SMB
Wolfgang Panofsky
[At SLAC?]

by Elizabeth Paris
11 July 1996

PANOFSKY: Well, I heard about storage rings in a general way even—I believe the first time I heard about it is there was a publication about colliding beams by a Russian by the name of Petukhov—which I think that was even before the war; you can probably look that up—about colliding beams in general being the way to beat the center of mass motion question. Then, I didn't pay any attention to the subject. And then while I was director of the High Energy Physics Lab here at Stanford, I received a communication—I don't remember how—from Gerry O'Neill and Bernie Gittelman from Princeton, who proposed that Stanford and Princeton should collaborate in an electron-electron storage ring. And then their proposal got them interest from Carl Barber and Burt Richter, who at that time were at the high-energy physics laboratory. And so O'Neill, Gittelman, Barber, and Richter formed a gang of four collaboration to build the thing.

Initially we had some difficulties in getting approval for that joint venture from the physics department, because the High Energy Physics Lab was not a national facility which was open to proposals, but was a Stanford machine, and the physics people weren't very happy with this application. And then I agreed to formally join the collaboration, although I didn't contribute anything much technically. The only thing I did was working on the magnet design which—the figure eight design had some peculiar magnetic properties in the magnetic path. But since I was a member of the physics department also, that—my participation basically calmed down the critics in the physics department, and so the project went ahead.

PARIS: What were Richter and Barber's positions relative to—?

PANOFSKY: They were both at the High Energy Physics Lab but not in the physics department. Richter was a research associate—a postdoc at HEPL and Barber was a member of the technical staff but not of the faculty, and he was in charge of the Mark II linear accelerator doing photonuclear work. So both of them were professional staff members of the high-energy physics lab, but they were not part of the academic establishment in the physics department at Stanford.

PARIS: And why do you think O'Neill and Gittelman chose Stanford?

PANOFSKY: Because we had electrons. It had nothing to do with love of Stanford. It had to do with the fact that the injector for the storage ring was there and no place else. So it was simply a technical opportunity.

PARIS: Was it because it was a linear accelerator? I mean, Caltech had an electron synchrotron—.

PANOFSKY: No, but they had no external beams. You needed an external—Caltech had an electron machine but its use is simply by x-ray generation, so they use the external x-ray beam. There was no extracted electron beam. So I believe Stanford was unique in terms of an external free electron beam.

PARIS: And was the first time you had heard of O'Neill was when he called you?

PANOFSKY: I think so. In fact, I recall that's the first time we became acquainted. Now, I do remember—and I don't remember—and I'm being very vague about this—when I was an undergraduate in Princeton, which was between 1934 and 1938, there was a project there building a small electron storage ring. Do you know anything about that? There was a small electron storage ring which never worked. Some poor graduate student was supposed to get a PhD on building this thing, and he never got it to work. But it was indeed—the intent was to build an electron storage ring.

PARIS: As separate from a synchrotron? Not storing—?

PANOFSKY: Quite separate from the synchrotron. It was a separate project. It was a small gadget. It was about so big. Well, probably less than a meter in diameter. And I just remember as an undergraduate, I was doing my senior thesis work there, and here was this gadget being built. But I don't have any remembrance who was the unfortunate student who never got it to work, and who was the professor who directed this.

PARIS: So when you say that Barber and Richter got interested, was that because O'Neill came and interested them—?

PANOFSKY: If I remember—and again, this is very unreliable—I think they got them interested while they were visiting in the East. I think they already knew about that. So it was prior to them visiting. And then the work was divided up. The vacuum chamber was actually built in Princeton, mainly by Gittelman, and Richter worked on the RF here, and Barber and I mainly worked on the magnet. And then I believe Gittelman and Richter and Barber did the primary work on the detector. But I was mainly the godfather of the thing in the sense of defending it administratively and justifying spending the money and all that, and all that. It worked. Reports were given at conferences. And it set a new limit on validity of quantum electrodynamics for hard electron-electron collision. So it was basically successful. It had relatively limited technical use. I mean, it was really a one-experiment machine.

PARIS: Let me ask you about the early days when you said you were the defender, because the people at MURA were very skeptical about whether this could be done. Do you remember any of that?

PANOFSKY: The people at MURA were somewhat skeptical, yes. I read some of those papers, and I looked at all the calculations, and I decided that it could—I mean, I did do my homework, before endorsing it, in terms of looking at the orbit calculations, and all that and all that. I did independently some beam-beam scattering calculations, and whether that would give any trouble, and the answer was no. So offhand, I didn't see any basis for skepticism, other than the vacuum had to be very good, had to be ten to the minus ninth level, and the magnet configuration was sort of crazy, in terms of the double ring structure and having the flux returns outright in the interaction region. But otherwise, there didn't seem to be anything fundamentally wrong with the thing. But it did seem to be a rather large effort for a single experiment. But at that time, there hadn't been any other experiments which pushed the limits on electrodynamics very far, so it sounded like a very valuable thing that the theorists were very interested in seeing whether something new would show up in QED for these—for collisions that hard. So after studying that, I felt it was technically slightly nasty, but I didn't see anything fundamentally wrong with it.

PARIS: Why do you think they were so skeptical?

PANOFSKY: I don't have the slightest idea. [laugh]

PARIS: You didn't encounter any—?

PANOFSKY: No, I wasn't directly involved in debating the critics. The only criticism which I remember was not technical but bureaucratic at Stanford.

PARIS: And it was?

PANOFSKY: From the physics department. It was simply—and that has nothing to do with the fundamental merit, but because the physics department basically at that time there was no machinery like there is today in having program committees evaluating competing proposals and all that. The laboratory was simply run as an instrument of Stanford. And so evaluating the proposals of outsiders who want to use stuff—that simply was not in the standard pattern of doing business those days. But I was quite enthusiastic about it. So anyway. And I did look in real detail technically, into the orbit stability calculations, and all that and all that. And I personally became convinced that it would work all right. But certainly it was initially definitely O'Neill's baby.

PARIS: Do you remember the name you used to refer to the collider?

PANOFSKY: No. Electron-electron storage ring? No, I—no, I don't think it had any particular pet name other than electron-electron storage ring. I don't remember. I reported on it at several conferences, and you can probably look in the literature. There were a fair number of papers, and a conference report. Do you have those?

PARIS: I think so. What interests me is that some people referred to it as the Princeton-Stanford ring, and some people referred to it as the Stanford-Princeton ring.

PANOFSKY: Oh, I don't have the foggiest notion.

PARIS: I was just curious.

PANOFSKY: I mean, it's certainly Princeton's initiative. There's certainly no question that O'Neill was the godfather. Certainly he deserves credit or blame for initially promoting the idea.

PARIS: And do you think you encountered any skepticism from proton physicists versus electron physicists?

PANOFSKY: I didn't. From my point of view, the only skepticism which was significant was, "Is it really worth this large an effort?" Because it's not a general tool in which it can do many things. You know that almost identical machine was built at Kharkov, in the Ukraine, in Kiev?

PARIS: Oh, I thought there was one at Novosibirsk.

PANOFSKY: No, that was an electron positron machine.

PARIS: I knew there was an electron-electron—.

PANOFSKY: But electron-electron machine was built, of almost identical design, in fact following this design, at the Physical Technical Institute at Kharkov in the Ukraine.

PARIS: And was that a result of O'Neill's meeting with Budker or was that—had it started earlier?

PANOFSKY: No, Budker had nothing to do with that. That one was not Budker's. Those were the people—I can look it up in the literature—at Kharkov. No, that had nothing to do with

Budker and that machine never worked right. Because they goofed up how to build the magnetic field right.

PARIS: And was that started after the meeting with O'Neill—?

PANOFSKY: A little bit after.

PARIS: They got the idea from a conference—?

PANOFSKY: I think so. I think so.

PARIS: During the construction of the electron storage rings, there's a lot in the literature, memos back and forth, about Mount Panofsky, and moving Mount Panofsky in order to—.

PANOFSKY: Oh yeah. That—the Mount Panofsky was an earth mound which was the backstop of the electron beam. I don't remember the details. In the High Energy Physics Lab, the beam had to be stopped, and rather than having an elaborate concrete structure or whatever, we dug up a big earth mound with a reentrant thing and had a beam dump into them, into the hole, and we calculated how thick it had to be, and all that and all that. And in order to build the enclosure for the electron storage ring, we had to I think move some of that dirt back. But that's not a big deal. Because it was technically—I mean, Mount Panofsky was simply a bunch of earth. It was a beam backstop.

PARIS: And how did it get its name?

PANOFSKY: I haven't the slightest idea. Somebody made a joke, and it stuck. I was designing how big it had to be, and all that and all that, in order to stop the beam properly. So some humorous character decided to call it that, and I think the joke stuck. [laugh]

PARIS: Let me ask you a little about the money. You went to the ONR with O'Neill's proposal.

PANOFSKY: Yes.

PARIS: And you ended up getting money somehow from both the AEC and the nuclear physics branch of ONR.

PANOFSKY: Yes. Now that, I don't remember how that worked. I believe I got—if I am not mistaken, William E. Wright, who is now dead, of ONR, was very sympathetic. He was the program manager for the Office of Naval Research. He was very sympathetic, but they didn't have enough flexibility, so George Kolstad, who at that time was a program guy in AEC, was also sympathetic. But I don't remember exactly how those discussions went. But I did get support from both of them, and that's about all I remember. I don't have the foggiest notion, memory, on how much money was involved and to what extent we supported it from the regular funds of the lab. I don't remember. Sorry.

PARIS: That's OK. In 1960, you were at the PSAC meeting on—I guess it would have been on high energy physics, or a meeting just the future of physics.

PANOFSKY: What meeting?

PARIS: The PSAC, President's Science Advisory Committee.

PANOFSKY: Yeah.

PARIS: And the General Advisory Committee. There was a joint meeting.

PANOFSKY: GAC-SAC, yeah.

PARIS: But storage rings weren't very prominent at that meeting, in the committee report at least. And I wondered if they would have been mentioned at all if you hadn't been there, and how much confidence you had in them working at that time.

PANOFSKY: I don't remember at all. You're drawing a blank. One thing which happened—now, again, I don't know about that meeting—it was definitely the President's Science Advisory Committee, and I think it was the Ramsey panel where storage rings were mentioned as being a promising thing of the future, and specifically the people at Argonne were requested to make a design study of a one-GeV proton storage ring. You remember that? That was an initiative. That was one of the rare items where the government, rather than being responsive to a proposal from people, where a design was conceived in committee and we sort of crammed it down Argonne's throat to do the study. But at that time, it was clear that the standard mass advantages of storage rings clearly pertained to proton-hadron machines and the time was ripe for somebody to take that seriously. But that, I think, had nothing to do with this—with electron machines. I think that was an independent realization that proton-proton colliders were here to stay.

PARIS: Well now, O'Neill seemed to think that the electron-electron collider would be a forerunner, would teach things that would then help the proton-proton.

PANOFSKY: Oh yeah, that's true. O'Neill—I mean, O'Neill is dead now. Did you ever meet him when he was alive?

PARIS: I talked to him on the phone once. But I've seen his papers.

PANOFSKY: I see. Well, he was sort of a strange fellow, as you know. He was one of the people who—he was a very original guy. He was not very popular at Princeton, because he would always sort of pursue various and sundry ideas which were sort of off the mainstream of work, and which always took him away from the laboratory, so that he was not very popular because of his—he didn't attend to his teaching duties very well. As you know, in his later life, he worked on storage rings, and then he switched to space station work. O'Neill did a very unsuccessful experiment here on the SPEAR storage ring. He proposed and we approved a single-function detector which he built for looking at kaon and pion production at very high momentum, higher than was reachable with the Mark I and Mark II general purpose detectors. And the trouble was that he was wildly optimistic on counting rates, so that experiment was a very large effort and got only very few events in the entire lifetime. O'Neill also was a user of Burt Richter's storage ring here at SLAC later on.

PARIS: How come he wasn't involved in building that ring, since he was—?

PANOFSKY: I don't really know. Burt Richter simply grabbed the initiative on that, and did a beautiful job doing it. The idea—I understood—I was director of the lab here—perfectly well that whole idea of building an electron positron storage ring was not original with Richter. I mean, O'Neill had talked about that—O'Neill had talked also about that. But the two of them simply didn't collaborate, and the matter simply never came up. I mean, O'Neill never proposed—O'Neill never took any initiatives to join that venture. So it was basically directed by

Richter. Gittelman, if I recall, visited a few times, but O'Neill didn't. I'm not quite sure whether O'Neill by that time had already got interested in the space business. I frankly don't remember the timing. Anyway, O'Neill was not involved at all with those rings, but then he made a proposal to become a user of the ring, and we approved his proposal. And he did—he built a high-energy spectrometer but which had such a small solid angle that the data rates were low.

PARIS: You said that he wasn't particularly popular at Princeton.

PANOFSKY: Right.

PARIS: How did he get along with the people at Stanford?

PANOFSKY: Oh, pretty well. I think it was a fairly successful collaboration. And people recognized that it was his baby, in the sense that he had given the original impetus to it. So I think that worked fairly well. I think people, if I remember—I don't know how much you're writing about human element—people, again, griped a little bit that he was a man full of ideas, but he wouldn't sit still long enough to really participate much in the dirty work. Gittelman and Barber and Richter were really the three guys who actually did the hard work. Gittelman did the hard work on the vacuum chamber at Princeton, and Barber and Richter did the hard work in getting things going here. So I don't recall that giving rise to any friction. But it was certainly true that O'Neill was more of an idea man than really a sort of steady hands-on plumber. [laugh]

PARIS: What was the interest in the physics then once—there's the building of the machine. There's the data analysis.

PANOFSKY: Yeah.

PARIS: Did he have anything to do with the data analysis?

PANOFSKY: No. Did I have? No.

PARIS: Who did? Anybody?

PANOFSKY: Oh, that analysis was largely done by Richter and Barber. Gittelman was not involved very much, and O'Neill was not involved very much. It was largely done by Barber and Richter. Have you talked to Richter?

PARIS: Monday.

PANOFSKY: Oh, yeah, he's in Sonoma. Yeah, he can probably tell you that in more detail. But as I recall, that was basically Barber and Burt Richter. But all four of them signed the papers.

PARIS: You, early on in the process, recommended to O'Neill present a paper just under his name, and you made some half-joking, half-serious—"to protect himself."

PANOFSKY: I don't remember that.

PARIS: You don't? OK. I was just wondering why you might have said that.

PANOFSKY: It could be, if you got that scuttlebutt from somewhere. But darned if I remember.

PARIS: Also, even as late as '65, O'Neill was talking about turning the electron-electron collider into an electron-positron collider maybe by turning over one of the—.

PANOFSKY: Yeah, that doesn't work.

PARIS: Do you remember any of that?

PANOFSKY: Yes. I remember that quite early, O'Neill understood very clearly that electron-positron collider was a much more flexible device in terms of multiplicity of reaction channels, than the electron-electron collider. But he never—he talked about it, but he never made any serious proposal. And just as a technical matter, there's no way you can have that electron-electron machine be converted to an electron-positron machine. In addition to that of course the Mark III linear accelerator did not have a positron beam. I mean, it had plenty of—I mean, one could have put in a positron converter and all that and all that, but nobody ever proposed that. So it would have been a fairly—in addition to building another ring, you would have to make a positron beam installation into the Mark III accelerator.

PARIS: Was that ever done?

PANOFSKY: No.

PARIS: When he first brought the proposal to you for the E-minus E-minus, was there any talk at that time of, "Oh, it could be plus-E-minus but we don't have the injector"?

PANOFSKY: Oh, not to my recol…not initially, I don't recall any such discussions. I know that later on, he at various times said, "But of course a long-range future is electron-positron colliders." He understood quite clearly that the physics was much more powerful for electron-positron colliders. But he never converted that to serious proposals and we never did anything. But he was certainly fully aware of it. And then, again, I would suspect he took the initiative, but that's—but not—and nothing serious was ever done, and he never did make a serious proposal, and nor did Richter or anybody else.

PARIS: It was initially proposed—it was supposed to take two years; one year to build and one year to take the data. And so say it's the year '60 to '63 when it's still not working right—I mean, even for single-beam storage—

PANOFSKY: Yeah.

PARIS: —how did you kind of keep the faith? Was there any doubt that it would work?

PANOFSKY: No, no. You see, there were lots of things going on at the High Energy Physics Lab. This was just one of the things. And it went slower, and so there was no panic or desperation or whatever. We just plugged along and fixed what had to be fixed. No. I think I am not aware of any panic or desperation or suicidal tendencies or what have you. Just took longer. But we were accustomed of that. Some things work—in a certain way, you have to put yourself into that period. Things were not as—the high-energy physics lab was not a user facility. There were not many people standing in line waiting for the guy before to get out and get in there, and so forth and so forth. So if things took longer than people proposed, then they took longer. If it took seven years to get a PhD rather than five, it was a poor student's hard luck, but that's the way life went. So the others, no. There were certainly no—and we were not being hounded by the funding agencies to either put up or shut up. I mean, there was no such—it wasn't that kind of atmosphere.

PARIS: Do you remember any differences that maybe caused friction, or maybe didn't, between the Stanford administrative practices and Princeton administrative practices?

PANOFSKY: None that I recall. That doesn't mean—there may have been. I don't think there were any administrative problems of any kind. I mean, they—the Princeton side of the house functioned fairly independently. They manufactured—the entire vacuum chamber was built in Princeton, and then was hauled out here. And I don't remember if there were any schedule slippages there or any other such troubles, and it worked fairly well, and it was a well—I mean, Gittelman—that was mainly Gittelman. Have you interviewed him?

PARIS: Not yet.

PANOFSKY: He's a fine fellow. And they did a workmanlike job, and I think there was minimal trouble with that. Whether there were administrative troubles, I don't recall, but technically, it was OK.

PARIS: What do you think the most important aspect—thing that came out of that machine was?

PANOFSKY: Well, the basic—firstly, it was a pioneering thing. It was the first machine, electron-electron—electron—or electron-positron or whatever storage ring, which A, got built, and the same time did some physics. I mean, there had been several—as I mentioned, certainly my knowledge is not very complete—there had been several other attempts around the world to build storage rings. None of them ever worked very well and none of them ever contributed to physics. So this was I think the first storage ring which worked more or less as advertised, had a good beam lifetime, and it actually produced physics. They advanced the limit on the validity of the QED for hard collisions. And people were interested in it, because there were speculations that there would be deviations. So it was a pioneering effort in that respect, both of having a storage ring really work, and having it really do physics. But it was not the first attempt at building such a beast.

PARIS: Do you remember any issues around the injector?

PANOFSKY: About what?

PARIS: The injector.

PANOFSKY: No.

PARIS: Not in particular?

PANOFSKY: Not particularly. I mean, of course the electron beam was there. I mean, the Mark III accelerator was a routine operating machine, and but very easy to change the energy of the Mark III accelerator to anything you wanted to. So that was not a big deal. But the pulse injector was built and worked reasonably well. They had some breakdown problems and so forth, but I don't remember—it was never a crisis object. It was a limited thing. By injector, do you mean the pulser or the electron beam?

PARIS: The injector into the rings.

PANOFSKY: The injector into the ring, OK. No. I mean, that, again, was an original design, but it worked. It had some breakdown problems if I remember, but it was basically—it was not a disaster.

PARIS: Maybe we can move to about '63 when the beam is being—single beam in the e-minus e-minus and began to think about SPEAR—not SPEAR, can't remember the—just electron-positron, I guess, at that time, the name—

PANOFSKY: Yeah.

PARIS: —and begin to propose that and have some trouble. Do you remember?

PANOFSKY: I don't remember. You have to talk to Richter. Of course you understood that SPEAR was originally proposed as a two-ring machine, not a one-ring machine. In fact, you remember it was proposed first—the reason it's called SPEAR—the "A" stands for asymmetric. So it was one ring which was sort of pear-shaped, and the second one, pear-shaped, and then there were two interaction regions. And then with more analysis, it was decided there was no point to build a two-ring machine, but a one-ring machine with electrostatic separators. But that was a proposal to SLAC, and the proposal was by Richter, and Phil Martin who was here, who was one of the main beam design…was involved in the beam design. And Burt Richter collected his crew from several places. I was not involved at all. By that time, I was lab director here, and I had administratively a fair amount, but I was not involved in the design or anything. And there was basically a transition. It started out—we have some early drawings of it being a two-ring, double-pear-shaped machine, and then that design got changed after realizing that that [inaudible].

And then of course SPEAR, I tried to get approval of SPEAR as a construction project from the government and failed. And so we finally decided to basically bootleg the thing, although the AEC people of course knew about it. But we basically—I finally decided it was so important that we should get it out of our own hide. And so it was built, as you know, not in a real building, but out of shielding blocks and so forth. So it was never authorized as a construction project. But I had several discussions with the then-controller of the Atomic Energy Commission, a somewhat humorous gentleman by the name of Abbadessa and he was quite aware of what we're doing. But it was never approved. It was sort of a paradoxical thing. SPEAR was probably the most cost-effective machine ever built in the history of high-energy physics. It was very inexpensive. It was done very much in a hands-on fashion. The building in which it is never had a floor. It was just on individual foundations drilled into the ground. And it did a fantastic amount of physics. But it never had any real opposition, but some other people couldn't scrounge up the support, had tried very hard and failed, to get it authorized as a specific activity. And they never worked.

PARIS: And you don’t know why you couldn't—

PANOFSKY: It was mainly inertia. It wasn’t really opposition. It was just the fact that during those days, there were just a lot of ongoing user activities all over the place, so funds had been scarce and people just kept saying, “Not this year.” And we finally said, “The devil with it. We'll do it.” And I once—when it was done and we discovered the psi and all that, I told the controller that I wanted to report the discovery of an unauthorized particle on an unauthorized accelerator. [laugh] An unauthorized storage ring. And he laughed, and said he was glad to hear it. So it was never in any way an acrimonious debate, like whether it should or should not be built, but it was sort of an always “mañana” situation.

PARIS: And during that time, storage ring construction started to pop up all over the world in other places.

PANOFSKY: Yeah, well, I don’t— of course during then, that’s right. Well, there were the Russian machines, the French machine, the Italian mach…there was the two rings in Italy. Are there any other more popping up all over the world?

PARIS: Did you mention the French one?

PANOFSKY: I mean, Russia, France, and Italy I think are the—so but clearly the idea of e-minus e-plus machines. And of course we were very for…since we had an accelerator of much larger energy than anybody who had conceived to build a storage ring, it was a very arbitrary decision whether to make it at the energy in question. So we were very fortunate that we had the highest-energy machine, because the available injection was not remotely limiting. Both electron beam and positron beam from SLAC were way in excess what was needed.

PARIS: Do you remember any contact between the other storage ring physicists and people at SLAC?

PANOFSKY: The other what physicists?

PARIS: The other storage ring physicists in Italy and in France and in Russia?

PANOFSKY: I don't remember any details, no. Certainly there were no secrets. There were certainly visits and conferences, and you can probably—and everybody reported their progress. The answer is no, I don't remember any specific contacts. But Richter probably would. But I do remember progress reports being given on high energy physics conferences, both by us and by the others. But I don't remember any specifics.

PARIS: You said eventually you realized that two rings were a waste of money?

PANOFSKY: Yes.

PARIS: How long between, say, '63, when the initial intent to submit a proposal was submitted, and '70, when you finally went ahead? Do you remember?

PANOFSKY: Richter would certainly know. No, I don't know exactly when that happened.

PARIS: You also mentioned that it was the most cost-effective machine ever built?

PANOFSKY: Yeah.

PARIS: What made it cost effective? What contributed to it?

PANOFSKY: It gave rise to the whole psi/J and then psion spectroscopy and the tau and the spin structure of the quarks. There were really some major pieces of physics. And it was a relatively—and it was built out of ongoing operating funds. So in terms of a total construction cost, Richter may have a figure on total construction cost. I don't think we even kept track of it.

PARIS: I understand why you define it that way, but how do you think it got to be cost-effective? What contributed to it being able to be that way?

PANOFSKY: Well, I can only give the usual answer—skill and luck. Basically [laugh] Richter and company did an excellent job building the thing. The RF system, which was new, was done by a fellow by the name of Matt Allen, which was also very good. Reis was involved in the early days. But it's actually a simple machine. And then Matt Sands who was for a while top deputy director here, wrote his—at that time really did the fundamental—he sort of wrote the bible on the orbitry. So it was built with an excellent understanding of the orbitry so there weren't any mysterious things to beat up on by trial and effort. And there weren't any great surprises in the machine working.

PARIS: Do you think that was helped by the machines that were already being built?

PANOFSKY: Yes, although many new things were new. Also of course a thing which was very new and which has been sort of the model ever since was the use of a single solenoidal detector. You have to give Richter credit for the fact—there was a lot of luck in the actual discoveries; on the other hand, he did a fantastic job in terms of really seeing the power of building a 4Pi detector, which is probably as much a reason for the success of the thing as anything else. I mean, it was very characteristic that in SPEAR, they built the Mark I, the 4Pi detector, but then several proposals were made to build single-function detectors, including the one by O'Neill for the high-energy e-plus e-minus to K+, K-, and Pi+, Pi- things, and then a single—Hofstadter and his people built a relatively small angle gamma detector. And when it actually all died down, it turns out the single universal detector basically wiped up the field. It turned out that the single function detectors to pinpoint similar experiments but at much lower solar angles were never competitive with what could be done in a single comprehensive detector. And I believe Burt Richter deserves credit for being the sort of pioneer of that. And since then, every single detector in a storage ring in the world sort of looks like that.

PARIS: What was the detector on the electron-electron collider?

PANOFSKY: It was a more finite solid angle. It's published. It had, I believe, a plus or minus 45 degree aperture and a series of chambers. I'd have to look it up. But anyway, it was not—if was of course non-magnetic. They were not magnetic. And it was two 45-degree cones in coincidence on the two sides, with a series of detection planes. So but it had a broad enough sort of angle so you could get enough angular distribution to look at any deviation. But it didn't go very far into the forward and backwards directions.

PARIS: Let me get back to the design of SPEAR. What are advantages versus disadvantages of weak focusing versus strong focusing?

PANOFSKY: Well, the advantage of weak versus strong focusing are the fact that one is weak and one is strong. I mean, basically, in strong focusing, you can keep the beam diameter and chamber aperture much lower. And in fact, old circular machines, of all the circular machines which were ever built, weak focusing, which were successful, was first the Bevatron, and of course it had a very large aperture. And then the ZGS was not—well, wedge focusing, namely focusing due to the slanting on the edge. And since then, all machines have been strong-focusing, and you simply enormously gain in aperture.

PARIS: What do you lose?

PANOFSKY: You lose some in orbit complexity, but you don't really lose anything. It's simply a better way to go. And basically strong focusing and phase stability were the two big things which basically have been the roots of all future machines. So it's simply a way of getting reasonable magnetic fields, of getting much tighter orbits. And that of course drastically reduces the cost, because of magnet apertures and the bending magnets can be much lower, and blah blah blah. All that.

PARIS: Did the designer—?

PANOFSKY: You know how strong focusing works?

PARIS: Not particularly. I know that—

PANOFSKY: I can pontificate and give you a—

PARIS: Sure, that would be great.

PANOFSKY: Basically, what strong focusing is based on is the fact that, you see, you cannot build a magnet—in order to focus with a magnet, the only way people knew before strong focusing—the only way people knew how to focus was with a solenoid. Now, a solenoid is very tricky. If you have a coil like this, then the magnetic field goes like this. But then what happens is that when a particle enters, this way, then the fringe field give the momentum in this direction. So the beam then screws around this way, and so then the interaction between the beam spiraling and the magnetic field in this direction drives it in. But turns out that the focal length with a solenoid is weak, because the magnetic field has to act twice, namely the first field fringing field has to act to spin the particle around, and then the main field interacts with the spinning around orbits and drive it in. And it turns out the focal length [phone rings] is given—can you get that?—turns out is given by this square. It's a quadratic effect. That's what makes it weak. Over b rho squared.

PERSON: It's Dick Garwin.

PANOFSKY: Oh, OK. Let me get that.

[Break in audio]

PANOFSKY: —solenoidal focusing. And then you can—it's a straight line. That's all you can do. Now when you have a bending magnet and the magnetic field—if you have a bending magnet here where the field sort of goes like this, then when action—when you're off center, then in addition to the main component, there's also the radial component. The radial component drives it in. And so if the magnetic field falls off as the radius of a—ah—the radius of a constant to the minus n. Then if n is between zero and one, then you focus, because of the fact that there is a magnetic field has a horizontal component which interacts with the orbit going this way and drive you in there. But the field is very weak, because it's only the small component. So that is—so the normal weak focusing is due to the fact that you get a radial field fall-off of the field. And in strong focusing, you use quadrupoles. You do the following thing. You use north, north, south, south pole. And so you get a field which looks like so. So if a particle comes in, then this field here will drive you inside, in, and this field in the other direction will drive you out. So this means you have a converging lens in one plane, and a diverging lens in the other plane.

Now, one thing in optics is if you separate the converging and a diverging fields, the net effect is positive, because what happens is if a beam comes in, it moves in—now if it's diverging, you move out again. But since the beam diameter is smaller here than it is here, then you're closer to the axis and there the diverging effect is less. So the net result is if you have two lenses each of focal length f but minus and plus, separated, then the combination turns out to give you a net focusing effect which is that. And that's called strong focusing.

So the invention of strong focusing was that if you make a quadrupole but separate two of them, 90 degrees, then the combination of the very strong focusing effect in one plane and defocusing in the other plane give you a net focusing in both planes. And that's called strong focusing. And it turns out it's much stronger than either solenoidal focusing or the radial falloff focusing. Those are the other kinds. And if you focus stronger, then you pinch the beam up more. So that was a major breakthrough, if you call it that—invention of strong focusing by Livingston, Snyder, and company. And therefore the advantage is—and the beam is much tighter, the magnet can be much tighter, and that makes it cheaper. And so it's a huge reduction in cost of storage rings and any other kind of circular machine.

PARIS: Livingston—those are CEA people.

PANOFSKY: Yes. Courant.

PARIS: I'm sorry?

PANOFSKY: Yeah, Courant, Livingston, and Snyder wrote the first paper.

PARIS: After, say, '65, when the CEA machine was not approved and it was going to be at SLAC if it ever was going to happen at all, did you learn—did you get anything from the design of the CEA machine that wasn't going to be built? Or did the people come over? Or how did that affect—?

PANOFSKY: Not really. Of course, the CEA machine was—CEA was converted into a storage ring by the so-called bypass scheme, and it worked. It worked. It didn't give any very high luminosity, and they didn't do a very good job doing physics with it. I mean, one of the sad things about CEA is that it was really a very good machine, and they did lots of pioneering things—I mean, it was used—you know the difference between a storage ring and a synchrotron? A synchrotron, you ramp up and—so, OK. In the CEA, they had the ingenious idea to convert the CEA into a storage ring by the so-called bypass scheme, but they didn't build a very good detector. And in general, the weakness of the CEA was that by and large, the people who did physics didn't really do as good a job as they did here.

We did a much better job in integrating the construction of good detectors and good machines sort of into a unified whole. And Richter is a prime example of sort of simultaneously building a good storage ring and a good detector, while CEA was designed to be a machine sort of as a service machine for MIT and Harvard. And MIT and Harvard people would sort of—weren't integrated into the running of the place, but they were strictly sort of users, and they never instrumented what the CEA people did with as much total effort and quality and quantity as here. So the weakness there was in a funny way, not the machine builders, but the particle physicists. The machine builders in CEA did a fantastically good job in innovating all sorts of things including the one and only synchrotron with a storage ring annex, so to speak.

PARIS: Can you think of any other reasons why you say they were such good designers or such good engineers? Any other particular innovations, engineering-wise and structurally?

PANOFSKY: At the CEA?

PARIS: Yeah.

PANOFSKY: Well, no, but it was a big leap forward. I mean, building a 6 GeV storage ring—electron synchrotron was a big engineering advance. And the sad thing is that they didn't use it all that—but you see, the CEA was a major advance, not in storage rings, but in electron synchrotrons.

PARIS: Going back a little bit, because you mentioned Hofstadter, and I wanted to ask you if you knew anything about the relationship between Hofstadter and O'Neill.

PANOFSKY: Hofstadter and O'Neill? They didn't like one another very well. Hofstadter didn't always—he didn't like O'Neill. That was one of the reasons why in order to get the electron ring going at Stanford, I had to personally intervene. Hofstadter and I were both members of the physics department. And Stanford University in the old days was basically—the physics department was very much proud of its past record, and tended to be not very hospitable to outside users. HEPL, High Energy Physics Lab, was in no way a national laboratory, but more the proprietary fiefdom of the local inmates. And Hofstadter in particular was very proud of that, and he didn't like O'Neill from his Princeton days. He felt that O'Neill was sort of operated—anyway, I don't know—the chemistry wasn't good between the two gentlemen. So when O'Neill made his proposal, Hofstadter was unsympathetic.

PARIS: This is in '58 when he made the proposal?

PANOFSKY: Yes. That was—'58. Yeah, right. But since I was the director of the High Energy Physics Lab and also a member of the club, if you wish, of the physics department, I basically said, "But I'm sponsoring it" or something, and so that means by the rules of the game, it was efficient. So it was sort of unfortunate. I always felt much more liberal—that the High Energy Physics Lab belonged to the world, and if somebody had something good to offer like O'Neill did, more power to him. But anyway, the chemistry was not good between the two gentlemen. I don't know whether they had any prior background or mainly had to do with the fact that Hofstadter felt that grafting such a new object to the High Energy Physics Lab would simply interfere with what he was trying to do. I'm not sure.

PARIS: I talked to Dave Ritson yesterday. He mentioned that he and Richter—and there was some controversy over whether you were there, so I want to ask you if you were—went to CEA in 1963 to talk to I guess Livingston, probably, at the time, about the fact that both labs wanted an electron-positron collider. Do you remember that trip at all? Do you remember talking to him?

PANOFSKY: Sorry.

PARIS: Do you remember when you first heard that CEA was interested in an electron-positron collider as well?

PANOFSKY: No. Well, they built the bypass.

PARIS: Well, the bypass was after '65, when they found out they weren't going to get one.

PANOFSKY: Right. No, I don't remember that. No. I mean, certainly the CEA folks knew about how to build electron-positron collider. And storage rings. What can I say? They're good. No. I mean, CEA to me has always been a bit of a tragedy in the sense that they were very good machine builder, and the unfortunate thing was that the whole laboratory was set up sort of as a service organization to MIT and Harvard, but MIT and Harvard didn't put enough intellectual effort to really support it. I mean, they wanted to use it, but they didn't want to support it. This is not deliberate, but that's the way it worked.

So the research output of the place didn't match its performance as an accelerator laboratory. It did a superb job as an accelerator laboratory in innovation in understanding about storage rings and understanding about characteristics of synchrotrons and original ideas about injectors and ejectors, and lots of other good stuff. And after all, Livingston was the co-inventor of the strong focusing business. But some other—the physics never came out. And of course, they built a bubble chamber there, they had a disaster, and it blew up and killed one guy, and a friend of mine got very seriously injured and so on. But again, that was the outside people not being careful enough. So it was to me a great lesson—that you just can't set up the accelerator people to be servants of the particle physicists, but you have to basically appreciate the intellectual contributions of the accelerator people and the physicists in a totally parallel, integrated way, rather than having one be the hand maidens of the other, or the servant of the other.

PARIS: Do you remember how SPEAR got its name?

PANOFSKY: Yes. The Stanford Positron-Electron Asymmetric Ring. It was called that because that's what it was, even though it didn't end up to be that. So it was sort of a joke, that when we decided to throw away the asymmetric feature, since it was such a cute name, people just said, "Oh, the hell with it. Keep the name."

PARIS: Do you remember how the name came up? Was it one person—?

PANOFSKY: No. I don't have the foggiest notion.

PARIS: Matt Sands says he presented you with five or so acronyms after he got off a plane from Europe one time.

PANOFSKY: Not that I remember. Could be. Life is pretty full.

PARIS: [laugh]

PANOFSKY: [laugh] No, I'm sorry. A lot of those human interest remembrances, I simply don't remember when and how. And I'm very unreliable with dates.

PARIS: I'd like to, if you could, ask you a little bit about your Washington experiences, because you're someone who served on a lot of committees.

PANOFSKY: Right. Still do.

PARIS: I'm curious what was happening behind the scenes. What kind of influence did you feel that you had, say, on PSAC, on policy?

PANOFSKY: On PSAC?

PARIS: Well, or pick a committee that you feel had more—

PANOFSKY: Well, on PSAC, I had quite a lot of influence, because I was relatively young and loudmouthed relative to some of the other more dignified characters. I was mainly interested—on PSAC, my main role was not at all in basic science support, but was almost entirely in national security policy, namely I was chairman of the strategic panel—what's called strategic panel. I was very much worried about the arms race starting, the evolution of ICBMs, and I wrote a major report on vulnerability of ICBMs to countermeasures. I wrote a very negative report during the Eisenhower years on civil defense. I was mainly interested in military affairs.

PARIS: Did you feel that you were listened to?

PANOFSKY: Very much so. And I took a lot of initiative on moving within the government on the Nuclear Test Ban Treaty. I was an official—1959, I was a negotiator for the government on technical means to verify cessation of nuclear testing. So I had relatively little to do when I was a member of PSAC and lots of other panels and committees and whatever—essentially I had very little to do with—let me distinguish between government in science and science in government, namely science in government meaning giving technical input to policy decisions. Government in science means support of science by government.

I had essentially very little to do in formulating any kind of lobbying or support of governmental support of science. For two reasons. Firstly, on many things, it would have been a conflict of interest because I was directly the head of the lab here. And secondly, I really wasn’t—paradoxically, I never was all that interested and I didn't think all that carefully about the general principles about it. Only thing on that—I was a member of the National Academy of Sciences, of what’s called Physics Survey Committee, under George Pake, which was one of the committees which was periodically convened to look at the prospect of physics for the next ten years ahead or something. So most of my governmental work was largely in security and mainly in arms control. I was very much involved in the original move to set up an arms control and disarmament agency in the government. And then I talked personally to Eisenhower several times on nuclear test cessation and all that.

So that has been an avocation, sort of, in parallel with high energy physics stuff. So I sort of isolated those things. I didn't have a heck of a lot to do with money for science. I had to testify on behalf of SLAC several times. I gave testimony on what we were doing and what we needed and what have you, for SLAC, of course quite a few times. But that was under my hat as lab director, not under my hat of members of PSAC or what have you.

PARIS: Maybe you can help me out. I've come across several times security clearance Q, security clearance F. Do you know—do you remember—?

PANOFSKY: Security clearance what?

PARIS: Q.

PANOFSKY: Q?

PARIS: Q, yeah. I guess letters seem to rank the security clearance.

PANOFSKY: I've always had security clearances. Q is a code name for the clearance which the Department of Energy gives for access to classified information, but to be technical, what's called restricted data. And for complicated reasons, the secrecy rules in the Defense Department and Energy Department and its predecessor agencies are different. The legislation which set up the Atomic Energy Commission and so forth defined certain areas of science as to what's called "born classified." That means whether the government supports it or not. I mean, if you in your garage invent a new way to make a nuclear bomb, that's classified, and you need a Q clearance to learn about it. And on the other hand, if you in your garage make a new cannon or rocket or missile or something like that, then you can sell it to the bad guys and you're not in violation in any kind of a law unless the government paid for it. So there's a tremendous difference the way security is being run by legislation on stuff in nuclear energy and stuff in other military affairs.

So a Q clearance is a clearance for the Department of Energy, or what used to be, before that, Energy Research and Development Administration, and before that, Atomic Energy Commission, having to do basically with the science and technology associated with making nuclear weapons. And top-secret or what have you clearances of the Defense Department give you access on a need-to-know basis on information which is classified because it's technology on missiles or missile defense or operational plans for military use and so forth and so forth and so forth. Recently, as you probably know, Hazel O'Leary, the secretary of energy, started a major initiative to basically clean house and decrease drastically the total amount of stuff which is classified in the Defense Department. She started in 1994 what's called the Openness Initiative. I was a member of two panels—she asked the National Academy of Sciences to help her on policy with that, and I was very much involved in that, to try to decrease some of the sort of secrecy culture in the Energy Department. And she has done a good job doing that, although it's slow going.

PARIS: It seemed that scientific expertise at the AEC back when it was the AEC was fairly high.

PANOFSKY: It was higher than it is now, but it's again very different. It was not as high as you might guess it was. And the reason is the following: the national laboratories are really from the very beginning of the AEC—I mean, it includes the war—were always partners with the AEC or the DoE. So as an example, when I was on the President's Science Advisory Committee, when some military things were being discussed about nuclear weapons or nuclear testing or what have you, the AEC would always bring in people from the laboratories to testify for them before us. While if you wanted to have military information from the Defense Department, they either bring in military people or civilian staff people from the Pentagon. They did not depend on the contractors.

So there has always been a tension. On the one hand, the national labs like Livermore and Los Alamos and Berkeley and Oak Ridge and whatever are formally contractors to the government. They are operated through private institutions. But at the same time, they are really part of the government in terms of the fact that the real expertise in the witchcraft for nuclear weapons and nuclear reactors or what have you resides in the laboratories and not in the civil servant personnel. And so now that relationship has been somewhat under fire, because Congress has been critical to some extent saying that's too cozy of a relationship between the government and the contractors.

So it has always been an ambivalent situation whether the big labs—and this one included, as far as that goes—whether we are partners with the government or we are simply hired hands, contractors. So purely legally there's no question. If tomorrow the DOE wants to cancel the contract with Stanford to run SLAC and instead contract with the Elks Club to run this place, legally they can do that. As a practical matter, it's pretty hard to do that. And as you know, there are debates going on whether Los Alamos and Livermore should be contracted to the University of California or whether it should be contracted through Lockheed aircraft or the University of Texas or some darn thing. But as a practical matter, the technical competence of DOE or AEC or ERDA has always been relatively lower, so they needed the national laboratories to be really extensions of their own technical ability.

And that has gotten worse. DOE has lost a lot of technical talent they had, so they are really very dependent on the labs. And that's not necessarily bad. It's not necessarily good. It's one way to do business. But you shouldn't ignore it. DOE can in fact not simply throw out the labs and substitute Lockheed Martin or Douglas or the RAND Corporation or never mind what all. I mean, there is an intimate relationship. So there is sort of a tension between are the labs partners or are they contractors. I mean, they're both. But you can't ignore the one or the other.

PARIS: Now when you say relatively low, that would be something like the ONR, where they would have the in-house people?

PANOFSKY: ONR had much better in-house people in general. And in general of course the military, it was quite an innovation after the war for ONR to do what they did. They basically—it's purely historical accident. After the war, the Navy had a lot of money left over for research, and because of the strong incentive after the war, people—the government realized that the academic community, if well supported, was really very effective in terms of turning out things both in terms of science and technology. And so ONR had money left, and had very progressive leadership mainly under a guy by the name of Manny Piori. And so they were the initial sponsors of high energy physics, and they were the initial sponsors of the High Energy Lab here at Stanford. But they sponsored high energy physics all over the country. And then that money ran out and people didn't really feel that it was up to the Navy, of all places, to run high energy physics. So then there was a rather difficult transition from the Office of Naval Research to the then Atomic Energy Commission.

So a lot of the activities were turned over from ONR to AEC. But AEC never pretended to have the in-house civil service scientific competence. And now that the Defense Department always had labs which were direct of—part of the Defense Department. The Naval Research Laboratory and the Wright Patterson Air Force Base—there were lots of captive civil service labs. But this partnership arrangement, with the benefit of historical hindsight, has always been more effective, because it gave—on the one hand, it gave the government people authority to say yes or no to various initiatives, but on the other hand, it meant that you have room for decentralized initiatives. You can, to quote Chairman Mao, let a hundred flowers bloom. You can basically—and then—so the governmental role turns out to be much more effective if it says yes or no to initiatives which are generated by the performers, rather than if somebody in their greatest wisdom decides what this country needs is two more electron-positron storage rings.

So by and large the management of all these things has always been responsive by the government. The government never or very, very rarely—there are very, very few instances in history, post-war history, where the government comes to a university and said, “Wouldn't you like to build another high-energy machine?” or something like that. That's not the way it works. The initiative comes from the grassroots, and people basically say they’re building such and such a thing or doing such and such experiments or whatever is something which I want to do, but it happens to be also in the national interest to do that, so please help.

PARIS: Do you feel then that you can get a fair evaluation or that maybe you have to tailor—not you, but one has to tailor a proposal then, because you're talking to people who aren’t as expert or won’t understand?

PANOFSKY: No. I mean basically—again, there are exceptions for everything—by and large, both—particularly DOE people have been very good in recognizing their own limitation. And so what they do is when there is a proposal or when there is a new initiative, you appoint a review committee and so forth and so forth. I mean, you get the expertise, even if you don’t have it. And as you know, the high-energy physics branch of DOE has what’s called HEPAP, the High Energy Physics Advisory Panel. And basically that system works very well, because even though all these people are colleagues, when they're put on committees, they tend to be, A, objective, and B, try very hard—they're not particularly kind to their colleagues when it comes to these judgments. So that system which DOE has, namely not to have much expertise but get it if they need it, is a pretty good way of doing things. Like all these things, there are failures. Sure, if you write proposals, you have to write them in such a way that on the first go-round, the people—they have to be clear enough that you understand it, but that you're not talking to the world's greatest expert.

PARIS: What about the NSF in relationship to the AEC?

PANOFSKY: Well, it sort of evolved historically. As you know, the National Science Foundation again was started after the war, but it has never been in position to support very large facilities. The largest—and that has a history. They had several large facilities which turned out to be failures. The most famous one was the so-called Moho. It was a big expedition to make a big drilling thing to what's called the Mohorovičić discontinuity. Did very deep drilling to find out what the earth mantle is like. And it overran the budget and they didn't have expertise in mobilizing ships and all that and all that. And it was a bust. And then their record—they operated large radio frequency telescopes and again, that had some overrunning troubles and one mechanical collapse and so forth. So they have never inspired much confidence.

Cornell University is a great exception. They have done a beautiful job in supporting the Cornell High Energy Newman Lab. That has been a success story. So basically, the National Science Foundation has been supporting quote-unquote "small science." That means user groups in high-energy physics and smaller laboratories in other fields and in mathematics and astronomy and so forth. And so it has become sort of a tradition. It's not a huge—you can't justify it by logic very much—that National Science Foundation does not support really very large user facilities.

PARIS: And is their expertise—it works the way the AEC does? When they need someone, they pull them in?

PANOFSKY: Yes. They have advisory panels and all sorts of user groups, but they have quite good expertise in-house in those different fields. But on the other hand, they rarely had to make large decisions on sort of large departures of starting major large new ventures. Most of the decisions are either decisions whether to support relatively small initiatives or simply what are called level of effort decisions.

PARIS: And Paul McDaniel and Bill Wallenmeyer, did you have a problem with them in terms of expertise?

PANOFSKY: Oh, no.

PARIS: Did you feel like they pretty much understood—? What was your relationship with them?

PANOFSKY: They're quite different. Paul McDaniel was what now would be director of energy research, and he didn't really know very—he was relatively inexpert in many of these things. He was sort of a nice guy type. I knew him reasonably well. I once gave some lectures in Australia, and so did he, and we were together. Wallenmeyer was quite a capable accelerator physicist. He had never done any particle physics. Well, he had done some cosmic ray work in his past, but mainly he worked at MURA. He knew a lot about accelerators, actually, but of course then as time went on, his main expertise is more of the people than of the—I mean, he knew the community extremely well, and he knew sort of who had a record of getting things done and who had a record of talking nonsense. So he's very good. And he had sort of this bluff way of making decisions and promoting the subject. He was good. And of course then he retired and became a president of the Southeastern university association, the contracting consortium for the CEBAF accelerator. And he is now fully retired. And what can I say? Sort of interesting thing—he was a graduate student of a Chinese physicist at Purdue who then became the founder of Chinese high-energy physics. Sort of an amusing connection.

PARIS: How about—Hosmer and Holifield were two senators.

PANOFSKY: Who?

PARIS: Hosmer—Craig Hosmer.

PANOFSKY: Yeah, I know him.

PARIS: Yeah, OK. And Holifield—Chet Holifield.

PANOFSKY: Yes.

PARIS: Both senators around the time that—

PANOFSKY: They were congressmen. They were not senators. They were both in Congress.

PARIS: They were definitely in Congress, yeah. Can you comment on the interactions with them in relation to the proposal?

PANOFSKY: I had very good relations with both of them. They were in Congress during the days when Congress was organized differently, when Congress had what's called the Joint Committee on Atomic Energy. That got abolished. That is a tremendous anomaly in American bureaucratic ways of doing things. After the war, Congress organized what's called the Joint Committee on Atomic Energy, where it was decided that in those days, the first in 1945 was the so-called Atomic Energy Act, which among other things established the "born classified" provision, and there was a lot of secrecy. So therefore rather than having two committees—because right now, in order to do anything, usually four committees have to act. I mean, there has to be the authorizing and appropriating committees in the House, and the authorizing and appropriating committee in the Senate. And so because of the secrecy and all that, that was believed to be in urgency too complicated. So all that was simplified for atomic energy for the old AEC, and the Joint Committee on Atomic Energy was created. And Holifield and Hosmer were the members of the JCAE who took a great deal of interest in basic science in the Atomic Energy Commission.

But they also took a lot of interest in atomic energy and nuclear reactors, nuclear power, and so forth and so forth. They were very active, and they learned a lot. I mean, they were not physicists, they were not technicians, but they really learned—did their homework. And Holifield, I knew very well. He died about a couple of years ago. In fact I have a book—I have his biography here, which his wife—well, his wife died before that, but his children sent it to me, because we were good friends. And that arrangement with the Joint Committee on Atomic Energy was criticized a little bit because it was believed that the critics felt it was too cozy of a relationship between the Congressional organization and an executive branch organization, and that that had blurred the separations of powers between Congress and the executive branch. And to some extent, that's true. For instance, the staff of the AEC wouldn't do anything before clearing it beforehand with the staff of the Joint Committee on Atomic Energy, so that—for instance, when SLAC was authorized, we had a lot of interactions. I mean, we had to give a lot of information to the Joint Committee on Atomic Energy in anticipation of legislation. But on the other hand, if the Joint Committee on Energy was happy, then the rest of the Congress would almost automatically go along, because they felt this was too esoteric stuff for them to deal with. So it was an unusual situation. And Holifield—I don't know why you're picking on Holifield and Hosmer. There was quite a few other people.

PARIS: Who else would you—?

PANOFSKY: Well, Senator Anderson from New Mexico was a real powerhouse in there. And in the old days and blah blah blah. But anyway. So it's more the institution of the Joint Committee on Atomic Energy which was unusual. But Holifield and Hosmer did take a real interest in the basic science component of atomic energy, not so much in the weapons and what have you. So it's an anomalous structure in the history of the way the United States government functions.

PARIS: One of the reasons I picked on them—I think it was Holifield who was the chairman of the JCAE.

PANOFSKY: It would alternate between—the chairmanship would alternate between the House and the Senate. So I don't remember whether it alternated yearly or biyearly or whatever.

PARIS: But they were both involved in the hearings for SPEAR—the JCE hearings.

PANOFSKY: I see.

PARIS: So I was curious what their effect might have been.

PANOFSKY: On SPEAR? Not very much. We kept them informed. The important thing is they did not—they were always resentful if things happened which they didn't know anything about. They wanted to be part of the action, if you wish. And so it was always important to keep them informed, or their staff members informed. And I did that. So they kept—both in official hearings and unofficially by keeping them informed what we were up to. But they didn't take any—as I recall, neither of them took any strong positions for or against SPEAR. They were sympathetic what we were doing and they appreciate that we're getting work done and doing good, and all that and all that.

PARIS: Let me ask you a more general question about the relative difficulties of being involved in something like PSAC or HEPAP. Was it easy to be reimbursed? Is there a lot of red tape involved in that?

PANOFSKY: Reimbursed? You mean—?

PARIS: Reimbursed for staying—flying back and forth to Washington, or—?

PANOFSKY: No.

PARIS: It's easy?

PANOFSKY: No. We didn't get paid for anything. We just got reimbursed. But on the contrary, the bureaucracy with getting reimbursed was relatively trivial. They would actually, if I remember, pre-issue vouchers for getting airline tickets, so you didn't even have to—first you get it out of your own pocket, and then get reimbursed. No, no, the bureaucracy of travel reimbursement and all that kind of stuff, that worked fine.

PARIS: So serving on a committee wouldn't have been a big, huge pain?

PANOFSKY: No. No, there was no problem. It was more complicated, because you couldn't—airplanes would stop everywhere, and so it would take long, and all that and all that. But I mean, no, the bureaucracy was trivial. That all went simpler than it does now. Now, suddenly some auditor will challenge whether something or other shouldn't have been cheaper or whatnot. They didn't do anything of that kind. That was zero trouble.

PARIS: And lastly, I wonder, are you still in contact with Abbadessa?

PANOFSKY: No. In fact, I don't even know whether he—is he alive?

PARIS: I think so. He has an address in Maryland. I'm working on him.

PANOFSKY: I see. He's a nice guy. He was a nice guy. No, the answer is no. I'm not in contact with him. He's a nice guy. He's sort of a tough, pragmatic character.

PARIS: Oh, I do have one leftover question. You talked about the users on SPEAR. I think you talked about O'Neill's group being on the other pit, right? On the outside users' pit at SPEAR. Do you feel that SLAC's accommodation of outside users in the early days of SPEAR—at SPEAR and in the early days of the linac was adequate? Was there any controversy about it?

PANOFSKY: Oh, there was controversy, right? There was always—people were always—there was always a problem—is SLAC a quote-unquote truly national facility? I mean, there were truly [inaudible]. And we had the formal structure of program advisor committee and scientific policy committee and so forth. But people got always confused between two issues, namely did we really do everything to accommodate users, or to what extent did the technology of how to do work at SLAC make it harder for users to participate? Now, this is a technical issue. Many experiments on proton accelerators are what I like to call building block experiments. There's a target, there's a beam, and you assemble a relatively small piece of hardware detectors and do an experiment.

At SLAC, that basically doesn't work. SLAC, both at SPEAR but also SLAC itself, has always been what I call facility-centered. Now namely that you build a big beast like the big spectrometers for electron scattering or large bubble chambers or the large solenoidal detectors in the interaction region of SPEAR. And then there were many users. Now history has basically caught up. I mean now everybody does business that way. Today, almost all experiments in the colliding beam machines in the world are those huge collaborations. Now, but during the time when SLAC started, it was very exceptional. It was much more common to have a very small group, set up an experiment built out of building blocks, if you wish, do the experiments, and then take it apart again and go home. So the fact of being quote-unquote facility-centered, we were ahead of the time, if you wish.

But this was not a matter of policy or social structure or something. It was a matter of necessity. Because the standard way of doing things simply didn't work. At SLAC—SLAC is a pulse machine. The pulse length is only one microsecond, the repetition rate up to 360 pulses per second. And therefore what is known in the trade—you know what I mean by coincident [?] experiment? Where you have counters, and if they're in time coincident [?]—don't work here, or work very badly, because since the machine—the pulse is only on a microsecond, almost everything is in coincidence. So therefore, to be more technical, the ratio of accidental to real coincidences becomes too large. And so the standard way of doing—so you need large spectrometers to sort things out, or you need these very large 4Pi detectors and so forth. So we were always facility-centered.

So when I started SPEAR, we had two interaction regions, and the one interaction region was occupied by the large detector which Richter designed, and as a matter of policy, we threw the other side open to users. But the users in the other side were never very competitive. But the reason—that had nothing to do with social problems or politics or even human relations or what have you. It had to do with the fact that relatively small solid angle detectors simply didn't have enough counting rate. And so that the dedicated experiment for a specific physics channel like the one which O'Neill put there, and then there was one by Hofstadter, there was one by a group from Argonne—they never produced a heck of a lot. And so no, we made a deliberate effort to keep that second area open for users. But you just couldn't be—

[End of audio]