser, and she is a manager of one of these research projects. She loves it. She enjoys doing that kind of work very much. She lives in Oakland, and her husband is a science teacher in high school, and so she lives not far away from the Kaiser headquarters, and there's a parking lot there, so she's quite happy doing that. And my son lives in Atlanta. He's a Regents' Professor at the Georgia Institute of Technology, Georgia Tech. And he likes being there. They've treated him very well. He works on drug delivery problems, and he's come up with some new techniques for bringing drugs to where they're supposed to be.

06-01:34:54
Burnett: Right. That's a massive chemical engineering problem, isn't it? Effectively.

06-01:34:59
Prausnitz: It is a chemical engineering problem, that's right, and he has developed a patch with microneedles, so that's a very nice way of delivering medicine because you hardly feel it. There are many people who don't like needles, who are very scared of getting injections, and he has this patch. The patch has medicine in it, and it has microneedles, which are big enough to penetrate the outer layer of the skin, and so you can get big molecules into your system, which, without these needles, you couldn't get that. So he's been quite successful using microneedle and other techniques. He's been working now on drug delivery to the back of the eye. This is for macular degeneration. The way they do that now is the doctor has to take a needle right into the eye, which is not—yeah, exactly; it's not very pleasant. [laughter] And it has to be done very carefully, obviously, lest you do some damage. Well, he's found a new way of a little device at the edge of the eye for doing this, and he does that at the medical school at Emory University. Georgia Tech does not have a medical school, but Emory does, so he's an adjunct professor there. And he's gotten a lot of support, including support from Bill Gates, about this research.

06-01:36:36
Burnett: So the Gates Foundation.

06-01:36:37
Prausnitz: The Gates Foundation. He's met Bill Gates, and hopefully impressed him. I don't know whether or not he—

06-01:36:44
Burnett: He must have. [laughter]

06-01:36:46
Prausnitz: But, no, he's been very, very successful. And his wife is a physician, and she does research at the Centers for Disease Control in Atlanta. And they have three children, and, of course, we don't get to see them very often, but the oldest one just entered Stanford. She's now a freshman at Stanford, so at least we have one of the grandchildren nearby.

06-01:37:19
Burnett: Wow, that's wonderful.

06-01:37:20
Prausnitz: Yes, it's worked out very, very well. I am very grateful. Everything has worked out nicely.

06-01:37:28
Burnett: I want to read back to you something. You delivered a paper at a conference in 2007 in Crete, of all places, on properties in phase equilibria for product and process design, and it was published in *Fluid Phase Equilibria*. So I just want to read from this, if I may. "For the material and economic needs of mankind, the task of chemical engineers is not only to advance knowledge (Athena) for new or better chemical technology, but also to apply that knowledge to new or better products and processes (Hercules). Toward those

ends, some useful tools are provided by classical and statistical thermodynamics, quantum mechanics, molecular simulations, and, perhaps most important, by utilizing suitable physicochemical properties revealed by chemistry. However, in the postmodern world, Athena and Hercules are not sufficient. Social and political trends clearly indicate that chemical engineers must also give increasing attention to cultural needs, not only to such well-known requirements as safety, sustainability, and environmental protection, but also to sensitive awareness of a variety of human factors, such as cultural heritage, diversity in the workforce, special needs of women and families, and continuity between what we do at work and what we do at leisure (Nausicaa). In the twenty-first century, the public we serve demands integration of chemical technology with those humanities that promote a more just society and a more meaningful life."

06-01:39:12
Prausnitz: I feel it sounds like a sermon. [laughter] Yes, that summarizes very well my general feeling about things, and those characters from Greek mythology illustrate it. It's taken out of context in the sense that I, at first, talk about the Greek mythology, which most people don't know. So I talked about Athena, the goddess of knowledge; and I talked about Hercules, and his famous story about cleaning the stables; and then Nausicaa, who helped Odysseus when he was washed up on the beach, and Nausicaa nurtured him, nursed him back to health so he could return to his home. Most people don't know these stories, so I first talked about the Greek mythology, and showed how they are symbolic of our lives today. I love Greek mythology. It has everything in it you could possibly want. The Greek myths really illustrate life beautifully. So I discussed that, and then came to this paragraph that you read. Yeah, I think that paragraph sums up very nicely what I believe in.

06-01:40:43
Burnett: So you have excelled in these highly specialized realms, but in part, in perhaps large part, your success is due to your wider consideration of the meaning of that highly specialized work, the context of that highly specialized work, and the purpose of that highly specialized work, and I think that—

06-01:41:08
Prausnitz: Yes, a lot of people don't think about these things. I have all too often encountered, especially in Europe, the following experience: I would be shown a laboratory in the university, and it would be graduate students doing research, and I would ask the student, "Now, you're doing such-and-such; that's very nice; you're measuring this and that; but why are you doing that?" And all too often I got the answer, "Because my professor told me to do it." And that's a terrible answer. [laughs] And yet, it happens, I think probably less so now than ten or twenty years ago, when I had these experiences. But people just are blinded, of course, with these blinders on the eyes. People don't ask themselves, "Well, now, what is all this about? How does this fit in, and why should anybody care about this? Where is it going, and what's it got to do with anything else?" People don't ask those questions.

06-01:42:18
Burnett: Well, the motto of the Royal Society of London was "*Nullius in verba*," and that was a rejection of the scholastic approach where you just get the received wisdom from the established scholarship, and you maybe scribble a little bit in the margins, and that's your contribution, but, rather, don't take the word alone. You need to go out and see for yourself. And I think that that's something that you've done in your career is that you have ranged widely both over existing scholarship across multiple fields, and also across many experimental fields, as well, and you've integrated them, and that's been your project.

06-01:43:02
Prausnitz: Absolutely, and I preach it wherever I get the chance. [laughs]

06-01:43:08
Burnett: Dr. Prausnitz, I want to thank you for taking the time to talk with us.

06-01:43:11
Prausnitz: Good. I'm very glad that you asked me leading questions. [laughter]

[End of Interview]