An Oral History of

S. JONATHAN SINGER and STANLEY CHODOROW

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CHODOROW: I'm talking to people who were here from the beginning, involved in the foundation of the departments and asking questions about the intellectual vision and the nature of the discipline of the time—the way in which the vision and intentions of the founders were affected by early recruitments, both positive and negative, as ones that succeeded and ones that failed. What you were really trying to accomplish in those early days and how you got from essentially T=0 to a point where the department was beginning to function. Do you want to just start by talking about that?

SINGER: Okay. Well, I'm Jon Singer. And I got here about 1961. So, we were there sort of at the beginning, but I have to tell you right off, I was not a major figure in the way things got started. I came here to get some work done and was gradually dragged into a lot of the operations of the place, which I fully enjoyed but didn't anticipate doing. I came here from a chemistry department position in Yale [University] for ten years to enter into a biology department and under the chairmanship of David Bonner—a sainted memory. David and I had been close friends at Yale, but I had never been in a biology department, so—. I knew the difference between a rat and a rabbit, but that was about it. So, I had a lot of adjusting to do. Very new circumstances. There were only four of us in the biology department the time I came. I was the only other tenured member of the department.

CHODOROW: Who were the four? David Bonner—

SINGER: David and myself, Stanley Mills, and Jack DeMoss. Stanley and Jack were assistant profs [professors]. Jack has left since Stan's died. It turned out that David Bonner was quite a visionary in a lot of matters—and especially in biology and medicine. And I was not. I mean, I didn't know much about it. But as time went on, I could see the direction he was moving, and I fully participated and agreed with him. I think his appointment in biology was engineered of course by Roger Revelle. It's an apparent small indicator—maybe not so small—of how the place started up at the beginning. David at Yale was known as a sort of ex-bad boy. He was a maverick, loudmouth—.Good researcher, everybody agreed. But nobody accorded any weight. He had been suffering from Hodgkin's disease, but had been in remission for quite a long time

so we thought that he was okay. But Yale treated him like dirt, so—.He wasn't even on the regular faculty. But when it became clear that he could move because wherever he went, they would pick up his insurance policy, he began to look around. And Roger was the gourd then, apparently from the beginning. Because he saw in David what a lot of people didn't—including myself. That he was quite a visionary in matters pertaining to biology and medicine. I mention that because all the people came here at the beginning under Roger's influence—his persuasion. Like Keith Brueckner in physics, Jim [James] Arnold in chemistry, and David in biology. Were extraordinary people. Tremendous judgment about their fields. And each of the three departments independently chose to go a very unusual way. Physics, for example, which at that time was in the nation's hole—very much involved in high energy physics and so on—.Brueckner said this is silly. We're not going to have a big accelerated down here. Not right away or anything like that. All he needs is to concentrate in other areas that are not volatile. We're going to heavily emphasize state physics, for example, which turned out to be a very wise move. Solid state physics was one of the big areas—simple conductors and so on and so forth. I remember he brought in Bernd T. Matthias to some of that pioneering work. Likewise, in chemistry—.You know, the big thing in chemistry in those days was organic chemistry. If you didn't have half the faculty in organic chemistry, something was the matter. But we didn't have an organic chemist on the faculty before 1966. Fred Friedl came. Because Jim saw that this was not any longer the area of choice for concentration and went into another field. Likewise, in biology, David had the remarkable vision that I must say that I didn't share at the time fully. That molecular biology which was just really starting out in the sixties. DNA, double helix, was just recently done a few years before, but it was still a field with relevant influence. And biology was, you know—plant biology, physiology—all kinds of—probably ecology. He wanted the staff, the whole department of molecular biologists who would learn in these areas what had to be done and then bring molecular biology into these areas. Something that most people didn't think could be done. But that is what has happened in the ensuing forty years.

CHODOROW: That's important. It wasn't merely that he wanted to create a molecular biology department. It was that he wanted to introduce molecular biological technique and knowledge into all the areas of life sciences.

SINGER: Because in fact, that he saw what was true. True as in—a lot of people still don't appreciate. Which is that molecular biology is the foundation of all the other ones. So, the pyramid—you have the base. And the idea was if you learned enough molecular biology and began to branch out, you would see the relevance of it in other areas. Cell biology, physiology,

so on. And make advances that you wouldn't otherwise. The conventional people in the field who didn't know any molecular biology—they wouldn't be in a position to take advantage of it. Well, unfortunately for David, he was here only three years before he died. But in that time, he did manage to set the medical school on its way. Fabulous story.

Shortly after we all got here, the intention was to create a medical school down here. And there was nobody here. I mean, the only person in fact was David who had been in the medical school at Yale. So, he undertook, while getting the biology department started, to also initiate the medical school, which involved what the planners so-called Bonner Plan. Which was to do some medical school different from most others. In most medical schools, you have the basic science division and a clinical division. And if it's on a campus, you have a campus division. And these three have overlapping and often difficult relations with one another. As was the case of Yale. He saw that if you could take the basic science component of the medical school and incorporate with the campus departments of biology, you could get the best of both worlds. You could bring better science into the medical school and you could, in general, make a more scientific medicine locally. And this scheme was bought. And it operated pretty well for a while. The crunch came when Reagan became governor and the grand eloquent ideas of the early sixties were squelched pretty much by cutting back FTEs [Full Time Equivalent] and so on. So, the medical school went through a period of kind of inversion of its original directions. That made things a little difficult.

WESTBROOK: *[inaudible]*

SINGER: I don't know. I don't know any of that since.

WESTBROOK: *[inaudible]*

SINGER: No. In fact, I remember I was up at Stanford [University] in 1964. David was still here in '63. Yes. David was still alive. And I was talking to Arthur Kornberg who was the *[inaudible]* of the biochemistry of the Stanford Medical School. And who had been in medical school all his life. He was in a convention of medical school—and Stanford still is a very good one though, but very conventional. And he said, "What the hell do you guys think you're doing down there? This is not the way a medical school in organized. You're going to fall on your face." Well, so this was totally out of sync with the profession whether what was curious was how every area in the sciences developed in a similar way. That is, great planned departure from the conventional. While at the same time—now, this is very important—bringing in professionals. This was not a

Santa Cruz with a lot of fuzzy ideas and people. This was a group of people, and I would include myself, who are real pros. Maybe unusual, but nonetheless, people who would get into the National Academy of American *[inaudible],* not just a fly-by-night visionary. So, I must say the first six years I was here had gradually became completely absorbed in the process of building the campus, along with dozens of other ones. It was the best academic time of my life by far. I can't imagine for me a set of circumstances that would have been—. Because this combination of visionary and professional abilities, that to me was unbelievable.

CHODOROW: You came from a chemistry department. That implies that when David Bonner was setting up the department of biology, he was casting a broader than usual net.

SINGER: Well, yes. I think so. But also, I had been—. He knew who I was since we were pretty close at Yale. But I had a lot of interest that would move him in the direction of biology. I was a physical biochemist in the chemistry department. But he knew that by the time my tenth year at Yale was passed, that I was firmly moving in the direction of biology. In 1959—which would have been a few years before I left—I quite independently, with everything else that going on in my life, devised a means of sustaining antibodies for electron microscopy. It was a very biologically oriented thing to do. Especially in connection with the kinds of things I was supposed to be doing in the chemistry department. And it's had a lot of success since then. David knew about that. He figured that I was a biologist as far as he was concerned. It didn't matter that there were a lot of things I didn't know. And of course, I started teaching right away. We had graduate students very early on before we had undergraduates. While we were still down at SIO [Scripps Institute of Oceanography]. And I was teaching within the biology department. I was bringing, in a way, physical chemistry and *[inaudible]* chemistry into the biology department. It was very appropriate.

CHODOROW: One of the things about UCSD's organization is that there were biochemists of course in chemistry and biochemists in biology. Was that common around the country? Or was that—?

SINGER: Well, no. The common thing was to have a separate department of biochemistry on the campus. And likewise, a separate department of biochemistry in the medical school.

CHODOROW: There are five that did. Five departments of biochemistry.

SINGER: We decided very early on that biochemistry was not a department arrangement. We were doing biochemistry which was more or less synonymous with molecular biology. We were doing a lot of that and we had great empathy with that. But we recognize that the connection of biochemistry with chemistry on one hand and biology on the other was what really mattered. So, chemistry continually fed new ideas into biochemistry. And biochemistry fed new ideas into biology. What you wanted was to bring biochemistry into association—close association with biology and with chemistry. So that arrangement was placed in the beginning. Martin Kamen was really the first biochemist and he was in the chemistry department while I was a card-carrying biochemist and I was in the biology department. So that was deliberate. In fact, the other deliberate aspect of it was to have a single department of biology. That was envisioned when we got big that we wouldn't split up into all these compartments, which was the rationale of science at Yale. Because you could see it—it was terrible.

CHODOROW: There were in microbiology, zoology, botany. All physiology.

SINGER: Right, right. This was an arrangement that was suitable for the 1920s, but you know, it's clearly going on—. That's because at that time, each of these areas was a set of phenomena. They didn't have anything to do one another. What molecular biology has done—and cell biology, as well—has been to provide the common base for all of this kind of biology. Except for ecology. Sort of separate. But genetics—. There are plenty of places that have departments of genetics—and still do. And that, to us, was an absurdity. You can't separate genetics from molecular biology. The whole triumph of modern biology is called molecular genetics.

CHODOROW: What happened in the early recruitments? You had—. David got here at '60 or '59.

SINGER: '61.

CHODOROW: He got here in '61. So, you came with me.

SINGER: Yeah. More or less.

CHODOROW: And Stan Mills—?

SINGER: Yeah. We all came together.

CHODOROW: All came together. So where did they come from?

SINGER: All from Yale.

CHODOROW: All from Yale. So how—?

SINGER: Stanley was an assistant professor in David's—in the same department as David.

CHODOROW: And then, where did you go from there? What happened in those days?

SINGER: Well, let me tell you one—. First of all, I remember I did something quite difficult and then I'll come back to that. Because I think that was more interesting than staying in Yale. When I was recruited—. David and I were recruited. A transition had emerged. A mock transition had occurred in the academic plan. The was slated originally to be a kind of UC-Caltech. And Riverside, a UC Pomona or something like that. The demographics suggested that there was going to be a tremendous need for full-scale campuses, so somewhere around '60 or '59—I don't remember—from Berkeley, they came down and created what would now become changed into full campuses.

The reasons the three departments that were recruited were first the physics, chemistry, and biology was in regard to this Caltech organization. So suddenly, we were confronted with the necessity to convert into a broader campus. Which almost everybody in the sciences accepted enthusiastically. I mean, none of us had any kind of group of people. Came and they would consult Jim. We were humanists, primarily, even though we were scientists. And none of us had any negative feelings at all about becoming a regular campus instead of a Caltech. The opposite was true of Riverside. Where when they were required to go into the sciences, a lot of people left. Called us. A whole set of them decided, you know, this was not—. They didn't like having the sciences around. I guess. But here the atmosphere was going really well. So suddenly the department was—I mean the campus which was all physics, chemistry and a little bit of biology—had thrust on it the necessity to go into the humanities. Which, of course, we all took very seriously and hoped to in fact with the same kind of class that we had achieved in the sciences.

By the way, it's interesting the psychology was involved in coming to a place like this. I knew a lot of scientist who wouldn't be caught dead here. They had a good set up wherever they were in a place they had been well-established. They had good labs and good reputations. Why would they come to a little dirt water—. The place is just starting up. We had all the work that we

have in the process of starting up. And I got to Yale—. The year I got there, they celebrated their 250th anniversary. That put me right in my place.

CHODOROW: That's right. 1701. It was 1951.

SINGER: So, I never thought about the work that would be involved in coming. I was very excited. And I had David, too. What I saw was the fact that this place had already hired Bruno Zimm, Martin Kamen—I didn't know Jim—Joe [Joseph E.] Mayer, Maria [Goeppert] Mayer. It was obvious this place was on the road to becoming a mecca of the sciences. It didn't need any vision whatsoever. If you didn't want a comfort place like this, you needed to have your head examined. So, for all of us, the same attitude. Martin came from Brandeis because, you know, this is the place to be. It wasn't because we were given anything special in the way of inducing financially or otherwise. It was just the idea. I mean, you had to be a fool not to appreciate what was happening. And California had an aura in 1961 that Reagan destroyed forever. It still had Pat Brown as governor and was promising to the sun, the moon, the stars. Sputnik had just—- Anyway, coming back to this. We had to recruit in the humanities. So naturally what was done was that upstate and ourselves put together some kind of *[inaudible]* committee which would advise about the appropriate people in the humanities and social science. And they began operating. We had a system at the time. It was so small. When somebody would come down who was being interviewed, he would give a lecture in Sherwood Hall or in Scripps—.

CHODOROW: The Scripps Auditorium

SINGER: One or the other. And everybody came. It's open to all. So, we would all religiously go up on the weekend, or whatever, and listened. After a while it dawned on us these weren't the kind of people we want. I still remember—they brought in the chairman of English of Washington University of Seattle. I think it was Washington. Who I learned later—I guess his claim to fame was that he had refuge in drinking alcohol or suicide. Including the time he was there. Then he gave a talk on Harding—Thomas Harding—which was so dull that everybody was eventually put to sleep. And this was the way they were made. Grading in these people of secondary prominence, which was all we could expect to get in a dirt water place that had no library and no tradition.

CHODOROW: Who were they bringing—. Who are the they who were bringing in them?

SINGER: Well, so the Brahman's [?]—. I don't remember that whole committee. I remember that Steve [Stephen Coburn] Pepper was the one from philosophy from Berkeley, professor of philosophy. They had somebody from UCLA [University of California, Los Angeles]. And then three people from local.

CHODOROW: Was [John] Galbraith one of them?

SINGER: No. He wasn't here yet. He was—. I don't think he was involved in it. I can't remember who the others were. Herb York wasn't here, you know. But of course, you're getting my side of things—you'd probably get a different side from somebody else, like Herb.

WESTBROOK: What was the rationale *[inaudible]*—?

SINGER: Well, they didn't know anything about it. This was an area that they needed time. It's perfectly legitimate. The problem was, these people started out—. I mean, we didn't realize till later, these people didn't understand us. And they started out with the premise that we were, you know, Berkeley or UCLA. La Jolla, UCSD. What did they hope to get? They were doing us a great favor by bringing in the chairman of English at University of Washington. Who turned out to be as bad as you could get.

So, I recall Martin, David, and myself storming into Herb York's office the day after this debacle. And I was saying, "You know, this—. Well, why? We didn't come here to participate in a society which was the inverse of Yale." Humanities were everything and the sciences were dirt. We weren't trying to—. We wanted things to be right and these people who were in writing, history and all were terrible. So, actually, the Brahmans [?] from the outside weren't there at that time in that office. But I recall at least Jim Arnold from the local committee. I guess Jim got really irritated with all of this and he said, "Well, if you guys think you can do better, go ahead!" And Martin said, "Okay!" You know, we all knew a lot of people, even though we didn't know the subject. I got on the phone to Charles Feidelson at Yale. He's a friend of mine in the literature department.

Well, meanwhile, we all decided to divvy up. Mine was going to do history and so on and so forth. And I would do literature and psychology because I knew people at Yale and Hopkins there. And David would—. He also got involved in literature. He was a very good friend of Arnold Stegman at Stanford. So, I got on the board. Charlie said to me, "I got just the man for you." Ohio State is imploding. There's a guy named John Bricker who was a senator who was in

the sixties. It was not long after the McCarthy trials. And then there were people who were ready to leave. Roy Harvey Pearce. I never heard of him, of course. So that's how we got around. And I don't know a lot of other people's names, but I think especially because of the types that were coming in, he was totally a breath of fresh air.

CHODOROW: Do you have any idea how Martin found Geoffrey Barraclough in history?

SINGER: Well, I remember the first man who came through. It was Leon Epstein from Wisconsin. And he was apparently one of these people who couldn't see himself moving into a place like this. I don't know how Barraclough was—got involved. And I don't remember what stage because he sort of gave up on history after a while. We weren't having a lot of success. It was not the earliest of the group—

CHODOROW: No. In fact, philosophy and literature—

SINGER: —psychology—

CHODOROW: and was very—

SINGER: —were relatively early. Now psychology—I knew a guy in there named Irv [Irving] Janis at Yale who was—. You know, had a lot of favor in him. He said, "There's only one guy you should get. He's a nut—absolutely crazy—and he has a very controversial reputation, but [B.F.] Skinner is the only man." So, we went after Skinner, who was past his prime. What we didn't know was that Skinner was angling for a US Public Health Service lifetime professorship in Harvard. That was what he was really interested in. He wasn't interested in us. But he was fun. And so, he set the tone. Skinner was a little different from the guy at the University of Washington.

Well, this was this early—. And things were very hectic. All this time, I was trying to get my work done, which was in fact done very well. But I must have had enormous energy in those days that I don't have anymore. But we were all pretty much together. And the first kinds of people we got were, I think, looking back, still quite extraordinary. Roy, for example, is probably the key figure in the academic plan that Revelle College adopted eventually. It's a very ambitious plan. And the remarkable thing always to me was that he would work with this group of people congregating from every place. Jim Arnold came from Chicago and then Princeton. I came from, you know, with David from Yale. From all over. And we've never met—. We've never known each other before. Furthermore, you're all in retrospect fillies, mavericks, misfits—put in the

most pejorative way. We were all misfits. A scientist who was more apt to humanities—that's already been missing. It turned out that Roy was very partial to the idea of the humanities would equal learning science. So, he was a key figure in the system against the advice of the scientist. But the people in humanities should have the same science treatment. You know, he was amazing. So, as I say, he didn't devise the scheme along, but he was responsible as anybody.

I'm always a little sad about what's happened with Roy over the years because I think he was a very influential, visionary figure in the early days. He also wanted to put together a department that was unconventional and critical. We wouldn't have an English department. He wanted a department of comparative literature.

WESTBROOK: That sounds like that was pretty much what was described as *[inaudible]—.*

SINGER: Yes. It was part of the syndrome. But what was amazing was all these misfits came together, each one of which would have been imagined to have his own crazy ideas about what to do. And when they came together, in a few hours, days, weeks, we put together a plan that involved all our participation and agreement. They tried at Harvard to put together this core curriculum, you know, fifteen years ago, and it was a disaster. When they finished, they had one-twentieth of what we started with here. Still have a good part of it. And that dissipated in about three years. You couldn't do it. With these entrenched interests and lack of vision and so on. Here, we're starting from scratch with a bunch of people who are happy to have the chance to do something different.

And I must say, I think—. You know, there's all this question of is this history deterministic or matter of individual persons at the right time, right place. Roger [Revelle] being here at that time was clearly the great determining factor. His individuality was what made this place what it became. In my opinion, it's been going downhill ever since. But the momentum that it got from his activities at the beginning and from the group of people that came here at the beginning was incredible.

WESTBROOK: Can you say more about how *[inaudible]*—.

SINGER: Well, I don't know if you know anything about—. This was a place of very low entropy in the beginning. It was very highly unusual. Entropy is a measure of order. This place was crystalline at the beginning. And there's no way that any institution's going to be maintained for

a very long time that way. For example, the first group of people we came to recruit was subsequently—. We didn't do a very good job. Physics, chemistry, biology.

CHODOROW: Same thing. The second generation—. The ones you recruited first in the sixties were not as good as you got.

SINGER: We didn't know who we were. We didn't know what we were looking for.

CHODOROW: That's an interesting issue because it suggests that the defect in crystalline binder—the realization that you just described—Is that it doesn't have the traditions of judgment of talent?

SINGER: Well, neither did the places that are senile or in mid-life. They don't have it either.

CHODOROW: So, what you're pretty much saying is that—

SINGER: Except Caltech.

CHODOROW: Except for Caltech. Why Caltech?

SINGER: Caltech, I don't know. But somebody ought to really examine that. Caltech through minimum of four generations of faculty has—in physics, chemistry, and biology—maintained a very high level of professional competence. It's amazing.

CHODOROW: Would you say that it's a matter of leadership?

SINGER: Yes.

CHODOROW: Because they have chosen the right, chairs, deans—

SINGER: They have chosen—. It was mostly a matter of judgment on the part of certain officers, certain people. In our situation, it was Roger. And then [Keith A.] Brueckner, who was very good. Involved in physics in picking first-rate people. Jim was fair in chemistry in picking first-rate people. Bruno and Martin were okay. Certainly, put in that category. Some of the others, too. Biology—. Well, David died right away. So, there wasn't any possibility.

CHODOROW: So, who did leadership fall down to?

SINGER: There wasn't anybody else. And I panicked. I still didn't know any biology or biochemistry. We're about to recruit a whole department and start an undergraduate program. I was in way over my head. So, my first job was to recruit a chairman. And I thought with great, great good luck I recruited Brookstone [?] from Stanford who took on the job of then building the—

CHODOROW: They had in fact just made him chair at Stanford. So, you were essentially borrowing their judgment.

SINGER: Well, I don't know how much this thing is. There is a lot of things I could say that I'd better not. But, yeah. So, things didn't develop terribly well. Now during my time as chairman, I recruited Herb [Herbert] Stern, Warren Butler, Don Helinski, and Nelson. Nelson had already been recruited by David Bonner. He's a junior. Works at the heart. So, I don't feel anything to that. Without being much of an expert, I was using the same technique I used to find Roy Harvey Pearce in literature. But when we—. After that, I don't understand why in detail—I understand why in general. But after that, things went very seriously downhill. They did in all the departments.

WESTBROOK: *[inaudible]*

SINGER: Yes. And also, just—. I mean, judgment—. I mean, after all these years in thinking about things—. It's been on my mind a lot. More than you might expect. The judgment about other people doesn't go with professionalism in one's own area.

CHODOROW: These are separate times.

SINGER: Yeah.

CHODOROW: Sometimes they're together and sometimes they're not.

SINGER: And what Roger did was to find people who had both, amazingly. And the subsequent degenerations of chairs and so on may have been professionally very adept, but they didn't—

[END OF PART ONE, BEGIN PART TWO]

SINGER: Sociology—. The development of this campus was very important that had a lot to tell academics about what should be done or shouldn't.

CHODOROW: Let me ask you a question.

SINGER: By the way, I should say. I'm not happy with the biology department till about ten years ago. We had what was considered in the country one of the five best departments around. I mean, I always we could do better, but we were not that bad. We had ten members of the National Academy of Science—unheard of number. We were pretty good. But it was partly why getting rid of a lot of people we hired early on weren't good. And then it became a little bit of a problem of retaining people out of empathy. Clouding our judgments about who were bright and so on. A lot of the elder people now retired were first rate. There was a bolus in the second generation. Competent, but no cigar in biology. And I suspected that was true in all the others.

CHODOROW: When you look back, where did the founders and the first recruitments come from? What kinds of institutions? You came from Yale and what—?

SINGER: Everybody came. As I said, the marvelous thing was that we were all mavericks and misfits, but we were all first-rate. I mean, we came from first-rate places.

CHODOROW: It's different from one department to another.

SINGER: Well, Ron Pearl [?] came from Columbia [University]. So, did George Feher, he came from Bell Labs. One of the problems in chemistry was that they brought in these very good people that had never been very much involved in teaching, especially teaching at a university [*inaudible*]—. When we instituted this curriculum plan, which involved a good dose of science teaching, in chemistry—. Let's see, when I was in Yale, I taught freshman chemistry for ten years. It turns out I had more main years of teaching freshman chemistry to a biology person than the entire chemistry department had. So it was a very, very strange arrangement. It actually was marvelous.

CHODOROW: One thing you said earlier was that David thought that molecular biology should penetrate into all the [*inaudible*]—. How did you recruit to get that done?

SINGER: Well, that was where we didn't all together succeed. But I felt we did very well in departments like Warren Butler.

CHODOROW: He was on the plant side.

SINGER: He was a plant—. He was a biophysicist. But he had been at the department of agriculture for a number of years. And in the presence of a lot of plant physiologists. He was working on and prominently started [*inaudible*]— which captured our attention. He was looking at a protein that's called phytochrome. Which turns that it's clear already at the time, controls everything about [*inaudible*]—. It's a light-sensitive pigment in plants which apparently—which was vitally responsible for flowering and for season—for the response to seasonal changes in the mitoseminary and so on. It's the central motor of plant development. And they had isolated it. This is exactly the kind of thing we were looking for. It was the molecular entry into this enormous problem of plant development. Now, in the process, Warren learned about plants. Herb Stern [?] started out as a plant physiologist but turned to the molecular basis of meiosis. Very important problem involved. So, these were people who had already made the entry into the respective—

CHODOROW: What was Herb bringing?

SINGER: Herb was—. He got in for knowing [*inaudible*]—. But I got him because I called somebody else who was trying to recruit him. So, we were in conflict of going after Herb Stern whom I never heard of before. So, we did that. Don Helinski was the best graduate student. He was a friend of mine's child up at Santa Cruz. We did things that way. Which was, I think, not hard for Bill. It was a different time. And I was a novice at it. I was torn by the end with whether to retain the job for about a tenth of a microsecond. Or give it up. Because I could sense I could do as well as anything. But it's going to be a depart signal. It really which would startle. So, I did what I thought was tremendous for the campus—brought down one of the key figures in the molecular biology of development. Which we all understood was going to be the next big area—that molecular biology was going to be on the forum. Which was happening. The next twenty-five years, the basic mystery of development—that has been solved. The molecule is solved. But it was not from here. It was going to come from there.

CHODOROW: Let's turn back a little bit to the medical school [*inaudible*]—. In 1961-63, there was no one here who was actually a founder of the medical school.

SINGER: Joe—. I mean, David Bonner's first job was to put together appointments for medical school and then find a dean. He put the plan together first and tried to find a dean—a prominent medical professional who would buy into the plan—thinking there ought to be a lot of people.

And there was a parade of people we had. Luke Thomas [?] was one of them—I remember Luke Thomas. They looked at this plan and wouldn't have anything to do with it. There was nobody of any prominence David could induce to undertake this kind of—. It says something about the medical profession. A lot of people came through. Dozens, at least, over a period of year and half. We finally had—. We proposed that he himself would become dean and the regents started that. So almost by default, turned to a friend of Sherm [Sherman] Mellinkoff who was dean at UCLA.

CHODOROW: He was already dean at UCLA?

SINGER: Yeah. Anyway, he had a good friend who was head of Queens Hospital in Honolulu. And Joe Stokes.

CHODOROW: Very famous family. His father was the man who moved the Children's Hospital to here.

SINGER: So, we had Joe. Joe was —— [*inaudible*] to the plan. Joe was not very respected apparently. But then recruited some very good people. Marshall Orloff in surgery, Gene Bernstein [?] in [*inaudible*]—.

CHODOROW: Gene was in vascular surgery.

SINGER: No, no. It wasn't Bernstein.

CHODOROW: Head of medicine was Grounder? [?]

SINGER: Braunwald. So, Gene [Eugene] Braunwald was very good. And they began to move things that they set out to move and did. But were strung up by changes in the attitudes of finances of the state government. Also, some internal fighting which Gerald put example of.

CHODOROW: Which is typical [*inaudible*]—.

SINGER: Anyway, right now it's a good medical school. Probably one of the top twenty-five. None of us ever had the envision of being on the twenty-five. It wasn't either the top one or two. Anyhow, forget it. The marvelous thing was the level of naivete to begin with. There's nobody here—. And that was absolutely the essential part of it, I'm sure. There's nobody here who's a hardened veteran of academic wars. Most of us had been on the outside looking in.

CHODOROW: Not even Roger?

SINGER: Not even Roger.

CHODOROW: Let me ask a question about—. You were talking earlier about how, you know, kind of miraculous way these very diverse people came together and formed a curriculum which was at a greater means. But let's turn the view to the thick of this warmth on scientific work. One of the things I wondered about was whether there wasn't an inevitable over time—an inevitable growth and division to departments. And the beginning was nowhere near as powerful because there were so few people. And that there was a great deal more exchange of ideas as someone had one on their mind that affected the way science was going.

SINGER: Science was still at that time and is finally becoming more so a matter of individuals doing things. It's useful perhaps—. It's useful from a point of view of stimulation to have other people around with whom you can exchange ideas or something and ask advice when—from their expertise when you needed it. You mean that kind of thing, in fact. But in terms of active collaborations or special arrangements that were made possible there. I don't think so. I don't know any case of. What has happened since, which is partly—mostly, I think, the subject matter—the changes in the way the subject matter had gone in biology. And apparently in the training that new faculty have had over the years, what is the same is that everybody is an individual and works in his own problems and doesn't do that much interacting. What's different is the nature of the personalities. It was important to be founders, to be involved in new academic institutions at the same time they were carrying on the individual professional activities. And they would enjoy going to seminars in literature. And they would attend the chamber music in the city. That's all the change.

In our department we have recruited a bunch of young people who are very good professionally, but never go out of their immediate environment. They don't attend a seminar that's in the department that isn't immediately relevant. Something other than those. That's the way they've been trained. That's the way they're training the students. So, we're going to have—. We see the course of development that this guy [Jose] Ortega y Gasset predicted. We are developing in all areas, not just the sciences. What he called "learned barbarians" in 1932. I mean these are people who never read a book outside this field. Who don't go to concert—classical music concert. Who would never attend a seminar that wasn't absolutely essential to their work. Who are not part of academia really. They could just well be a matter of research institution. They do

the teaching. And in our case in our department, they do pretty well. But it's remarkably [*inaudible*]—.

And as I said, it's practically the major of the subjects. Subjects—. I used to work on—in my lab with postdocs and so on, and usually five or six remotely related problems. But there was every post doc working on a single problem. And they were all different. And that interaction within the laboratory was tremendously interesting and efficient. Every one of those—. Any of those six subject barriers has now gotten to the point where I can't keep up with any of them. And the whole thing has changed so dramatically that you have to be an exceptional person to do it. And the tragedy is biology is not that kind of subject. The magic—. I mean, the unifying thing about biology is evolution. Evolution has dictated that things have gone—that look very different but are exceedingly homologous. They stem from earlier stages in evolution. And so, it means in everything in biology—lots of things in biology, over a whole spectrum of biological diversity are phenomenally I really think related when you get down to it. It puts a premium on being well-versed. So that you see the connection between a phenomenon that's occurring with the [*inaudible*] —— and a phenomenon that's occurring in Alzheimer's disease.

CHODOROW: The key is the evolutionary—

SINGER: The fact is that what's happening in this [*inaudible*] —— had nothing whatsoever to do with Alzheimer's disease. But the phenomenology turns out at the molecular level to be very parallel. And nobody would anticipate that.

CHODOROW: One way of looking at the history of the field is that the resulting double helix is essentially a declining of a common core. And it now had become a dozen different fields. And one wonders whether one needs another such revolution to re-emphasize the core of these different research projects.

SINGER: No. It's not so much that. It's that, you know—. Try to put it this way. In biology, there are a billion things that are happening. Fly, worm—they are all billion things based on a thousand patterns. That's what evolution has done. Not one pattern about DNA, but a thousand. So, people are learning about these thousands and these billions. But the hard thing to is to—

CHODOROW: Is over here.

SINGER: —trace the connections. And you have to know a lot about the billion to trace these connections. And the field is pushing people away from being able to do that.

WESTBROOK: [*inaudible*]

SINGER: Then the problem is how to be both. And that's very hard to do. And the people who are like that in the old days, the polities [?] who are——a wealth of information.

WESTBROOK: [*inaudible*]

SINGER: Well, it's just become more and more the case. I mean, nucleic acid was deranged. They did it at school. I mean that's pretty early for this phenomenon which was recognizable at the time. Well, you know, in history how many [*inaudible*] —— a hundred.

CHODOROW: Well, in fact, a lot of the things you named—the ability to identify scholars at that level has become very, very—. You do not find—. It's very hard to name people everybody could miss. And one of the reasons is that writing, that is the literary component of most fields in the humanities has dropped. And was in fact through that literary [*inaudible*] —— component that could be reached—it could be on their own narrow specialty. They were able to fact, to explain in a way that was interesting enough and compelling what it was that they were discovering. Or how they were picking an era of time. And the great historian—. And even in history and literature, which are fields where good writings always valuable as opposed to social sciences where it wasn't. It's very hard to find good writers [*inaudible*]—. And good writers write for people. Not just for the five other people in the field.

SINGER: Well, if you come to a biology—. We'll take a look at the proceedings at the National Academy of Sciences, which is mostly the internal biology these days. And you look at the titles of—the brook-2 gene [?] encodes a hierosync [?] of a specific kinase of—. We can't understand what the hell the titles are. I mean another biologist become a jargon almost of necessity. I mean, even as if it's a woefully obscure—it's a problem in the growth and development of the subject. Biology is, after all, the most complicated subject in certainly science. And it's the youngest. So, we're going through this period of strong [*inaudible*] —— where the idea of being a generalist is absurd. And what's going to happen in the future, I don't know. I think—. Well, it's a problem.

CHODOROW: What are the big discoveries made at UCSD? At UCSD given, let's say, the first ten years of [*inaudible*]—. Were there some real breakthroughs?

SINGER: Well, it was a lot of work. And a lot of what I'm not familiar with. Bernd Matthias [?] was—before he died—on the verge of discovering what has become one of the good things in physics. High temperature [*inaudible*]—-. That was what he was working on. And he discovered just the first modestly effective higher temperatures since then. So, he probably would have instrument in subsequent developments. You have to ask people in separate departments.

CHODOROW: What about biology?

SINGER: Well, there was a lot of good work. Don Helinski, for example, pioneered a field which has since become extremely important. Things at the beginning, when he got into it, was very obscure. The so-called plasmoids—bacteria. It turns out, that now in all of biology, there is this phenomenon transposed of jumping genes. We normally think of genes being lined up in the chromosomes and very stable arrangement. And it's true. But there are mechanisms to excise pieces of genes and transport them somewhere else. First discovered by Barbara McClintock. And nobody believed her when she did them. These plasmoids have [*inaudible*] — work and is example of—. It turned out to be an example of [*inaudible*] —— and very important. We talked of—. We explained of a lot of people who done a lot of different things.

CHODOROW: Anything else? Fred? This is very interesting discussion.

SINGER: Well, you know, I hasten to add I've already talked to a number of people on various cases over the years. And it's amazing everybody has a very different perspective. Who did what and how it happened and so on. And the worse of the viewpoint is very [*inaudible*]——. So, I'm sure you'll hear different things from different people. But what was interesting to me, I mean what I considered my great achievements in this place—was recruiting Roy Harvey Pearce and Andy [Andrew] Wright

CHODOROW: Who recruited Richard Popkin?

SINGER: That was Steve Peppers [?]. He was—. Pepper was outside from Berkeley [*inaudible*]——. Popkin, [Jason L.] Saunders, and [Avrum] Stroll. Again, they put together a philosophy department—humanities. There was nothing like. It was all analytical philosophy.

And Avrum was the only analytical philosopher they had. Everybody else was—. Dick was a historian of ideas. [*inaudible*] —— Father Henry [?]. He's a Christian medieval philosopher.

CHODOROW: He was recalled by his superior.

SINGER: And of course, Herbert.

CHODOROW: Avrum's background in education of [*inaudible*] —— philosophy. He was a very unique [*inaudible*] ——. Very unusual. Linguistics. But that was just exactly the point. The point was the beginning—extraordinary people who were involved in many areas. And [George] Mandler was the founder of psychology

WESTBROOK: [*inaudible*].

SINGER: I found him. I don't take a lot of credit for it, but I did. He was third on a list of people that Merv Janis [?] had recommended. And second after Skinner was charged with harassment. Put him out and decided not to. And then Brueckner went to [*inaudible*]——.

CHODOROW: And they brought out [*inaudible*] —— shortly after that.

SINGER: A guy named Warner [?] who then went back to Columbia.

CHODOROW: And [George S.] Reynolds.

SINGER: And [*inaudible*] ——. It was interesting. But we knew what—. I mean, we all knew what was going on. It was a community, but I think I think of very fondly in retrospective. You were asking why I think differently. Something has to do with being at a settled place. And the attractions of a settled place are different from the attractions of a brand-new place. And the kind of people who come are now, I think, characteristic of different [*inaudible*]——.

CHODOROW: The same kind of people who go to Illinois, Yale.

SINGER: It's a good place to go. Good libraries in Illinois. And so on. Hey, I came here because it wasn't like that. I came here because I tried to get into biology at Yale during the last few years I was there—under circumstances that I would feel comfortable with. That is, not knowing any biology, which could be tolerated in a community for a while, but not necessarily be productive. And that was attractive here. And David was a very good friend who was confident that I would do all right arrangement. And the combination of chemistry and biology for me was

what is at the heart of my own work. But it had to be tolerated by an institution. At Yale, you would have gone into a department of botany, and I didn't know anything about plants. Zoology, I didn't know anything about animals. By biophysics, which was idiotic separation of physics and all the others. And biochemistry—. I tried as an assistant prof, an associate prof to cajole the biology departments into uniting. Which they did after David and I left. There was pressure from three members. But they weren't about to—.

CHODOROW: Well, a great project Berkeley implied in a broad part.

SINGER: I was involved in that. I was at the medical group. But they didn't really do—they didn't do near what we did. They did into three divisions. By the way, we're doing the singing now. Our biology department decided to become a school of biology, if they can manage it.

WESTBROOK: [*inaudible*].

SINGER: Well, the fragmentation is what's real. The means of—. They'll wind up being very much like Berkeley. See, the problem in biology as a science, there's a unity of the subject that you can't escape—it's evolution. So, if you're interested in the neurosciences, for example, everything that goes on in the nerve, characteristic behaviors of cell in the brain is all a matter of molecular and cell biology. They specialize to do things that are in the nervous system and are different version of the system. And that's true of all of them. The muscle is a very specialized tissue in biology. But it utilizes my five forms of [*inaudible*] —— come in all other cells as well. Because that's done in a special arrangement, especially the molecular structure that do a very particular kind of thing in muscle where it is done differently. Every cell has a kind of mechanical chemistry. Every cell has to move through things. Muscle is a matter of mechanics. The molecules are specialized to pull, contract—they do that on other cells. But the muscle is the organ that's discovered to have these [*inaudible*] —— first because they are so highly organized. The limited thing is labeling. Utilizes same programs occur—every cell. But that's biology.

CHODOROW: Good. Thank you. You've been terrific.

SINGER: Well, thank you. I hope this is kind of an opportunity, huh. Nobody ever talked to me about this thing that Dick [Richard C.] Atkinson put together. And that completely eliminated the first ten years.

CHODOROW: Because she was incapable of dealing with the intellectual tradition of LSG [?]—focused entirely on the administrative and the property acquisitions and politics to the extend you couldn't understand.

SINGER: By the way, another element there. Which was saying something sort of rather obscure. This place really—after Roger was out of the picture—was the product of the faculty. The faculty did almost everything. We had a succession of chancellors who were fine, in a way—I'd say more, but this is public. But they didn't have that kind of impact. The special qualities of this place were the product of the actions of the faculty who were very busy in nearly ways in every aspect. What it came to—designing buildings and everything. We did that. The administration followed it. That's all changed. And I think the faculty in this place, quite in the same point of view, are very active. But I don't think faculty determines policy as much as it used to. On the other hand, as I was saying. As far as administration was concerned, without Roger—. I don't think this place would've developed along the lines of following. But we would have been like Herb done. He's okay. But the special qualities of this place, compared to Herb, I think Roger did it right—.

CHODOROW: Okay. Good.

WESTBROOK: [*inaudible*]

[END OF INTERVIEW]