



PETR VOGEL
(Born 1937)

INTERVIEWED BY
SHIRLEY K. COHEN

December 16, 2002 and January 16,
2003

Photo courtesy CIT Public Relations

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Subject area

Nuclear physics

Abstract

An interview in two sessions, December 2002 and January 2003, with Petr Vogel, Senior Research Associate in Physics, Emeritus in the Division of Physics, Mathematics and Astronomy. Born and raised in Prague, Vogel recalls his family's experiences in the concentration camp in Theresienstadt, his post-World War II experiences growing up under Communism; college preparation in the LaGuardia Gymnasium; transfer from Charles University to the Prague Institute of Technology; post-graduate work at the Nuclear Research Institute of the Academy of Sciences near Prague in the early sixties; contacts with scientists from Russia (Vadim Soloviev) and from the West at a summer school in 1962 in Slovakia and transfer to the Joint Institute of Nuclear Research in Dubna (USSR), where Vogel finished his graduate studies (1966) and returned to his job at the Prague Institute. He recalls the Prague Spring of 1968 and its collapse; postdoctoral work at the Niels Bohr Institute in Copenhagen and his interactions there with Kai Neergaard (1968-70), working on pure nuclear structure problems; and his arrival at Caltech in 1970 to work with Felix Boehm. Vogel also began collaborating with Aage Winther on the problems of mu-mesic atoms; and also

with Steven Koonin and Brian Davis having to do with work done in Boehm's group on some effects that looked like time reversal. He comments on the interest in neutrino physics at the end of the seventies by Fred Reines's (at Irvine) and Boehm's group; theoretical calculations on the neutrino spectrum at nuclear reactors that led to important experimental work in the eighties; Reines's claim that he had discovered neutrino oscillations (and Richard Feynman's efforts to disprove it); and his work with Feynman on the supposed existence of a fifth force, also a wrong claim.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH PETR VOGEL

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Petr Vogel
Pasadena, California

by Shirley K. Cohen

Session 1 December 16, 2002

Session 2 January 16, 2003

Begin Tape 1, Side 1

COHEN: Welcome, Petr. Perhaps we can start with your telling us a bit about your family.

VOGEL: Well, I was born and grew up in Prague. The country has gone through many names, of course. It was called Czechoslovakia when I was born, just before World War II; now it's the Czech Republic. My father was an engineer. But when I was born, he was a businessman, in plumbing supplies, an installation company, and my mother helped take care of the administration of the business. It wasn't a very big business. Then the war came and everything was confiscated. My father was among the first people to end up in the concentration camp in Theresienstadt. I went there a few months after he did, with my mother. I was four years old. We spent three years there. We survived thanks to the fact that my father was in charge of plumbing there.

COHEN: Did you live there as a family, or were you separated?

VOGEL: We were privileged, because my father had the number 18 in our unit. There was a very careful administration there, and we actually had a small room for ourselves, which was very rare. Two of my cousins slept with us, because it was nicer. They both perished there, unfortunately. And when I came back, when the war ended in '45—

COHEN: Let me just ask you a question. Did you go to school while you were in the camp?

VOGEL: No. It was forbidden to have any education. But a few of us little boys were schooled by somebody. So I started in the third grade in '45, when we moved back to Prague. I had missed the first two grades, but I could read and write, so I didn't miss too much.

COHEN: Did your father go back into his plumbing business?

VOGEL: No, that was finished. Everything was different. It was a socialist country now, and my father was a bureaucrat, I guess.

COHEN: But he was doing the work he knew how to do?

VOGEL: That's right. He was first working in some office that had to do with the water supply and then more generally in construction. He was in the ministry of construction, or whatever it was called. My mother worked as well, and I went to school, which was organized differently from the way it is here. Everybody went to the same school until ninth grade, and then, at the age of about fifteen, there was a separation. A relatively few people went into what is called a *Gymnasium* in Europe, college preparation—only, I'd say, ten percent or so.

COHEN: Now, was this different because the Communists were running the system?

VOGEL: It was different in the sense that in older times the separation came after the fifth grade and then came eight years of *Gymnasium*. The democratic reform was that everybody now went to the same school for nine years, until age fifteen, and then came the separation, either into college preparation, or various trade schools, or directly into learning some trade—like becoming a mechanic, or whatever.

COHEN: Was the path determined by an examination?

VOGEL: Well, we had some exit examination, but I think the decision was based on grades and things like that.

COHEN: Did the government interfere at all with this? You know, we hear stories of how the big

shots' kids could go—

VOGEL: Not so much at that stage. But if you had a so-called bad origin, then you did have trouble already at this stage. Later, in the university, it was really taken into account whether your parents were rich or not. That mattered.

COHEN: How about the politics? I assume everybody's a Communist at this point.

VOGEL: Well, yes. Nobody was openly anti-Communist. [Laughter] That wasn't the criterion; the criterion was more what they called origins.

COHEN: I see. That smacks of capitalism more than politics.

VOGEL: For me it would have been bad, but my parents actually were very left-oriented, as many intellectuals were in the thirties. So from that point of view, I didn't have any problems. Both were members of the Party. After all, it was the Red Army that saved our lives. My brother is named Joseph, after Stalin, actually. [Laughter] In the *Gymnasium*, because it was selective, we had an excellent teacher in the first year, in mathematics—a guy, I guess, who should have been at the university, but he had some political problems. He was very enthusiastic. He was probably a bad teacher, in the sense that most people got lost. But a few of us got really interested in the way he was enthusiastic about geometry.

COHEN: He inspired you.

VOGEL: I think so—yes, very much so.

COHEN: Do you remember his name?

VOGEL: Ressler. Prague is a small city. I knew his son, and so on; we knew each other. Everybody knew everybody. Our school was called LaGuardia Gymnasium.

COHEN: LaGuardia?

VOGEL: LaGuardia—the mayor of New York.

COHEN: How did it get that name?

VOGEL: I do not know. I actually do not know why.

COHEN: I mean, he's hardly a Czech. [Laughter]

VOGEL: No, no—and even in the worst Communist time, it was called that. The teachers were mostly older people, who didn't push much politics on us. Then after high school, there was the question.... Of course, there was almost *no* question of whether you would go to the university or not.

COHEN: If you were in this *Gymnasium*, you went to the university?

VOGEL: If you were there, it was assumed that you would go. A few people, of course, left—but generally you went to the university. And these schools were much smaller than the schools here. We were two groups, of fourteen each. So that was nice, because we still keep in touch. We meet regularly whenever I go to Prague.

COHEN: Your schoolmates from all those years ago. Are most of them still in Prague?

VOGEL: Yes. Three of us, I think, are outside the country, but the rest are in Prague.

So there was the question of where to go to university, and for me there was no hesitation. Again, the university is different from universities in this country. You would start in the first year to study some subject. The faculty of mathematics and physics was the obvious choice for me. There was an entrance exam, and I was accepted.

COHEN: That was in Prague?

VOGEL: That was in Prague—Charles University. In my first year, I wanted to study physics. I didn't feel that I wanted to do mathematics. I didn't think I was good enough. My father was a

very practical man, and I felt that pure mathematics was too far from the real world. But that was a time when people talked about nuclear power a lot, and they started a special division—nuclear physics, nuclear engineering, and chemistry, those three groups. I was transferred into that—transferred from Charles University to the Prague Institute of Technology. So from the second year on, we were students of the Institute of Technology, not of the university. This was a new thing, with new buildings, new faculty. Several people from our class were invited to study in Russia, but for that, you really had to be very politically pure; I wasn't, so I studied in Prague.

COHEN: Were any Jewish people asked?

VOGEL: I don't think there were many other Jewish people. The Jewish community of Prague—

COHEN: It was decimated.

VOGEL: Very few survived the war. Then after the war, many left the country, either for Israel or somewhere else. Particularly in '48, when the Communists took over, there was a moment when you could leave. So there were very few Jewish people left—which is the case still.

I finished what you would call a master's, I guess, in Prague. And then at the end of that, the standard practice was that the Institute of Technology found you a job. You had some choice, but it was rather limited; you had to take what was available. And at that time they started what here would be called graduate studies—that is, studies for a higher degree, with fellowships and so on. And I got one of those in a research institute—the Nuclear Research Institute of the Academy of Sciences, which is near Prague. So I went there, but there was not so much actual physics done there. This was in the early sixties. There was a little lessening of tension, and the head of our group organized a summer school in '62, in Slovakia, where various people, both from Russia and from the West, came and lectured. And this fellow named [Vadim] Soloviev came from Dubna, in Russia, which was a joint institute like CERN [the European Organization for Nuclear Research], except it was for Eastern Europe. He lectured at that summer school, and we talked, and he invited me to Dubna, so that's where I went to finish my graduate studies.

COHEN: How many years were you in Dubna?

VOGEL: Well, it was on-and-off. I went there at the end of '62, the last few days of '62, I think. It was a small group then, but relatively active. Soloviev spent some time in Copenhagen with the younger Bohr and had good contacts. Copenhagen was at that time the center of nuclear physics. And I had some advantage, because Russians couldn't travel at all, but I had a Czech passport, and somehow it was easier for me to go. So almost right away there was a big nuclear physics conference in Paris, and I went there with Soloviev and met Aage Bohr, and he invited me to Copenhagen. And so Dubna, I think—I don't know who paid for the Copenhagen visit, but I was able to go there for three months.

COHEN: While you were still a student?

VOGEL: While I was still a graduate student. But I was also writing papers. I was a little older than the average graduate student here. I spent three months in Copenhagen, and then I defended my thesis—that was in '66. By that time I had become sort of a star member in Dubna.

COHEN: Where is Dubna?

VOGEL: It's not far from Moscow—two hours by train from Moscow. It's a rather remote place—a small town in the middle of a forest, deliberately built that way. But once I had my degree, I was spending roughly half my time back in Prague, because I got married. [Laughter]

COHEN: Did you have a job, or a position, in Prague also?

VOGEL: Yes. I had a position reserved the whole time, at the same Institute of Technology.

COHEN: That was automatic, upon graduating?

VOGEL: That was, yes.

COHEN: Well, that's good. You could concentrate on your work. You didn't have to worry

about a job.

VOGEL: No, no, there was never a problem with that at that time. And actually we were, by Czech standards, reasonably well paid. We also had some privileges. We got an apartment in Prague.

COHEN: Not too much equality there, huh?

VOGEL: No, no, no. Compared with other people, we were quite well off. And then came the Prague Spring of 1968, when suddenly the borders opened. Even so, I was abroad around that time: I was at a conference in Paris; I was in Copenhagen; I was at some other conference. Once, I was supposed to go to the US to a Gordon Conference with Soloviev, and I had my passport, I had my ticket, I had dollars. But the day before we were supposed to leave, Soloviev was told that he could not go. When it looked like I would be going alone, they said, “*Nyet.*” So I didn’t go, either.

Anyway, in ’68, when it was easy to leave the country, I wrote a letter to Aage Bohr and asked if there were any jobs, and he invited me for a postdoc position in Copenhagen.

COHEN: What language did you speak when you traveled and talked to people? I mean, in Copenhagen you’d speak—

VOGEL: English.

COHEN: So English was not a problem?

VOGEL: No.

COHEN: You learned English very early?

VOGEL: Well, in the *Gymnasium* we had a choice of German, English, or French, and I took German. I do not remember—

COHEN: When you learned English? You don't remember?

VOGEL: At the university we had to take English classes and we had to have some exams. Of course, in Dubna we spoke Russian, and we published in Russian, mostly. But I—

COHEN: You don't remember when you learned English.

VOGEL: No. [Laughter]

COHEN: But whenever you were abroad, you spoke English?

VOGEL: Certainly. I've forgotten most of my German. I had a really hard time.

COHEN: Was your family Czech-speaking or German-speaking?

VOGEL: They were Czech-speaking, certainly after the war, [but they] spoke German fluently. And my grandmother, I think, preferred German. But after the war, nobody spoke German.

COHEN: When you traveled in scientific circles, it was English?

VOGEL: It was English, always.

So in '68, when the Prague Spring collapsed, I had already had a job offer from Copenhagen, so we simply went there—the family, with Martina, our daughter. And Vendula [Mrs. Vogel] was pregnant, so we drove to Copenhagen, and that's where Klara was born, that year.

COHEN: You had no trouble leaving Czechoslovakia?

VOGEL: That was just after the Soviet occupation, and there was chaos.

COHEN: So nobody knew what anybody was doing?

VOGEL: Well, the direct route to Copenhagen was straight north, through East Germany, and we

were told not to do that. So we drove actually south, to Austria, and then sort of around. And we had just enough money for gas and the ferry—you have to take a ferry—so we ended up in Copenhagen with no money at all.

COHEN: But you had a job.

VOGEL: I had a job.

COHEN: As a postdoc.

VOGEL: Yes. We were officially on leave, and in principle we could go back. Actually, in the summer of '69, we did go back to Czechoslovakia to visit family and show the baby to the grandparents, and we were able to leave again, but we were told that we should come back. That was the second year I was there. Sort of halfway through '69, we were told that we should come back, but it was obvious that they didn't think we would.

So by the time we decided not to go back, I wrote a few letters on the advice of Bohr and Mottelson to various places. And Felix Boehm [Valentine Professor of Physics, emeritus] was one of the people I wrote to, and he offered me a job here at Caltech. So that's where we went.

COHEN: You were how many years in Copenhagen?

VOGEL: Two.

COHEN: And then I have something about Norway here, too.

VOGEL: Yes. Well, the second year in Copenhagen was complicated. The first year in Copenhagen I was on a postdoc fellowship, which paid incredibly little. And, you know, we had two children and so on. But in Copenhagen there was an organization called NORDITA [Nordic Institute for Theoretical Physics], which is a joint organization of Nordic countries—Norway, Sweden, Finland, Denmark, and Iceland—with headquarters in Copenhagen, at the Niels Bohr Institute. They offered me a job in Bergen, Norway, which paid much more, with the proviso that I could fly to Copenhagen for a month every three months or so.

COHEN: So it was a joint appointment.

VOGEL: It was sort of a joint appointment. I had some teaching duties in Bergen, but my active work was still in Copenhagen.

COHEN: Who were you working with in Copenhagen?

VOGEL: Well, there was a young, fresh PhD named Kai Neergaard. We worked together. We still keep in touch. The people who ran the place were Aage Bohr and Ben Mottelson—who got the Nobel Prize, together with our president, David Baltimore, at the same ceremony. We have a picture of them standing together. [Laughter]

So then in 1970 we flew from Bergen to California. Even so, I had asked for an extension of my official Czech passport, but it wasn't granted and I was fired from my job. Our apartment in Prague was confiscated, and we were sentenced—

COHEN: Is that because you didn't go back?

VOGEL: Yes. And I was told that we were sentenced *in absentia* for leaving the country. Well, that was, I guess, formally necessary so that they could confiscate our stuff. But there was some treaty between the US and Czechoslovakia that was still valid—a treaty from 1910, or something—and based on that treaty, my sentence was waived.

COHEN: You mean they actually would have put you in prison there, if you went back?

VOGEL: That was the idea—that they could, for three and a half years. But once I became an American citizen, I wrote to the Czech Embassy in Washington and told them that I was now an American citizen. I paid seventy bucks or something, and they sent me a piece of paper saying that the crime I committed by leaving the country was erased.

COHEN: OK. But they didn't give you back your apartment in Prague.

VOGEL: No, no, of course. Nothing, no.

COHEN: Your parents were there all this time?

VOGEL: Yes, that's right.

COHEN: None of this was put on them? They were not bothered, or inconvenienced, by this, were they?

VOGEL: I guess they were to some extent, but I don't know. Well, they were by that time retired, so it didn't—

COHEN: It didn't affect them.

VOGEL: Yes, and in fact they visited us here several times, and they were encouraged to do so, because the authorities hoped they would not come back, so the government would save on their pension. [Laughter]

But after we became American citizens, we needed a visa to go back. And since my parents were already old, and traveling here is not so trivial, we wanted to visit them, but we needed a visa, and we didn't get it. We were told that getting a visa is a privilege, not automatic. And because it is a privilege, it is not the duty of the embassy to explain to us why we would not get the visa. So for a long time we were not able to visit.

COHEN: When did that all stop?

VOGEL: Well, it's a complicated story. I was able to contact somebody in Washington, Senator Howard Baker. I wrote to him about how this violated the spirit of the Helsinki Accords—that I couldn't be with my family. And I got an answer from some secretary saying they were not going to fight that battle, but if I personally needed to go there, they would arrange it. And then Senator Baker very graciously wrote to the American ambassador in Prague, who pulled some strings—called in some favors. And from that time on—

COHEN: You got a visa.

VOGEL: We got a visa every time we wanted. And we were told that this was purely—

COHEN: A political “Thank you.”

VOGEL: Yes, some private arrangement. That’s how it worked.

COHEN: OK. So you came to Caltech in 1970. You arrived in Pasadena as a postdoctoral fellow.

VOGEL: That’s right.

COHEN: And Felix Boehm already had his group?

VOGEL: That’s right. It doesn’t exist anymore, but it was quite large at that time. [Rudolf] Mössbauer was gone by then—the group had been blindsided by that.

COHEN: Pasadena must have seemed very new to you.

VOGEL: I knew various people here. There was a Czech fellow here who went to university with me—another Jewish fellow, Peter Herczeg, who is now at Los Alamos. He was a postdoc here, and his wife met us at the airport when we flew in.

COHEN: It’s nice to see somebody you know.

VOGEL: Yes. So it wasn’t that we didn’t know anybody at all.

COHEN: So you established yourself here in 1970. You knew you were going to stay in the United States.

VOGEL: Well, yes. I had some offers from Germany, but I didn’t want to go there. First of all, I wasn’t so enthusiastic about that, but I also felt that my parents wouldn’t like it.

COHEN: So how did you find Caltech in 1970, when you came?

VOGEL: It was, of course, different from my experience in Copenhagen. Copenhagen was nice because at that time it was really a very lively place in my field. There were many people, many visitors.

COHEN: Anybody doing physics came to Copenhagen?

VOGEL: Nuclear structure physics. And here it was slightly different. People were doing various things. There were, of course, famous people here. I switched a little bit here, in the early days. That was a time when Los Alamos built a new accelerator called LAMPF [Los Alamos Meson Physics Facility], a medium-energy facility. And Felix, also, was interested in atomic problems between nuclear physics and atomic physics. So I started to work on that. Also at Los Alamos were problems called mesic atoms, where you replaced an electron by a *mu* meson or a *pi* meson. There was quite a bit of experimental activity. I worked on theoretical problems like that.

COHEN: What was Boehm's group like when you first came here?

VOGEL: Well, as I said, it was an experimental group. There were several serious visitors—postdocs, like myself.

COHEN: You were doing theoretical calculations? You were not setting up experiments, or anything?

VOGEL: No, no, but I was always in close contact with the experimental activity, though. For example, that goes back to Jesse DuMond, who in the late 1940s had invented a device called a bent-crystal spectrometer, which could measure very accurately energies of gamma lines. And in that context, Felix had a program with several graduate students and several TAs [teaching assistants] to measure something called isotope shift—that is, a change of wavelengths of X rays when you go from one isotope to another of the same element, and that tells you something about the size of the nucleus. But it's not totally trivial to translate this X-ray energy into information on the nuclear size. I worked on that part of it.

The other similar technique was used to see.... It turns out that the X-ray energy also depends on the chemical compound your element is in. And again, the interpretation of what that means. I worked on those. And then came the problems, when the LAMPF facility started, to see what are the properties of atoms when you replace one of the electrons with a much heavier particle, like a *mu* or *pi* meson. That tells you, again, something about the size of the nucleus.

COHEN: Now, this was before the days of the big machines, wasn't it?

VOGEL: Well, this LAMPF machine was a reasonably big machine; it wasn't very high energy, but it was very high intensity. [Tape ends]

Begin Tape 1, Side 2

VOGEL: The bent-crystal work was done in the basement at Caltech. That was almost the last of the in-house things that were done here in Boehm's group, essentially.

COHEN: You were here during the shift from local to big physics.

VOGEL: Yes, except from the point of view of a theorist, I didn't have to spend time in Los Alamos. I went there occasionally, to present a proposal and things like that.

COHEN: But you weren't working on the actual experiments.

VOGEL: No. And I still had some contact with Kai Neergaard, with whom I had worked in Copenhagen. He came a few times here. So we worked still on these pure nuclear structure problems that we had in Copenhagen, and I had contact with people from Berkeley and other places.

COHEN: By this time you were no longer a postdoctoral fellow here; you were working for the research staff.

VOGEL: I was what is called a senior researcher, which is paid the same. So I still worked on

pure nuclear structure also, writing papers on that and going to meetings. In the first few years, I spent several summers back in Copenhagen.

COHEN: We're talking about the middle seventies now?

VOGEL: That's right. There was a fellow named Aage Winther, who used to come to Kellogg [W. K. Kellogg Radiation Laboratory] regularly and who was a director at that time of the Niels Bohr Institute. And we worked together here on the problems of mu-mesic atoms. So I spent several summers in Copenhagen working with him on these things.

COHEN: You were in your own nuclear physics group in physics. Did you have much to do with the rest of physics?

VOGEL: Well, I had papers together with people from Kellogg. There was a fellow named Peter Haff, who worked later with [Thomas A.] Tombrello [chairman, Division of Physics, Mathematics, and Astronomy] and is now at Duke. And with Aage Winther we had several papers. So that was a joint project. In the late seventies I worked also with Steve [Steven E.] Koonin [professor of theoretical physics]. That was, again, related to work done in Felix's group on looking for time reversal. And there were some effects that looked like time reversal but were not. Steve had a student, Brian Davis, whom I encouraged to work on that, and the three of us then wrote two papers which were quite, I think, influential. [Davis, B. R., Koonin, S. E., and Vogel, P., "Atomic Screening of Nuclear Transitions," *Phys. Rev. C* 22 (3), 1233-1244 (1980) and Davis, B. R., Koonin, S. E., and Vogel, P., "Atomic Interference Effects in Nuclear Transitions," *Bull. Am. Phys. Soc.* 25 (4), 502 (1980)]

At the end of the seventies came the interest in neutrino physics. And Felix, encouraged by Murray Gell-Mann, together with Mössbauer set up an experiment at a reactor in Grenoble in 1979. At that time, everybody knew that nuclear reactors made neutrinos, but it wasn't quite clear exactly how many and at what energy. There was also a big interest in that at Irvine. Fred Reines, who later got the Nobel Prize for that, was working on this problem. So we took it up. With Brian Davis, we looked into that problem and found that most papers written on the subject were not quite right. Brian's thesis ["Studies of antineutrinos at nuclear reactors and atomic final state effects in tests of T-violation" (1980)] was on calculating the spectrum of the reactor. And

that was a very important piece, because we found that in fact it was forty percent fewer neutrinos than people had thought before.

COHEN: Had anybody seen them at that time?

VOGEL: Yes, Reines did.

COHEN: In the machine in Grenoble?

VOGEL: No, no. He saw it at the Savannah River plant, which is in South Carolina. This was a reactor run by the military; I think it's now closed. There were several reactors. And so this calculation of the neutrino spectrum of the reactor was done also with the help of some people from Idaho Falls and Hanford, where they had reactors. They had a big database. Those papers became sort of famous [B. R. Davis et al., *Phys. Rev. C* 19 2259 (1979); P. Vogel et al., *Phys. Rev. C* 24 1543 (1981)]. They led later to some experimental work in which these things were verified. So from the eighties our theoretical calculations were superseded by the actual measurement.

That was and still is a very influential piece of work. It got me into contact with people in Europe and with people in Irvine.

COHEN: So you then started to do a good bit of traveling.

VOGEL: Well, yes, maybe a little more than I did before. We always liked to go to Europe, so—

COHEN: There's quite a list of these places where you taught—summer school things.

VOGEL: There was some sort of a summer course in Lausanne. One of our former collaborators, Jean-Luc Vuilleumier, is now a professor in Neuchatel, Switzerland, and he invited me to do that. That was a month or several months.

Actually a nice—well, I don't know whether it was so nice at that time, in the early eighties—there came a claim from Fred Reines that he had discovered neutrino oscillations, which was partially based on using this wrong spectrum. But there were some other problems

with that. And Dick [Richard P.] Feynman was interested in that, and we talked about it. We often met in the cafeteria for lunch. I explained to him what was going on, and he became convinced that this claim by Reines was incorrect. He enjoyed being a detective, so we worked on trying to explain what went wrong, and we thought we discovered what went wrong. We wrote a little paper about it. It was a really nice thing to work with Dick. But then when it was almost finished, Dick said, “You know, Reines is my friend. Let’s not publish it.”

COHEN: That’s not very scientifically honest. [Laughter]

VOGEL: No. Well, he said, “Let’s send it as a letter to a few people and let the Xerox machines do their job.”

COHEN: That’s not such a good friend. [Laughter]

VOGEL: Well, but he didn’t want to be openly against it. And indeed a few years later Reines withdrew that claim and explained what went wrong.

COHEN: So Reines was aware of all this work.

VOGEL: Oh, yes, yes, yes. He actually supplied us with detailed information.

COHEN: Ah, so he was interested to see if something was wrong there, too?

VOGEL: Well, obviously in physics you cannot deceive anybody for long. I’m sure it wasn’t deliberate at all. He underestimated some efficiency, and other things, I guess, went wrong. These things are nontrivial, no doubt about it. And Reines was a person who was very enthusiastic about things. He got very excited because he thought he had discovered something really fundamental, and then it was sort of difficult to let go.

COHEN: You said you enjoyed working with Feynman.

VOGEL: Yes. I did on two occasions. We talked—not regularly but quite often. There was

another one; again, a wrong claim, something called a fifth force, but it died by itself.

COHEN: Fifth force?

VOGEL: Fifth force. Dick was already quite sick by then. Actually what impressed me then—part of the claim was based on some work done by Baron Eötvös in 1912. He published in German, of course. The claim was that the gravity is not the same on all materials—that it depends on which nuclei you have. Eötvös studied gravity and measured gravity of different woods and different metals and so on. Eötvös concluded that there was no such effect, but he didn't know anything about nuclear properties. The claim made later was that when you arrange the Eötvös experiment according to nuclear binding energy, you saw some trend. And what impressed me about Feynman then was that he asked [physics departmental secretary] Helen Tuck to bring him the original papers in German. I had had German in school; my parents spoke German. But his German was better than mine. [Laughter]

COHEN: So you read the papers in the original?

VOGEL: That's right.

COHEN: What did you discover?

VOGEL: Well, that the fifth force doesn't exist. And it doesn't.

COHEN: Did you publish anything on this?

VOGEL: No, no. Other people did that. I published another piece of work, with somebody else, on a different aspect of it—that it cannot be true. But many people did that, and the thing died.

COHEN: So now you're doing some different kind of work.

VOGEL: Looking for neutrino oscillations and various aspects of neutrinos, the search for properties of neutrinos—that goes on. The last big experiment in which Felix was involved was

in the Palo Verde reactor, near Phoenix. That experiment was finished two or three years ago. We worked on it together with people from Stanford and other places. Then the next step was to make a much larger experiment of the same kind. And this is going on now in Japan at Kamiokande.

COHEN: Is that what you were doing in Japan this last year?

VOGEL: That's right. And we just submitted a paper in that regard, which is a very important one. [Eguchi, K., et al., "First Results from KamLAND: Evidence for Reactor Antineutrino Disappearance," *Phys. Rev. Letters* 90 (2), Jan. 17, 2003] That solves a lot of problems. So that's a very exciting thing. And I had this tradition of worrying about the reactor's neutrino spectrum, which measures the neutrinos from all the Japanese reactors, and we had to know this quite well, so that was my contribution.

COHEN: And Felix is out of it now?

VOGEL: Felix is no longer part of it. But our collaborators from Palo Verde are involved, together with other people. This is a big experiment now. Eighty people are on that paper.

COHEN: It's hard to know who does the work when it's a paper with eighty people.

VOGEL: Yes, that's right.

COHEN: Does the paper circulate to all eighty? How does that work?

VOGEL: That's sort of complicated, and slightly painful. There is a publication committee that makes the first few drafts, and then it circulates. Once it starts circulating, everybody wants to change something.

COHEN: So it takes a while before it comes out.

VOGEL: That's right.

COHEN: So you'll continue working on this?

VOGEL: That's right. Well, it's a long project, and I will probably do various other things. My other interest is something called double-beta decay. That was another thing I worked on with people from Kellogg. There was a postdoc there, or a Prize Fellow, named Martin Zirnbauer. In '86 we wrote a paper on double-beta decay. [Vogel, P., and Zirnbauer, M. R., "Suppression of the 2-Neutrino Double-Beta Decay by Nuclear Structure Effects," *Phys. Rev. Letters*, 57 (25), 3148-3151 (1986)]. This decay was at that time poorly understood, and it is still not fully understood. But we found some trick how to calculate the rate, and that became rather important, and people used the technique. I'm still very much interested in that, and I often go and talk about this subject. That's another large area of activity all over the world.

COHEN: OK. I think we'll stop here, Petr, because we've talked a long time.

VOGEL: Yes.

PETR VOGEL**SESSION 2****January 16, 2003****Begin Tape 2, Side 1**

COHEN: Happy New Year, first of all, and welcome. In your career you've been at three major places, Dubna, the Bohr Institute in Denmark, and of course here at Caltech for many years. Would you talk a little bit about the atmosphere or the climate at these different places? Start with Dubna, where you did your PhD work.

VOGEL: Well, that was of course a very different place from the others, because that was during the time when—we all read about that in books and so on—this was a very closely regulated place.

COHEN: This would have been in the 1960s?

VOGEL: Mid-sixties. A very large place. There were several thousand people working there. Even the theoretical physics lab, where I worked, had probably more theorists than any other place in the world. I do not remember the numbers, but maybe a hundred people. Of course I was in the lowest level of the hierarchy, but things were pretty well organized, in that there were groups and group leaders who basically told us what to do, and lots of approval needed for everything.

COHEN: Now, was that usual that they would invite somebody from Czechoslovakia to come and do the degree?

VOGEL: Well, Dubna was an international institution, for countries of Eastern Europe. It was organized in response to the establishment of CERN, in Western Europe. In fact, all Eastern European countries contributed to the budget and had a group of people there. The Soviet Union contributed forty-nine percent of the budget. When I came, there were still Chinese there. They left shortly after.

COHEN: That was for political reasons?

VOGEL: Yes, there was this break with China. There were groups from Czechoslovakia, Poland, East Germany, Bulgaria, and so on. But the leadership there was all Russians.

COHEN: And the decision making.

VOGEL: Yes. Well, there was some scientific committee, which was made up of East European countries, but I am not sure that it had any power. Of course later, for example, if you wanted to submit a paper for publication, you not only had to give a seminar about it, you had to submit the manuscript to the scientific secretary. It went for approval somewhere else, and only then you could publish.

COHEN: Now, was this different from CERN, which you got to know later? I mean, somebody must have overseen that, too. No?

VOGEL: Well, in principle, yes. When I was visiting at CERN, if you wanted to publish a paper with your name and CERN's, somebody had to approve of it, but that was pro forma. In Dubna it was all serious business and there was pressure to publish in Russian, German, and so on. But otherwise it was scientifically, at that time, a reasonably good place. Various famous people were there, and there were visitors. And I had a chance to go abroad from there to the West several times, which was very rare. So I cannot complain about the scientific atmosphere there. The computing facilities were very primitive, which was partly because of the time. We couldn't ourselves run the computer; you had to go through intermediaries. There was some guard at the door, and so on. The programming language was then so primitive. The codes were written in machine code, which we didn't know how to do. So there was a large group of women who would transcribe whatever your formulas were into this machine language and then punch cards. As theorists, we could have a briefcase, but other people couldn't bring anything into the lab. There was a guard. You had to not only have a badge, but carry nothing—

COHEN: No pockets. [Laughter]

VOGEL: Well, pockets were not searched. As theorists we were allowed briefcases, which in principle they could search.

COHEN: So it was very regulated.

VOGEL: Very regulated.

COHEN: But that was just the Russian way.

VOGEL: That was the Russian way. Even so, it was—you know, no military work was done there, that was basic research. And the whole place was surrounded by barbed wire, with sand which was smoothed so that they could see footprints, and so on.

COHEN: And you were there for how many years?

VOGEL: I was there on and off. Let's see. In the later years I would spend a few months there and a few months back home.

COHEN: You were married at this time and Vendula remained in Prague?

VOGEL: Yes. She came for visits, but basically I went back there. So I was in Dubna on and off a little more than three years.

COHEN: At which point you defended a thesis.

VOGEL: That's right. There the procedure is very formal. Again, you have to write a thesis. Then you have two opponents who are distinguished people and have to be from different institutions. So the lab flew in Professor A. S. Davydov, who was a rather famous guy from Kiev, who was my official opponent. He was supposed to—

COHEN: And he was really called an opponent?

VOGEL: He was called an opponent. He was supposed to ask unfriendly questions.

COHEN: That means he had to read your thesis carefully.

VOGEL: That's right. He had to write a report. So [there were] two such people. The second one didn't have to be a full professor. I do not remember who that was. And then there was a secret ballot of the scientific committee of the lab, on whether to grant the degree or not.

COHEN: So if somebody didn't like you, that was too bad.

VOGEL: Or if they didn't like my supervisor, which was more likely. Nobody cared about a young guy like myself, but my supervisor was somebody who was on a higher level and he was actually the party secretary. I think I wasn't unanimously voted. It was OK, but I think some people had some quarrels with my supervisor. And then the whole thing goes to Moscow, to some other committee, and only when they approve it do you get your official degree.

COHEN: How long did this process take?

VOGEL: Several months. It was all right. Here there is only the PhD, but there they had a lower degree, which was called candidate of science and which was basically equivalent to a PhD—actually, I think, a little tougher, but more or less. And then there was a higher degree called doctor, which was on the level of a full professor.

COHEN: So you immediately would have your degree and be a full professor someplace?

VOGEL: No, because the science was organized more in research institutes. At the academy there were no teachers, but the people there, the staff members, had different ranks. And if somebody had this doctor of science degree, that was considered equivalent to being a full professor at the university. So the committee had in principle the choice to grant that to the person right away, even if he was defending only a candidate's thesis. It happened once or twice. They gave someone the higher degree because they considered the thesis to be outstanding.

So that was Dubna. In our group at Dubna there were fifteen or twenty people, half Russians and half not. I was the only Czech, I think, and there were some Poles and Hungarians

and East Germans.

COHEN: So you then finished this degree. You noted in the last interview that you already had a position in Czechoslovakia.

VOGEL: That's right. I had the position there. They held it for me the whole time.

COHEN: So then you got to the Bohr Institute, and you were a postdoc there. The climate must have been very different there from Dubna. What we call culture shock, maybe.

VOGEL: Yes. And the Bohr Institute was a very lively place at that time. Nuclear structure theory was rather popular then—not like today—and this was one of the main places.

COHEN: That's what you had done your thesis on, nuclear structure?

VOGEL: Yes.

COHEN: And so you continued to do this.

VOGEL: Oh, yes, I worked on the same things. There was a large group of young people there. I think there were something like thirty postdocs.

COHEN: Now we're in the late sixties?

VOGEL: Yes. And so I got some advice from Bohr and Mottelson. They were at that time trying to write a definitive book, or series of books, on nuclear structure, and various versions of various chapters were circulating. That was nice, because they would write the draft of a chapter and give it to one of us to prepare figures and so on and discuss. Unfortunately, that never got really written up.

COHEN: The book that they were working on?

VOGEL: Well, there are two volumes out. The first one is excellent and then the second one—

there were originally supposed to be three volumes and then it shrank into two volumes. It wasn't finished, and by now it's old.

COHEN: You personally worked on some of these chapters?

VOGEL: That's right, yes. We worked on problems that were then relevant. Neergaard, who was a student of Bohr's, had just finished his thesis. So we had the opportunity to write some relatively influential papers.

COHEN: So that really established your reputation in the wider community, these papers?

VOGEL: That's right, yes. Well, I already had quite a few papers published from Dubna.

COHEN: And then in Norway you just did the teaching?

VOGEL: Yes. Actually I was able to bring in a Czech student. That was a funny story. He had worked with me in Prague and the Norwegians gave him a fellowship, so he came and worked with me in Bergen. But then we decided not to go back to Czechoslovakia.

COHEN: What was his name?

VOGEL: [Ladislav] Kocbach. I was hesitant to tell him that we were not going back, because if it got out, they would immediately confiscate whatever we had, our apartment in Prague, and our relatives would be in trouble right away, and so on. So I told him only when it was all settled. And he was unhappy that he hadn't known this, but he stayed too.

COHEN: So he didn't go back either.

VOGEL: No. He married a Norwegian girl and he's on the faculty there, so he did OK. But I felt sort of bad, because I brought him there. He could have gone back if he wanted and denounced me or something, but—

COHEN: Oh, he would have had to do that?

VOGEL: Oh, probably.

COHEN: That must have been such a huge change—the atmosphere at Dubna versus the atmosphere at the Bohr Institute.

VOGEL: Well, yes and no. The ordinary Russian physicists were OK. I have some good friends there with whom I still keep in touch. And the rules—there is some Russian saying that laws are like poles: You cannot bend them, but you can go around them. For example, we were not allowed to travel more than, I think, ten kilometers from Dubna without a permit. In wintertime when you were cross-country skiing, you didn't know whether you were breaking the law or not. But I went on many trips with Russians, and when somebody who wasn't from our group was around, I kept my mouth shut so that they couldn't hear that I spoke with an accent.

COHEN: One learns how to live with the rules, whatever they are.

VOGEL: That's right.

COHEN: So then at the Bohr Institute you then met people from California. You met Felix at that time?

VOGEL: No.

COHEN: How did all that come about then?

VOGEL: Well, when we decided not to go back, I asked Bohr for letters and suggestions, and he suggested various places, and Felix was one of the people I was advised to contact.

COHEN: You didn't know him then?

VOGEL: No, no. But he had spent a sabbatical in Copenhagen before I arrived there, I think. Well, there were contacts. The Lauritsens [Charles C. and Thomas Lauritsen] were in close contact with the Bohr Institute. So I wrote various letters looking for jobs and Felix offered me a

job here. So that's what we did.

COHEN: Who was the head of the group that you were in, in Copenhagen?

VOGEL: Oh, in Copenhagen it was Bohr and Mottelson.

COHEN: So you were directly in that primary group.

VOGEL: Yes, that's right. Well, Jerry Brown, who also comes here—

COHEN: He was there also?

VOGEL: Yes, he was at that time. We knew him quite well.

COHEN: He's here again now. I see him walking around.

VOGEL: Yes, he is again here. At the time he was very helpful actually, and his then-wife was very nice to us. Vendula gave Russian lessons to her and her children, which was, I guess, some way in which they gave some support to us. We got a cat from them. We smuggled the cat into Norway, which was against the rules. The cat was under the seat in the car. But to bring it here was totally legal. [Laughter]

COHEN: Lucky cat. [Laughter] OK, so you came here and became part of Boehm's group. What were you working on here, and with whom were you working?

VOGEL: Well, there were various people. In the beginning, as I said, I continued working on the nuclear structure, and with my contacts also from Copenhagen. Actually, Kai Neergaard came and stayed a few months here. But then the group here started to work in Los Alamos on the accelerator there, so I switched to those topics. I did most of my work on that more or less alone, but there was a fellow here called Ed Seltzer. He was a little older than I am, and he had various nice computer codes, which we shared, and that's what I used. He then left physics. He was from a quite well-off family who didn't approve of their son being a physicist, so they forced

him to take over the family factory. They made tennis shoes and such.

So that was one person. And then working on these mesic atoms—that was a topic with which the group was concerned. I got to know people in Switzerland, where there is a similar accelerator, near Zurich. I worked with some of the people there. I spent also a semester there.

COHEN: In Zurich?

VOGEL: Well, the place is actually a little outside Zurich. Now it is called the Paul Scherrer Institute, but at that time it was called SIN, the Swiss Institute for Nuclear Research. It's a fairly large place. I spent half a year there.

COHEN: So you would have moved your family then.

VOGEL: Oh, yes, they went with me, because Vendula had a sabbatical. We spent one summer also in Freiburg, which is a Swiss city. So that was that. And then at the end of '79 came the interest in neutrino physics, which was inspired by Gell-Mann, basically, and there were various postdocs working with me. Then Felix became involved in doing the experiment in Grenoble, at the reactor there where Mössbauer, whom we knew from before, was the director. In preparation for that, one had to know how many neutrinos a nuclear reactor makes, which was not very well known. So I worked on that. I realized that the existing calculations were no good. Brian Davis and I wrote these two papers, together with some people from nuclear industry who people in Kellogg knew well. So we wrote two papers on this topic of the neutrinos from the reactors—papers that became rather influential. Basically, they started the right way of looking at it. And later it was realized that one can actually measure some things to test that, and this was done. These measurements are now replacing our pure theory. But I think our theory was a rather important part of it and still is.

COHEN: So that was a big piece of work that you did with this Brian Davis. And he's gone on to be a physicist somewhere else?

VOGEL: No, he also left physics. I am not sure actually what he does. I last spoke with him a long time ago.

Then the other problem, related to this, was something called double-beta decay. Again, people did measurements, but it needed some theoretical calculations, because this is a nuclear process. And we started to look at that with a student here, Peter Fisher, who is now at MIT. But then came a fellow who was a Prize Fellow in Kellogg. His name was Martin Zirnbauer. And we discussed the problems of double-beta decay with him and realized that there is a different way to calculate it, and we wrote a paper which put most people on maybe the right track—I don't know. It's a very widely cited paper. Martin was rather formal, sort of mathematically oriented, and he got a job in Cologne. He's there now.

COHEN: He was German?

VOGEL: He was German. He is a professor now in Cologne. I am still working on the various aspects of double-beta decay. There were various other Prize Fellows. You know, Caltech has this Prize Fellow program. Caltech has money from various foundations, for example Fairchild money. And every year there are several theory postdocs financed by this private money.

COHEN: That's Caltech money?

VOGEL: That's Caltech money. And Martin was one of them. I worked with quite a few others over the years. Basically, every year I had one. These are three-year appointments. The fellows get better pay than the regular postdocs.

COHEN: How many of those Prize Fellows are there? It still exists, I gather.

VOGEL: Oh, yes, it still exists. We just got another \$5 million from the Sherman Fairchild Foundation for that. There are, I don't know, ten maybe at any given time, something like that, maybe a little more—in theoretical physics. There is a similar program for experimentalists called Millikan Fellows. There were at least five Prize Fellows in the last decade or so working with me. They were all very good.

COHEN: And they were from the United States or outside the United States?

VOGEL: Well, Martin was German. The last one, who is now in Minnesota, Yong Qian, who is working with Gerry [Gerald J.] Wasserburg [MacArthur Professor of Geology and Geophysics, emeritus] intensely, he is Chinese. This was all very nice, because our grant was basically experimental. If I were to take the money for a theoretical postdoc, it would be difficult.

COHEN: This gave you a window of access to theoretical people.

VOGEL: That's right.

COHEN: And this continues, Petr? Do you still have some of these people?

VOGEL: Right now, no. The last one left last year. But we have another one coming this summer, an Italian. You know, the theoretical work is such that—for example, there is a fellow, Jon Engel, who was one of the Prize Fellows here working on the double-beta decay and other things, and he is now in Chapel Hill, North Carolina, and we still collaborate. Every other year we write a paper.

COHEN: Given the fact that you are just part of the research group, have you ever considered leaving here?

VOGEL: Yes, but I didn't make a big effort. And one of the reasons was that I like it here. The other reason was that, of course, Vendula was settled here and wouldn't be quite happy to go to another place. The children were in school here and so on.

COHEN: So this was home.

VOGEL: This was basically home. Even so, the position was not quite ideal maybe, but—

COHEN: As far as your work went, you could do as you wished.

VOGEL: Basically, yes.

COHEN: And you certainly have an international reputation, Petr.

VOGEL: Well, yes. That was fine. Also, you know, Felix and I worked well together. And we wrote this book on neutrinos [*Physics of Massive Neutrinos*, Cambridge University Press, 1987]. Now there are many, but at that time when we wrote the first edition, that was basically the first one, which I think was quite successful.

COHEN: Tell me about the LAMPF accelerator.

VOGEL: This is the Los Alamos Meson Physics Facility. It doesn't exist anymore—I mean, as a meson physics facility. The accelerator is still there. It's used for work with neutrons. That was a time in the seventies when work on mesons was considered important. And there are three similar machines built, one in Los Alamos, one in Vancouver, and the one in Switzerland.

COHEN: And then your romance with Aspen [the Aspen Center for Physics]—how did this come about?

VOGEL: Well, that again came about basically through Felix, because he was one of the first people to go there. Many Caltech people go there. The founding members were all Caltech physicists—Gell-Mann, Fred Zachariasen, John Schwarz, Steven Frautschi.

COHEN: Was this just because they wanted to go up to the mountains to work on their physics?

VOGEL: Well, it's a nice place. In the beginning it was a nice thing to do, because it was quiet, you had collaborators from other places, you could work. And now it's turned into more of a conference center.

COHEN: So who funded the center to start with? I mean, somebody must have given the money.

VOGEL: It has some NSF [National Science Foundation] and NASA money, plus you have to pay a registration fee, and they have some grants from government agencies. We liked it there, so we went many times.

COHEN: And you continue to work there?

VOGEL: Oh, of course, yes. But as I said, it's changing more and more into sort of a workshop/conference center, where people give talks. But since neutrinos are one of these hot topics in the last decade or so, there are workshops on neutrinos every other year.

Begin Tape 2, Side 2

COHEN: Felix's group has really come apart. I mean, it doesn't exist anymore.

VOGEL: That's right, but the activity continues. The previous large experiment which Felix initiated was done in Palo Verde, which is near Phoenix, on a reactor. And the next big step in this effort to study neutrino oscillations using nuclear reactors is an experiment we just started to get data on in Japan, and Caltech now is part of that. It's a much bigger effort. Again, it's a reactor experiment, except it's a hundred times farther from the reactor and the fluxes are lower, and therefore the detector is much bigger and more complicated. It's a continuation of the effort that started in Grenoble more than twenty years ago.

COHEN: Who else is involved in this, here at Caltech?

VOGEL: Bob [Robert D.] McKeown [professor of physics], and we have two postdocs working on it. There are people from many institutions—about half Japanese and half US. There are bigger groups from Berkeley and Stanford. Caltech is one of the smaller groups, but I think we do our share.

COHEN: Where is the central place where people meet to discuss this as the data comes in?

VOGEL: Well, the experiment is in Japan. And the Japanese are all from Sendai, Tohoku University. In the US, it's more distributed. There are meetings; there are collaborations. And there are groups that specialize in calibration or data reduction and so on. Every week, we have a telephone conference.

COHEN: So now, Petr, you're going off for four months. Where are you going to?

VOGEL: Well, we will be one month in Prague.

COHEN: And that has nothing to do with your work?

VOGEL: No, I will be at the university. I will give some lectures there, and I will have a desk and a computer, so I will be tied to my computer here. Then I will be one month in Germany, in Tübingen, where there is a group working on double-beta decay. I will meet with people with whom I've competed and collaborated over the years. And then I will spend some time in Denmark, in Aarhus, where there is a fellow who used to be here—[Karlheinz] Langanke. He was here for years, and we have been working together. We have lots of papers together.

COHEN: It sounds like you won't unpack your suitcase very much.

VOGEL: Well, maybe I overdid it. And then I think the last few weeks we will have a vacation.

Right now the plan is that I will formally retire in the summer of 2004, one and a half years from now. So these four months are sort of a test of how it could go.

COHEN: Retirement at Caltech doesn't mean that people stop working.

VOGEL: I know. So I will keep my office here and work on this neutrino effort. Even this particular measurement and analysis will take another five years or so.

COHEN: So, Petr, looking back at this long career of yours, can you see how this person or that person has been influential in pointing you in this direction? Anybody special?

VOGEL: It's difficult to say. Many people influence you.

COHEN: Most of your life has been taken care of by circumstances, I think.

VOGEL: Well, yes.

COHEN: You've enjoyed living in Pasadena?

VOGEL: Yes. Well, you know, there are always things one can think about that could be better, but basically it was fine, yes. [Tape ends]