An Oral History of

STANFORD PENNER and STANLEY CHODOROW

On May 5, 1999

CHODOROW: Who was killed by his books. He reached for something on a high shelf [*crosstalk*] and it all came down on him and buried him. It's the way to go.

PENNER: Well, it is a very good way to go if you're a Talmudic scholar, I'd say. That's probably true. You've got a lot of nice Zeus things around here, haven't you?

WESTBROOK: Yes, yes.

PENNER: Are these the originals?

WESTBROOK: These are the originals. These are mock-ups for the books—

PENNER: Well, if the university ever needs money you can sell those.

CHODOROW: Well, that was the idea— Do you remember the state controller, I think it was under [President Ronald] Reagan, who proposed that if the University of California needed money, they should just sell the rare books nobody read anyway. [laughter] I'll never forget it.

PENNER: It sounds like one of [Edmund Gerald] Jerry Brown's suggestions, I'd say.

CHODOROW: Yeah, yeah, another one of his. That's true. Well, let me explain what we are doing, what this project is, and what our at least initial focus will be this morning. This began with a request from Jim [James R.] Arnold, who had arranged for himself and Keith [A.] Brueckner to come in to talk to Brad [Bradley D. Westbrook] about—on tape—the origins of the chemistry and the physics departments. And Jim asked me to come along, because as a historian he thought I would help keep the questions on track—the discussion on track. And it was a fascinating discussion, and led to a discussion between Brad and me about continuing this and really developing a record, not of the sort that was represented in that history of the university [*An Improbable Venture*] that Nancy Anderson did which I do not think very highly of, but rather on the intellectual side. What was the intellectual vision? How did it relate to the state of the disciplines at the time or the fields? And in your case, you're talking about a broad collection of disciplines really. What was the intention of the founding chairs? How did early

recruitments affect the realization of that vision, the intellectual vision? And the period we're concentrating on without keeping very strict limits on it is from roughly 1960 to about 1975. Sometimes we get up into the early '80s, but it depends on how the departments developed and whether major changes of orientation were taking place. And of course, in AMES [Applied Mathematics and Engineering Sciences] that was true for quite a long time.

PENNER: Not in that time frame; it happened later, really. What about Herb [Herbert F.] York and Roger [R.] Revelle, above all? Do you have something from him?

CHODOROW: Well, we have an oral history that was done by Roger before his death, and that was very important. And also, of course, he reverberates through a lot of the conversations we've had. We've had two conversations with Herb: one that was about the general campus, and the other about the medical school, which was of course, a very important—[*crosstalk*]

PENNER: That's an interesting—

CHODOROW: —and interesting story as well. And then we've talked to Mel [Melvin] Spiro and Joe [Joseph R.] Gusfield, and of course [*inaudible*] and Jim. We are— Gabe [Gabriel] Jackson, who was the first historian on the scene, Roy [Harvey] Pearce—

PENNER: Where is Gabe? Is he in Spain?

CHODOROW: Yes, he's in Barcelona, but he comes to San Diego once a year. And I heard about it and grabbed him. *[laughs]*

PENNER: You grabbed him, yeah. How is Roy doing? Is he alive and well?

CHODOROW: Well, he's alive and he's mostly well. He's a little slower than he was. And I will report that he is fighting all the old battles. He still keeps up part of his energy by living in that embattled past that [*crosstalk*] either was true or it was imagined.

PENNER: And John Galbraith? You must have talked to John Galbraith.

CHODOROW: Well, Galbraith we weren't able to catch before his decline. He’s suffering from Alzheimer's.

PENNER: You know, it's an interesting fact, though. I've seen John a number of times since he retired, and his memory of what happened in the '60s and '70s is very vivid. It's only the later time period—

CHODOROW: Well, then we should ask his wife.

PENNER: I think it would be very interesting for you to—

WESTBROOK: When did you last see him?

CHODOROW: Well, it's been about six months now, and I haven't—

WESTBROOK: Oh, really?

PENNER: Yeah. My wife [Beverly Penner] talked to Laura [Galbraith], and Laura said that things weren't going all that well. So, it may be that it's been rapidly downhill. But I was— I've always been impressed with how well he remembered—

CHODOROW: The early days.

PENNER: —the library battles and everything connected with that.

CHODOROW: We interviewed Mel [Melvin] Voigt and Andy [Andrew] Wright about the origins of the library.

PENNER: But you'll get a different story from Galbraith.

CHODOROW: Right. I'm sure that's right.

PENNER: It would be interesting to try, anyway.

CHODOROW: Well, we'll ask Laura about that.

PENNER: Yeah. Ask Laura and she can tell you whether he's up to this or not.

CHODOROW: But his decline was rapid. He pulled himself out of teaching in the middle of a term because he just found that he was unable to perform and asked the department to relieve him, which they did. They found somebody to take over the course. And since that time, he's not really been— He's declined fairly rapidly, I think. But if he has a memory intact—

PENNER: Well, many Alzheimer's people, the patients have— They remember early time periods very clearly, but—

CHODOROW: Yeah, that's right. The same problem is true of John [L.] Stewart in the arts; he is also suffering from Alzheimer's and, I gather, not doing very well. But we are working our way systematically as much as we can to—

PENNER: Well, but you've got to get these people first.

CHODOROW: And I've got to get some of these first.

PENNER: Yeah, yeah.

CHODOROW: So today I'd love to talk to you about both your own department and the foundation of that department: What brought you here? What the state of the art was, so to speak, at that time as you remember it and what you yourself were trying to accomplish? And also, because you served for a time as vice-chancellor or whatever they called it in those days—

PENNER: I was vice-chancellor for academic affairs. And I was also a member of the selection committee that appointed, or suggested Bill [William] McGill.

CHODOROW: Is that right?

PENNER: Yeah.

CHODOROW: Okay, so that's an important part of the record as well. We want to focus on the ideas, on the intellectual and disciplinary issues, and so I'll let you just start to say how it was you came and what it was that you were trying to do.

PENNER: Okay. Well how it was I came, that's sort of an interesting story that antedates the university by a long time. I was a faculty member at Caltech [California Institute of Technology], and I think it was in 1951 that I was in La Jolla for the first time. My wife and I went riding down to Tijuana and stopped here on the way. And we looked around and sort of said, "You know, this would be the ideal place to live." [laughter] That goes back to '51.

CHODOROW: [*inaudible*]

PENNER: Now, why did I come to UCSD [University of California, San Diego]? Well, there are a couple of really compelling reasons that relate to that. I met Keith Brueckner for the first time, I think, in the late '50s. We served on some Department of Defense committees. And at that time, Keith—as you probably know or remember— was a very vital person, a very strong person with very firm ideas on all sorts of topics. And he told me at these meetings about this wonderful new university that was being founded here. And a couple of years later he went to the Institute for Defense Analysis as director of research and persuaded me to take a leave of absence from Caltech to run what was called the Research and Engineering Division at the Institute for Defense Analysis. And I had a good deal of professional contact with Keith, and both of us had a great deal of professional contact with [Robert] McNamara's "whiz kids", who in a very real sense were our bosses, we worked for them; they assigned tasks to us. Well, I never intended to leave Caltech. You know, that's the kind of thing people just don't do; once they're there, they stay. It's, in many ways, an absolutely ideal place, except for the smog, which was horrible in the late '50s. One of my nearby neighbors was a leading exponent on defining the cause of smog in the Pasadena region. And I had walked with him many evenings and he pointed out the local firing or burning of debris and wood as, you know, prime cause of smog. And then he got involved with the cars—and of course, he was right. This was a very unpleasant part of living in Pasadena. People have different sensitivities, and certainly one of my family members was acutely sensitive to the smog. So that was a great inducement, that together with the early '50s experience when this place was really ideal and pristine. So, when Keith started to talk about La Jolla, that rang a bell, "Oh, that's a great place to go." And following that discussion with Keith I came here and met Herb [Herbert F.] York. We had a lunch down at Scripps [Institution of Oceanography] overlooking the ocean. And after that I talked a couple of times to Roger. That was, I think, as for many others, a really inspirational experience. I mean, Roger had a great vision for the campus. He wanted to build nothing less than the greatest graduate school in the United States. He didn't talk much about undergraduate education. I think in the early days it was his hope that this would be primarily a graduate school.

CHODOROW: Right.

PENNER: And this in turn was, you know, where I'd grown up. I mean, Caltech was primarily a graduate school. We did undergraduate teaching, but 90% of our time was spent with graduate students and doing research. Well, Roger was a very persuasive encounter. I mean, he certainly convinced me that this would be an absolutely ideal place to build what

should be the greatest academic institution in the country. And that, in a nutshell, is the primary reason why I came. And when I came, I had a great deal of leeway in building up what was called originally Aeronautical and Mechanical Engineering, and then Aerospace and Mechanical Engineering Sciences—I want to emphasize that. I did not visualize the department originally as a classical engineering department at all. It was to be a collection of all applied science and we needed a name for it. And the name, as you know, was changed by my successor to Applied Mechanics and Engineering Sciences. By the time you use a name like that, you encompass all of applied science. And there was strong support on the campus for having not engineering, but applied science. I remember talking with some of the other people who were here early and concerned about how the campus would develop about just this issue, particularly Jon [Jonathan S.] Singer. And he thought that was great, that was the way to go. Of course, there was another reason why that was the way to go, because in the early '60s when I came, the primary issues were the Cold War. The Cold War, in a sense, was a driver for very much basic applied science research. And the Cold War as a driver certainly did not recognize disciplinary boundaries. And the emphasis was, in fact, on engineering science, leaving the engineering to the industrial organizations where the people went afterwards. So, for this reason, I think, at that time, engineering science was the right way to go.

CHODOROW: It was peculiarly academic part of the enterprise: is that the way you would like it?

PENNER: It was very much the academic part of the enterprise. That is, the research we did was interdisciplinary and was almost indistinguishable from what was being done in psychics and chemistry or biology, depending on the topic that we picked. And the hope was that we could build here long-term something like the Caltech division of engineering, which also never had disciplinary boundaries. And of course, I am personally strongly opposed to the idea of proliferating departments and all of the administrative mischief that goes with having ten or twelve people, and a chairman, and a vice-chairman, and a secretary, and a business manager. I mean, this to my way of thinking has always been a horribly inefficient way of doing business. At the Institute for Defense Analysis I ultimately ended up with something over seventy senior engineers and scientists. It was all one group; we covered everything under the sun. I mean, that was the way we tried to do our business then. And I think it was really quite a successful activity at UCSD also. The very first person I hired really as an applied mathematician is Johnny [John Wilder] Miles. And Johnny, as you know, was vice-chancellor later on. But he is a member of the National Academy of Sciences. He obviously doesn't fit the mold of a classical engineer,

166 although he's done some very fine engineering in his day as well. And I think that many of the
167 early people who I brought here were really of that type: they were basic researchers with an
168 applied bend. The early group was very much multi-disciplinary. I mentioned Johnny Miles in
169 applied science, I brought in Paul [A.] Libby. Paul Libby was working with one of the country's
170 most distinguished aeronautical engineers at Brooklyn Polytech [Brooklyn Collegiate and
171 Polytechnic Institute] Antonio Ferri—who happens to have been a very close friend of mine. So
172 that covered— And a very practical engineer. That's a completely different kind of a field that
173 was really started with him. And then Bert Fulman was a colleague of mine at Caltech, came
174 down one day with Ben [Benjamin W.] Zweifach. Bert had started out in aeronautical
175 engineering, but by that time he was going into bioengineering and he brought his physiologist,
176 Ben Zweifach. And I remember the three of us had an absolutely wonderful time in my office
177 discussing what a great thing it would be to include in engineering science this disciplinary
178 activity. So, Bert Fulman and Ben Zweifach came in early. And that's a completely different field,
179 as you know.

180 **CHODOROW:** Yes.

181 **PENNER:** It's developed into a really major national enterprise. Now let me think about
182 some of the other people who came in.

183 **CHODOROW:** Did Ben or Bert come in on the early medical school FTEs [Full-time
184 equivalent] that were given to the department, or they were in the department?

185 **PENNER:** They were in AMES [Aerospace and Mechanical Engineering Sciences]; they
186 had AMES appointments. But from the very beginning they collaborated, mostly with the
187 medical school, because the work they did was really medical engineering science, if you like.

188 **CHODOROW:** Now my understanding is that their approach to bioengineering was the
189 application of classical engineering principles to organic systems. [*crosstalk*]

190 **PENNER:** Well, it was really more than that. You'll remember that Ben was a physiologist,
191 so his approach was that of the classical physiologist trained in the medical school.

192 **CHODOROW:** Right.

193 **PENNER:** Whereas Bert Fulman was in aeronautical engineering; his forte was solid
194 mechanics, and structures, and fatigue problems—that's what he worked on.

CHODOROW: Right.

PENNER: But— And he also knew fluid mechanics; you can't be in aeronautical engineering without knowing fluid mechanics. That's the all-pervading discipline. That's a field in which Libby worked, and a field in which Miles worked, and a field in which I worked in a very real sense. So, I think that the marriage between classical physiology and applied engineering science is what really brought together bioengineering as a new discipline. And we covered many other fields with the very initial group. And one of the people whom I brought in at the very beginning was S.C. [Shao-Chi] Lin. He had been trained at Cornell University and he was a laser specialist. As a matter of fact, he worked on excimer lasers: that's a special kind of laser that allows you to construct lasers at very short wavelengths where you can do all kinds of things, including manufacturing semi-conductors. So, he was also trained in fluid mechanics. He had worked with Arthur Kantrowitz—that's someone you may have heard of. He founded the Avco [Everett Research Lab (AERL)] research laboratory, which became sort of a summer mecca for all of us. For many summers we'd collect in Boston or Everett [Massachusetts] and work with Arthur Kantrowitz on problems closely related, actually, to the Cold War: ballistic missile penetration and defense and things like this. I remember one summer I was there I shared a tiny office with Herman [?] Kahn. I don't know if you knew Herman Kahn.

CHODOROW: No, I didn't.

PENNER: But he was an enormous man, and when he was in the office I was sitting in the doorway. [laughter] There was not room enough for me to be in the office. But Arthur Kantrowitz had a very distinguished stable of regulars during the summer activities, which involved many of us from here, like S.C. Lin and myself.

CHODOROW: And he was supported by contracts with the Defense Department [United States Department of Defense]? So, he was able to support these activities?

PENNER: Yes. He was a very influential man at the AVCO Corporation, and he had founded this AVCO Research Laboratory. One of the regulars there was Hans Bethe—he was always there during the summers—and Edward Teller would come in and out. And Teller and Bethe would have some tremendous arguments, as you can imagine. But there were very many other very distinguished—usually physicists and applied mathematicians. And in the group, some of us were there: like Lin was there every year and I was there every year. So, we started out with this applied science group that already had four completely different disciplines. And

then I knew Charlie [Charles S] Draper at the Lincoln Laboratories at MIT [Massachusetts Institute of Technology]. And we had decided early to get into—what shall I call it? — I should say systems and control engineering. And I asked him who would be a good person to hire. We were all very old then; I think I was about 41 [years-old], the others were ten years younger. [laughter] This is how Alan Schneider came here. He was an expert in controls and guidance. Although many of us, of course, changed fields later—many as dramatically as I did. I mean, I've been in the energy business almost exclusively since the middle '70s, so— We didn't stay where we began. That was, of course, sort of guaranteed from the kind of group that we assembled. Two of my former students came: Dan [Daniel B.] Olfe, and his expertise was in radiative heat transfer—again, a very different field in many ways—and Forman [A.] Williams who had been my Ph.D. student at Caltech, and he works in combustion. So, it was in the true sense of the word, I think, an agglomeration of people covering very many different disciplines.

CHODOROW: And the thing that— The intellectual thread was thermodynamics.

PENNER: I really wouldn't put it quite that way… Well, as you say it, you may be right, actually, because I don't think anyone was ever brought in who wouldn't have— Well, Schneider would not have— I mean, the controls [?] engineering is—[*crosstalk*]

CHODOROW: Very different.

PENNER: —far enough afield, so that— But for most of us, I think that would be a true statement, yes. Then we wanted to cover chemical engineering in some way. And, you know, this was a very informal beginning for the department. I had a very good friend at Princeton University, John [B.] Fenn, and when I ran into him, he kept telling me about this very bright, young fellow who was getting his Ph.D. with him: David [R.] Miller.

CHODOROW: Right.

PENNER: *[laughs]* That's how he came to UCSD. And working in a closely allied field was Carl [H.] Gibson from Stanford [University]. I took Roger Revelle's suggestion very literally: "When you put together this applied science department, remember, you want to cover everything." And that was certainly my predilection also, and I think we did. The split-up that's occurring now and dividing the departments according to more generally recognized activities simply reflects the fact that the core group was very much multi-disciplinary. And of course, I

think it was an exceptionally able group. I should mention Bill [William] Nachbar who was one of the early people who came here. He worked in solid mechanics.

CHODOROW: Right. He had come out of civil engineering, didn't he?

PENNER: No. Bill Nachbar at the time— He had been trained at Brown University. Brown University is—was—then the mecca for applied mechanics: structures, fatigue problems, materials problems—that was it. And the grandfather of that group was Bill [William] Prager; he was certainly the acknowledged expert in the United States in this area, and he was at Brown University. And I knew Bill Nachbar because he was working at Lockheed [Lockheed Missiles and Space Company] and I was a consultant at Lockheed where we worked, again, in fluid mechanics-related topics together. After Bill Nachbar came here, we wanted to take advantage of the fact that we had an entry to the senior people in this materials and applied mechanics area. And during the middle '60s I think the record will show that we brought Bill Prager and Erich [Max Erich (aka Eric)] Reissner from MIT here.

CHODOROW: Yes, I remember that.

PENNER: So, we had two very distinguished people in that field, which has by now become an extremely active area. Sia Nemat-Nasser was one of the young people we brought in; he's here now, and he's done a wonderful job of building up the interface between material science and structures.

CHODOROW: That I suppose led to Gil [Gilbert A.] Hegemier—

PENNER: Gil Hegemier came with a very early group. I knew him as a student at Caltech. He had been working, I think, [George W.] Housner and [Donald E.] Hudson—that group—in structural engineering at Caltech. And we wanted to have someone in that field.

CHODOROW: Right.

PENNER: And again, I think it was in this case Housner's recommendation that brought Gil Hegemier here. He was here very early, also. So, the first twenty-three or so people that were hired while I was chairman, I think, or very shortly—no, I think while I was chairman—covered essentially all of applied science and engineering. What was left out was electrical engineering and computer science, which Henry [G.] Booker founded [then known as Department of Applied Electrophysics], as you know. Henry Booker himself was trained as a physicist. His really

distinguished work was in atmospheric probing and radar and activities like this. These were very much complementary activities. There was overlap because our systems and control engineers could as well have been in Henry Booker's group. Although we never had any problems; Henry and I got along famously and we used to discuss the people we were going to bring in. I think Henry did a very good job of building a nucleus around which electrical engineering and computer science has been growing at UCSD. I see that Irving Jacobs is giving the plenary lecture, if you can call it that, at the graduation.

CHODOROW: Right.

PENNER: And the chancellor has a few misstatements in his biography; Henry Booker isn't even mentioned. I was vice-chancellor at the time Jacobs was hired, and I had the reception at my house for Jacobs—who came in as an associate professor, by the way. He didn't come in as a full professor. And he was here only a short time. I think his impact on the electrical engineering and computer science was certainly not as great as it was stated by the chancellor.

CHODOROW: His impact on me, however, was enormous because at a little-league game where our sons were playing, he turned me toward computing.

PENNER: Oh, he did?

CHODOROW: And I was the first humanist on this campus to go to the computing center and be put on the boroughs [?] with a big 12th century law project.

PENNER: That's wonderful.

CHODOROW: And so, yes. He turned me from the traditional path of the humanist into a very different course. It was very—

PENNER: Then he had an enormous influence on many of us because we bought Qualcomm [Corporation] when it was cheap.

CHODOROW: Yes, exactly. [laughter] That's right. I remember him as a faculty member in my early days, but he wasn't here very long because—

PENNER: No, he was only here about three years, I think.

CHODOROW: Yeah, I think that's right.

PENNER: Well, let's see. What else should I talk about?

CHODOROW: One question I have— Brad, you go ahead and—

WESTBROOK: Well, you're painting a picture of this early construction and it seems very smooth. I'm wondering if there were any failures? People you did not get that you went after? Or people who did not work out?

PENNER: We had very few casualties, actually. Bob [Robert?] Pavula in controls was an early entry. He was hired as an assistant professor, and he left because he didn't like the money we paid him. He could go into industry and double his salary, so he left. Dave [David D.] Sworder came in fairly early from UCLA. He was a very fine controls and systems engineer until he was transferred out of AMES. He worked, you know, very harmoniously and effectively in the group. I think very highly of him. I think he's a very competent person. We didn't have very many casualties, maybe because the environment was so enormously benign in another way. When I came from the Institute for Defense Analysis, I had a small contract which was something like $4.5 million dollars per year. And it dealt with atomic and molecular physics—that's really what it was—but the interpretation was so liberal that, you know, just about everybody was working on it and they did whatever they wanted to. I had to defend what they did to the contract monitors. Now, $4.5 million in 1964—that's like $20 million today. It's not a small amount of money. We had that, I think, until the middle '70s so people had good support. We never really had any problems, except the problems connected with the Vietnam War, as you might imagine. Those were real problems. Keith [A.] Brueckner and I and some of the others became real caricatures at the time. We never did any classified work here—that was absolutely taboo. I mean, that's the kind of thing I would never have tolerated. And we never split salaries between university money and extramural money; this was done very often at Caltech. And I saw the ups and downs that went with those funding cycles. I said— As a matter of fact, that was one of the first things I said to Keith: "If I come there as chairman one thing we don't do, we don't ever split salaries between university and extramural money because we want the people to feel that they have secure funding at all ranks." So, we never did that.

CHODOROW: They did it in physics.

PENNER: They did it in physics, but they never did it in AMES.

CHODOROW: And in the early '80s we paid for it.

PENNER: Well, we paid for it a couple times while I was at Caltech. And one experience like that will really cure you.

CHODOROW: That's right. I think it cured UCSD actually, because we had to work quite a lot on FTE resources in physics, and it stopped physics cold for a number of years until Harold Ticho came and began to build it again. But for a period when John Miles was vice-chancellor and I was chair of the [Academic] Senate, I remember, we were spending resources on absorbing physicists whose contracts would no longer support their salaries or half their salaries. It was a very painful period.

PENNER: Well, it's not a good thing to do. And I learned that in the middle '50s when I was at Caltech, and I saw some of our very best people who had to split appointments were really hanging by a thread. It was a question of whether they could be kept or not. One experience like that, as you say, is enough to cure you permanently.

CHODOROW: Let me ask you a question about the way the department— It's a very broad-gauged group in terms of their disciplines and their specific subjects of research. How did the group operate when they had to make decisions—personnel decisions—both about hiring someone and about promoting people? Because that's a common activity.

PENNER: There was never a problem in that connection. I mean, the people knew each other far better than the people know each other now in different academic engineering departments. We had common seminars. The group was small—that was a big advantage. For some years we didn't have any undergraduate students, as you remember.

CHODOROW: Right.

PENNER: And that helped the graduate programs enormously. And we had this common funding; that certainly helped a great deal, so that there was some kind of a research connection even if people were doing completely different things.

CHODOROW: So that held you together.

PENNER: And then later it became clear that— When the undergraduates came in in substantial numbers it became clear that it would be a good idea to have research organizations—ORUs [organized research units]—in which we could do the kind of research on an interdisciplinary basis again with communal support. This was also started by Keith. Keith

was, I think, for maybe a year or so the director of IPAPS: the Institute for Pure and Applied Physical Sciences. And I took that over after that. I was director for quite a long time; four years or so or five years, until my interests really changed. I should say that my first job after the Ph.D. was working at the Esso Research Laboratory for Exxon. So, I was in the energy business in a very real sense from the very beginning. And I had a sabbatical leave in '72-'73—I had a Guggenheim [Fellowship]—and we went around the globe. It was a wonderful opportunity to talk to people everywhere and find out what they considered to be the primary coming issues. And when I came back from that—at the time [William D.] McElroy was chancellor and Paul [D.] Saltman was vice-chancellor—we had a discussion, the three of us. "Well, what are the big issues? What should you be doing?" I said, "Well, the only thing that we ought to be doing here is to set up an energy center, because that's going to be where the real big problems are." I must say that McElroy and Saltman were both very enthusiastic about doing this. So that was the end of my activity for IPAPS, and I became director of the [UC San Diego] Energy Center then, which was going here until I retired. It's still going now; it's got a slightly modified name [Center for Energy Research].

CHODOROW: Right. One of the things that happens with those research institutes is, in fact, they evolve. They don't come to an end very often. That's a very rare thing. But they do evolve, as its research interests and its leadership changes.

PENNER: As leadership changes. I changed the name from Energy and Combustion Center to Center for Energy and Combustion Research when it was clear that [Forman] Williams was going to succeed me.

CHODOROW: Right.

PENNER: He doesn't work on energy but he works on combustion, so that was a better label. I think now that's likely to change again. The combustion may be dropped as a separate activity. And I think if [Robert] Conn takes it over, he'll probably prefer the name Energy Center.

CHODOROW: Right.

PENNER: Again, because it's more descriptive of what he does. Although it's not the kind of energy center I had. Fusion energy is a little bit different. It's plasma physics; it's something completely different, really. So, this, I think, was a very important development for the campus because our energy center was probably the first in the country. Within a year or two after that

there was one at [University of California,] Berkeley, and there was one at [University of California,] Davis, and they started to grow all over the place. But I think it's greatly to McElroy's credit that he realized very early and pushed this idea very effectively. You know what a job it is to get an organized research unit: it's a two-year ordeal. *[laughs]*

CHODOROW: That's right.

PENNER: But he said, "Don't worry about the name. I'll get you the name. Just start working." So, he was very much with it, I think, in many ways. This unfortunate disagreement between Saltman and McElroy hurt the campus a great deal. But the two together were a very effective pair; I wish they had gotten along better later.

CHODOROW: What— Would you say something about when you served as vice-chancellor and what was going on? The role you played in developing the campus at that time?

PENNER: Well, let me start with— I was through as chairman. See, I came in '64; I was chairman until '68—for four years. And during the last year of that term we were looking for a new chancellor and a committee was set up. I think Bill [William] McGill was chairman of the committee, if I remember right.

CHODOROW: He was chair of the Senate, I think, as well.

PENNER: He was chair of the committee, also, that selected Bill McGill. I remember that well.

CHODOROW: That's an unusual—

PENNER: Well, no, it's not unusual. That's the way things are supposed to be.

CHODOROW: Classic. *[laughs]*

PENNER: I thought history is rewritten by succeeding generations who run the show; isn't that correct?

CHODOROW: Right. That's partly right. *[laughs]*

PENNER: Well, I was a member of that committee, and we were all very happy about Bill McGill's selection. Bill McGill had very many attributes that made him an almost ideal choice. A

very persuasive fellow, very personable. He could communicate with almost anybody at any level. And I think he just had the right convictions and principles to make him bend just in the right direction as time required. And he, in turn, convinced me to become vice-chancellor for academic affairs. I was an institute director at the time, I had enormous contracts; so, I was vice-chancellor only half-time. And I never— I told Bill from the very beginning, you know, this is a job I'm going to do for one year, but I'm not going to— I'm too young to quit research. I was still not forty-five at the time. Oh, no, I had past forty-five—and I think that's a little bit too young to become a full-time administrator in a university. That was a pretty turbulent time during the Vietnam circus. I think [President William Jefferson] Clinton's smart enough to put ground troops into Kosovo because we'd have the same zoo over again if he did. Well, we were certainly misguided and misinformed on many issues then. The idea was prevalent that it would be easy to win, that we had the wherewithal to win. But we certainly never had the conviction to go in there and do the horrible things that would have been necessary to win. I think no one really counted on the Chinese getting involved as effectively as they did on the other side. It was a big mistake, in retrospect. That's clear. It was a terrible mistake. And we paid quite a price for it as a country, and we had quite a zoo for— One of my prodigies, Sia Nemat-Nasser was standing on Revelle Plaza giving lectures against the government money that we were bringing in for the war effort. He was being supported under that guard by my contracts. I thought that was very amusing, actually. [laughter] You ought to ask him about that.

CHODOROW: I will, I will.

PENNER: We're good friends, though. I mean, don't take that the wrong way.

CHODOROW: I've known Sia many, many years.

PENNER: Well, we're making a horrendous mistake now, maybe, in Kosovo as well. I don't know. I don't know how that's going to come out. This idea of making peace by killing large numbers of people on the ground is not very appealing. I always thought we should have trained a few James Bonds, send them in and do the necessary people in. It would be much cheaper. I don't know how we're going to get out of that Kosovo mess. It's horrible too.

CHODOROW: Well, when you were serving as vice-chancellor, did you serve, in fact, for one year?

PENNER: I served for one year, yeah.

CHODOROW: And — Were there any— In 1968-69—

PENNER: I believe that was the time.

CHODOROW: —that's right—which was my first year here as a junior faculty member, there were efforts at that time to try to construct a department of political science. I think that Mel Spiro and Joe [Joseph] Gusfield had already been appointed.

PENNER: I believe so.

CHODOROW: One of them, at least, had arrived.

PENNER: And I was also very much involved in setting up as vice-chancellor the original art department at UCSD. That must have—

CHODOROW: Oh, Paul Brach.

PENNER: Yes, my good friend Paul Brach. He was a wonderful man. Did you know him well?

CHODOROW: I knew him very briefly, because he wasn't here very long. But he established the department.

PENNER: He was really a great man. I mean, I'll never forget, at a cocktail party one evening we were talking about classical art and modern art. And he put his arm around my shoulder and walked with me in the corner and he said, "You know, I want you to know it's a big put-on." [laughter] This is before he went to the California—

CHODOROW: Institute of the Arts.

PENNER: The Institute of the Arts, yeah. He never took anything very seriously. But he was a very fine artist in his own right, too. And he brought good people here. He used to get very upset with me because, you know, there’s a dumb engineer pretending to know something about art. Actually, I'm an avid art collector. I think that qualifies me. [laughter]

CHODOROW: Well, they never did, of course. Were you also involved in music? Or was music already established?

PENNER: No, I think music had already been established a year or so earlier.

CHODOROW: And theater as well?

PENNER: Yes. Now, let's see. The chairman of theater was Molli [Wagner]—Molli was his wife.

CHODOROW: It's Arthur Wagner.

PENNER: Arthur Wagner, yes.

CHODOROW: Everybody remembers Molli. Molli was very special.

PENNER: She was always a very lively person. The Wagner's and my wife [Beverly] and I were very close friends. We really knew each other very well. I don't think I was instrumental in bringing him here, but we had a great deal of contact during that period and afterwards, too.

CHODOROW: What about—? Well, let's go back and talk a little bit about political science, which didn't get started right away. But I think it was about that period when the effort was beginning to be made. Can you remember anything about that?

PENNER: Let me try to remember what happened in political science. I think the effort was started under [John S.] Galbraith here. And during the year, the turbulent year that I was vice-chancellor, I don't think very much was done with political science. At least, I don't remember. I mean, it's not like Irving Jacobs coming here and we have a reception for him. Nothing like that happened. I remember talking frequently with Joe Gusfield at the time and with Mel Spiro, but they must have come earlier also. I think anthropology was here already.

CHODOROW: It was— I think he arrived to start the department but had made a couple of recruitments before he arrived in 1968.

PENNER: Yeah. So that was already—

CHODOROW: And Joe was doing the same thing, basically.

PENNER: That was a done deal for both of them.

CHODOROW: Yes, yes. Sure.

PENNER: But I was involved in the art department. I remember looking at the credentials of many of these people and getting a distinct impression that the people in the art department

508 took a very dim view about having a dumb engineer telling them about who to hire as an artist.
509 [laughter]

510 **CHODOROW:** Well, they were the same with this dumb historian, I can tell you, when I was
511 dean. [laughter] What other—? That year of '68-'69 was both an optimistic and a pessimistic
512 year, because when I arrived we still looked to becoming a campus of 27,000 students and
513 growing very rapidly through the century. We were supposed to reach completion in 1998, as I
514 remember, in the original plans, with twelve colleges and a huge faculty. And at the same time,
515 it was the beginning, really, of the time of disruption and controversy over Vietnam and over
516 research.

517 **PENNER:** And that hurt the campus a great deal for a long period of time, because there
518 was retribution on the part of the research funders against the universities, as you know.

519 **CHODOROW:** Right.

520 **PENNER:** And this type of composite grant where the local person could do whatever he
521 wanted with the money, that disappeared. There was much closer supervision of the funds and
522 people and much closer direction on what they could and couldn't do, and the amount of money
523 was greatly reduced. So, we paid a price for that for probably a couple of decades. It became a
524 bit of a problem. Because since I switched fields completely and got into the energy business, it
525 didn't affect me very much because the energy business, beginning with the middle '70s—

526 **CHODOROW:** That's right, yes.

527 **PENNER:** —when we had the first oil crunch, that became, you know, the sacrosanct
528 activity.

529 **CHODOROW:** Right.

530 **PENNER:** Bill McElroy used to go around and every time I saw him he says, "Weren't we
531 smart to get into that before?

532 **CHODOROW:** Yeah. Just before the crunch.

533 **PENNER:** Right before the crunch hit. I mean, we were all set up to take advantage of it.
534 And I ran, for a very long period of time, just about every major study in the U.S. Department of
535 Energy. All the studies connected with fossil fuels, I was chairman of those for— It was called

the Fossil Energy Research Working Group, which had on it—this was the middle '70s or a little bit earlier—typically people from industry and the universities. And the industry people tended to be, you know, VP's or even presidents of the companies. And we went systematically through every large-scale energy supply that was being used: coal, oil, gas, shale oil, and oil from tar sands. I remember taking a whole group once to northern Alberta, Fort McMurray, which is north of Edmonton, where the enormous Canadian tar sands to recover today up to a million barrels a day. That is a fantastic resource. If the prices go above about twenty-two dollars or so per barrel, this becomes, again, a very competitive supply source for oil of which we're never going to run out. You know, I mean, that's one of the great illusions. And in this period of time, I worked typically for the deputy secretary of energy; John Deutch had that job for a while. He is really the one who persuaded us here at UCSD to do this on a continuing basis for the U.S. Department of Energy. This went on from the middle 70's until—

[END OF PART ONE, BEGIN PART TWO]

CHODOROW: Of these big Composite grants**.**

PENNER: From the Department of Defense.

CHODOROW: From the [U.S.] Department—

PENNER: They continued from other agencies.

CHODOROW: I see. So— But— Well, the question— I'll ask the question anyway, you know, as a complete picture: and that is, what difference did it make to the academic life of the institution—particularly in the department, for example—what happened?

PENNER: It made a big difference because people now had to write their own small proposals and they had to go around and scrounge for money. They had to spend a great deal of their time and effort on very unproductive work. And I think if you ask the people today, "What is involved in getting a research contract?" there will be a universal complaint that "I have to just work from one grant proposal to the next." It's a perennial problem. We didn't have that in the '60s. We had a five-year contract that was renewed every year, so we always had a five-year lead time.

CHODOROW: Right.

PENNER: And the only person involved in writing the proposal was I; the other people didn't have to do anything.

CHODOROW: The head of the organizations.

PENNER: Yeah.

CHODOROW: And the— Did it have—? Was there a difference, for example, in the way you carried out publication strategy under that form as opposed to the small grant?

PENNER: The publications were always totally unconstrained. There was never a problem with it. People could do what they wanted to do, provided it was somehow justified to the sponsors.

CHODOROW: Right.

PENNER: And they published the way any college professor publishes—whenever he's got anything ready to go.

CHODOROW: But that— Yeah, that I understood. It's a different question. One of the complaints today is that people have chopped up their research into rather small published segments, partly to prove to the grantors that they are productive.

PENNER: Yes.

CHODOROW: Partly to prove to their university committees and promotion committees that—

PENNER: It's counting papers rather than content. Exactly.

CHODOROW: That's right, that's right. And I wonder whether the transition from the large, composite grant to the smaller, very specific grant had an effect on that development? Or was it not relevant? I mean, you know it could be—

PENNER: I think it probably— I mean, the university and the grantors are really separate entities. And the people in the universities are addressing the university community because they get promoted in the university community, and there is an unfortunate stress on numbers of papers. And that, in turn, describes the reason for—[*crosstalk*]

CHODOROW: This fine splicing.

PENNER: —these tiny partitions which aren't very useful. I think that just money getting tight was the primary reason why it became necessary to spend more and more time on grant proposals. More and more unproductive time. You know, when I was a young assistant professor at Caltech, I had a contract from the Office of Naval Research, which was initially the premium grant agency, long before NSF [National Science Foundation]. And once a year my contact monitor would call me up and he would say— I was working in spectroscopy then; it was very fundamental research on radiation spectroscopy and heat transfer. He called me up and said, "Next year I have"—let's say—"fifty-six-thousand dollars for you. Send me a one-paragraph lie"— that literally is true—

CHODOROW: A one-paragraph lie?

PENNER: "why you need fifty-six-thousand dollars." And then I would spend five minutes writing it up. That was the end of it. And that went on for a decade. You know, I never wrote a proposal at Caltech. It was just managed efficiently like this. This was Frank [?] Isaacson, who many people remember fondly because of the way he protected his investigators. He didn't— He came by once a year, and he'd look over everything and spend maybe a couple days talking to the students. It isn't as though he took my word for anything.

CHODOROW: Right, right.

PENNER: He did his homework very carefully.

CHODOROW: He understood what you were working on.

PENNER: He understood what I was doing and he approved of it, and then he didn't want to bother me with a proposal. He just sent me the money and he told me how much.

CHODOROW: And he was in—? Was there a difference in those days in length of service of people like Isaacson? Because—

PENNER: Isaacson was a lifetime employee at the Office of Naval Research, and many people at Scripps [Research Institute] will remember him for this type of, I would say, generous administration because he was protecting us from wasting our time.

CHODOROW: Very Interesting. Well, nobody protects anybody from wasting time now.

PENNER: No. Nobody does. There's one comment I ought to make. Of the original twenty-three people who came to AMES, well over a third have now been elected to the National Academy [of Science], which is probably a pretty good average.

CHODOROW: Could you comment on that process? And I ask the question because when I was at Penn [University of Pennsylvania], one of the complaints was that there were not as many—there was not a very large number of faculty in the National Academy. Was it a good faculty? When we investigated this, one of the things we found was that the senior faculty who were in the Academy were not very interested in helping their colleagues.

PENNER: That's one problem we've never had here.

CHODOROW: But that is an issue, isn't it?

PENNER: That is definitely an issue. But when you look at the nominations every year, I think all of us here at UCSD, in AMES certainly have been pushing our colleagues. And it takes a fair amount of work to do that, because you certainly can't have an in-grown nomination. I mean, Sia, who should have been elected long ago, is up for election. And the work that I did is all behind the scenes; it's not visible. The nominator is a colleague of his from Northwestern [University]. They're very good people. Actually, he and I had nominated him before, and this time he nominated him alone. I was ready to go in to nominate him alone, but then we communicated and the result of it is the people I had asked who he didn't ask got letters from me with a copy of his nomination saying, "Please write more letters to strengthen the nomination." It takes a fair amount of work. But we have not had on this campus ever, to my knowledge, the idea that anyone should be kept out. We've really been pushing our people. Otherwise, you don't get this kind of number.

CHODOROW: Right. Right.

PENNER: And that, I think, must have been the trouble at Penn.

CHODOROW: That's exactly right. We could not very often get the support of the members of the National Academy on the campus for pushing other—

PENNER: Good scientists.

CHODOROW: —very good scientists. They were very good.

PENNER: They have to do it. If they don't do it, you know, you can be the world's greatest scientist and nobody will bother.

CHODOROW: Right.

PENNER: Someone has to do the work; that's just it.

CHODOROW: Do you have anything else that you think we've missed? Anything else—

PENNER: No, I don't know. We've— What have we—? We've spent almost an hour here?

CHODOROW: That's right.

PENNER: No, unless you have questions—

CHODOROW: Brad, do you want to add anything? Or is that good? It's— This has been very useful, because one thing you will find— I'll make an observation. Two things have become fairly clear to me: one is that the departments really differ one from another quite a lot in their approach to early development. Yours is a big, multi-disciplinary effort, in effect, though with a certain link, which I see in thermodynamics.

PENNER: Well, the link— Not universal, because certainly the controls people and the people who are doing laser development, they don't quite fit that mold.

CHODOROW: No, but they— There was that one commonality, at least. And it was a very high-level one. It was at a very high level of abstraction relative to the work that was actually being done.

PENNER: Yes, yes.

CHODOROW: Whereas some departments were developed with very specific— Mel Spiro in anthropology, George Mandler, who we talked to about psychology—they had very specific approaches and they gathered people here who were concentrating in a particular field or sub-field of the discipline. And so, there were those differences. The other thing that has struck me, and I've been asking myself about, is the difference between a place like this and a place like [University of California,] Irvine, for example. Founded roughly at the same time, within a year or so, developed very differently. And one big difference that I would like your response to, and that I see, is that UCSD was essentially built by chairmen—bring a chairman in like yourself,

and give them the freedom to develop according to their own vision in their own field—whereas Irvine appears to have been developed by deans. They appointed deans of whole schools. So, there's a school of life sciences, a school of physical sciences, a school of humanities, a school of social sciences. And the deans dominated the building of the sub-departments within each school, and no one covers that large a range. And the result is it was a much spottier development.

PENNER: I think that's a good point. You don't really want a central organization to develop academic disciplines that they don't have intimate familiarity with. Also, it is true, I think, that people tend to mirror what they're most familiar with. And you have to remember, I came from a multi-disciplinary division of engineering.

CHODOROW: Right.

PENNER: And it was assumed that every person in this group was a stellar performer in his own field, but there was close collaboration across the board. There was never the kind of thing you described at Penn, at Caltech.

CHODOROW: Right.

PENNER: The Caltech faculty were always pushing the young people. So, we inherited that. I think that there are things to be said for compartmentalizing activities. I heard them from [Richard C.] Atkinson, who used to spout them before the school of engineering was founded, and very convincingly. I'm not sure that he ever really convinced me that that was the better way to go, but I mean, the statements were. And when you look at the administration chart, you can see mechanical engineering and chemical engineering and electrical engineering, and you know this helps bring in students. And everybody can identify with it. It's just a great way to advertise, if you like. On the other hand, my view would be that if you have good enough people you don't need to do much advertising. We never had trouble getting people here—good people, good graduate students. There's more of a problem in that respect now than we ever had in the past. I'm not sure that— Well, certainly the size has something to do with it. I mean, to my way of thinking, a hundred-person faculty department is easily manageable by one chairman. But if you prefer, you can have a hundred-person department managed by ten chairmen in ten different disciplines. It's just another way of doing business.

CHODOROW: And in fact, in essence, Scripps is a hundred-person department.

PENNER: And very effective and very well-managed, because the people cooperate across the board. Maybe when you get compartmentalization you get this jealousy that is absent when you don't have it.

CHODOROW: You get battles over resources.

PENNER: You get battles over turf and resources and prestige. Those things are absent if you just are one big group.

CHODOROW: Right. One of the questions would be, however, whether the Scripps model or model that you just enunciated that could be used in engineering, because that's roughly the size of the engineering faculty—about a hundred. It's a model that works very well at the graduate level and research, but not at the undergraduate level.

PENNER: Well, it certainly works well at the graduate level and research. Does it work at the undergraduate level? Well, I think it works if you've got the right people. My first— No, not my first year. My second year at Caltech, the person who was sort of running the Guggenheim Aeronautical Laboratory and Jet Propulsion, the one who was my boss in the real sense of the word, said, "I just wrote a book on controls, and I want to see whether it's a good book. Would you please teach this book?" And there I was, you know, a young assistant professor teaching a subject I knew nothing about with the author sitting in the front row telling me what he meant. [laughter] It was a wonderful experience. I never learned anything so fast in my life.

CHODOROW: I can imagine.

PENNER: This was H.S. [Hsue-Shen] Tsien. He was a very famous man before he went back to China. He became the director of the Chinese nuclear and missile programs. This was before your time. There was a big national scandal associated with it. He packed up some books that were supposedly secret. And then they found at the [U.S.] Immigration and Nationalization Department they were log tables, so— I think that the undergraduate teaching, if it's done with dedication, doesn't require the departmental structure. As a matter of fact, I think the departmental structure interferes with efficient undergraduate instruction. Let me give you an example: at UCSD right now, we probably have three or four separate versions of a junior or sophomore level course in thermodynamics. I would judge that to be a tremendous waste of academic effort, because one good course would serve. These are not that different. I mean, the emphasis might be slightly different, but the fundamentals are the same. This is certainly

something I would never have tolerated when I was chairman. If there's a thermodynamics course, it's for everybody; it's one subject.

CHODOROW: As a matter of fact, that was the strategy that we undertook at Penn. The dean of engineering there undertook to persuade—and succeeded in persuading—his colleagues who were divided up into traditional departments, and it was a faculty of roughly a hundred—that there were certain courses which were common to all of engineering that should be taught commonly. And he meant not only that there would be one course in that with the appropriate number of sections for discussion and so on, but that any member of the faculty from any department might teach it.

PENNER: So, you have a somewhat different slant in different years. What's wrong with that?

CHODOROW: That's right.

PENNER: That's fine. I mean, that—

CHODOROW: But there are fundamental principles that are embedded in every one of these courses.

PENNER: Right, right. So, in that sense, the division into departments puts an extra load on the faculty and doesn't really help the students very much, I think.

CHODOROW: Right. And one of the effects has to do with the pentiperameter—the famous pentiperameter.

PENNER: Well, that's a bad thing though, you know, to make the teaching load <u>the</u> criterion for how many people you can bring in.

CHODOROW: That's right. But it is accentuated by the walls that are built up around departments, because in a more fluid situation where the students are moving from faculty member to faculty member without regard to department in order to take especially the basic courses, the distribution of teaching credit for the purposes of trying to figure out what the economics of the system look like—which is what I presume you were after when you started trying to make these measurements—will come out much smoother than it does when there's an artificial wall that says you have to take that course over <u>here</u>, but not over here.

PENNER: No, I think you're completely right. And there are other problems that I see with the evolution of separate departments that concern the graduate students. And I would never have tolerated the graduate student being here for, say, three years to finish up his research, taking no courses at all, which is happening now.

CHODOROW: Yes.

PENNER: I think that's a terrible mistake. I mean, these people are in an environment where they have a unique opportunity to learn something about other fields—

CHODOROW: Right.

PENNER: —and they don't do it. They are so overloaded and over-pressured with their very narrow research topic that they forget about the educational opportunity in the university.

CHODOROW: That's right.

PENNER: And that's happening. I mean, many of my colleagues in AMES are complaining about that now, that there is no structured requirement for students to take at least one course. I mean, it doesn't take such—

CHODOROW: Right. Every term.

PENNER: Every term they should take something. If you were a student— I mean, even as a faculty member, that's the kind of thing you'd do.

CHODOROW: That's right. I did it as a graduate student. I took courses in literature and linguistics while carrying out my history major, but these were things that were relevant, I thought.

PENNER: They enriched your outlook, certainly.

CHODOROW: And they made, in the long run, it possible for me to teach things that I would not have been able to teach otherwise, or would have been rushing to catch up on and would have had no formal instruction whatsoever. And that was a big benefit to me.

PENNER: I'm sure it was. I'm sure it would be to our students, particularly when they go out now and start working in fields that may have relatively short lifetimes.

CHODOROW: That's true, too.

PENNER: We always say our students, when they have a Ph.D., they are ready to work on anything. They'll be more ready to work on anything if they know a little more.

CHODOROW: That's right. The great model of graduate education in my field was, for me anyway, set by the great Dekker, Paul Dekker at Cornell [University], who ran a research seminar for his students. And every student, regardless of the stage at which that student was working at this point, was in that seminar throughout the entire year.

PENNER: That's a wonderful idea. That would do it.

CHODOROW: So that it didn't matter that you were almost completed your dissertation. You were also in the room with a young student who was just starting off, and then the instruction that went on, the experience that was passed along was enormous. And the breadth of things that people were working on was quite enormous in those days. In those days, every student in American history, for example, had to take English constitutional history as well and European history. And they were— It was a very different approach to what is now a very narrow— Now, of course, you can come in and study Southern American history, and that's all you do.

PENNER: Well, the seminar idea is a good one.

CHODOROW: Yeah.

PENNER: We took— There's an effort by the old-timers to continue some of that tradition with this evening seminar that Jon [Jonathan S.] Singer initiated and with a luncheon seminar that we have periodically, and people talk about all kinds of topics. Unfortunately, the audience is very limited and typically it's the repeat elder statesmen who sit around and get re-educated.

CHODOROW: That's right, yes.

PENNER: But to have a universal seminar— Roger Revelle used to insist on that. I remember talking to him about that repeatedly, and he said, "I want a seminar where everybody goes and where everybody learns something. And I want the young people there, especially." That was one of his preferred statements.

CHODOROW: Well, he was right. Well, I think we've brought this to an end.

PENNER: Good.

CHODOROW: It was very useful and very, very good and enjoyable. It was good to see you again.

PENNER: Well, I'm glad you got me before I get Alzheimer's. *[laughs]*

[END OF INTERVIEW]