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Randy Schekman is a cell biologist in Berkeley’s Department of Molecular and Cellular Biology, a Lasker Awardee, and as of 2013 a Nobel Laureate in Physiology or Medicine. From his long association with Dan as colleague and friend, he recalls Dan’s creation of the Chancellor’s Advisory Committee, a key element in the reorganization of biology at Berkeley in the 1980s, how Dan successfully pushed for Howard Hughes Medical Research Foundation Awards at Berkeley, perhaps the first campus without a medical school that the foundation would consider for awards. Dr. Schekman also recalls Dan’s contributions to Berkeley’s Health Sciences Initiative, especially the Li Ka Shing Science Building where Dr. Schekman and his laboratory now reside.
Interview 1: April 19, 2012

Audio File 1

Birth and early childhood in Minnesota, move to Southern California at age 10 — eldest of five children; parents Alfred and Esther Scheckman — college at UCLA, 1966-1970 — working in Dan Ray’s lab as a freshman — finding a community of science nerds — eagerness to work with Arthur Kornberg, acceptance to Stanford — first awareness of Dan Koshland in 1974 at Stanford — Kornberg’s insular and exclusive biochemistry lab — 1975 appointment at UC Berkeley by Dan Koshland — post doc at UC San Diego with Jonathan Singer — decision to leave Kornberg’s lab — work on yeast — tension between molecular biology and biochemistry departments — mentorship with Howard Schachman — allostery — Gunther Stent — the reorganization of biology — the Chancellor’s Advisory Committee on Biology — resistance to the reorganization of biology — personal friendship with Dan Koshland, meeting Bunny Koshland — Koshland’s role in 1987 appointment of UC Berkeley’s first Howard Hughes Medical Institute Investigator — UC Berkeley’s focus on biomedical research in spite of not having a medical school — Jim Allison’s work on T cells — the patent royalty system at Berkeley — changing practices in patenting as an academic scientist — first awareness of the Health Sciences Initiative — questioning the viability of the interdisciplinary approach — Dan’s anonymous $50 million contribution — other donors: the Gettys, the Valley Foundation, Whitaker — retrofit and upgrade of Barker Hall

Audio File 2

Baker Hall renovation delays new construction, difficulties in upgrading Barker — arguing to include Barker Hall upgrade in the Health Sciences Initiative — balancing fundraising needs with academic/scientific vision — comparing fundraising styles of Chancellors Berdahl and Birgeneau — Dan Koshland’s fundraising connections and talent — Robert Tjian’s fundraising skill, travel to Hong Kong to meet Li Ka-shing during the SARS outbreak — the politics of allocating lab space and retaining attractive space for use in recruitment — Graham Fleming — the stem cell group — ethics and politics of stem cell work — QB3 — Regis Kelly
Introduction by Sally Smith Hughes

The Daniel E. Koshland, Jr. Oral History Retrospective documents the scientific, philanthropic, and academic service activities of a scientist with deep and broad ties to the University of California, Berkeley and the wider scientific and philanthropic communities. The videotaped interviews with family members, scientific colleagues, and university personnel focus on the last years of his life, before his death in 2007. They provide perspectives on his diverse activities, his personality traits, and help to bring up-to-date the lengthy oral history with Dr. Koshland himself, which concluded in 1999.

This project, conceived and generously supported by his widow Yvonne Koshland, highlights the years 1999-2007 but also includes flashbacks to Dr. Koshland’s earlier activities. The Retrospective thus constitutes an amplification and extension of the earlier oral history but also stands as an unabashed tribute to a man whom the interviewees held in high esteem.

The Retrospective consists of interviews with seven individuals, amounting to roughly twenty hours of recordings, conducted in 2011-2012. Yvonne Koshland, in consultation with the interviewer, suggested the individuals to be interviewed, basing her choices on the unique perspectives on Dr. Koshland that each would present. All the interviews were videotaped, except for those with Mrs. Koshland, which, at her request, were only audiotaped.

Interviewees included:

Bruce Alberts
Jenny Cutting
Catherine Preston Koshland
Douglas Koshland
Yvonne Cyr Koshland
Randy Schekman
Robert Tjian

Project Staff included:

Project consultant: Yvonne Koshland
Project director and interviewer: Sally Smith Hughes
Videographers: Julie Allen, Travis Thompson
Project Support: David Dunham

Sally Smith Hughes
Berkeley, CA, 2014
Hughes: It is April 19, 2012, and we are in the office of Randy Schekman to talk a little bit about him, but also mainly about Dan Koshland and the Health Sciences Initiative. I want to start back with your early life and education. What kind of a family did you grow up in?

Schekman: Well, I was born in Minnesota. We moved to southern California when I was ten. My father, Alfred Schekman, was an engineer and my mom, Esther Schekman, was a homemaker. Five kids. I’m the oldest. I developed an interest in science early on through science fair projects in middle school—it was called junior high back then. I was the high school science nerd. No one else had that passion about science. And then I applied to UCLA. That was the only school I applied to. My family couldn’t imagine spending money on education. [laughter] It was back in a time in California when anybody could go to the University of California and affording it was not an issue. I could work a summer job and pay for the whole rest of the year. My parents didn’t have to pay anything, so what’s happened is tragic. Really, it’s a sea change. The campus administration refers to it as a disinvestment in higher education. I refer to it as an abandonment of higher education by the state. It’s just terrible. But there it is.

Anyway, so I went to UCLA. I loved it. They gave me everything that I needed. It gave me an opportunity to get into science. I worked in a lab starting my freshman year. I published papers as an undergraduate.

Hughes: Wasn’t that highly unusual, particularly at that time?

Schekman: Yes, it was unusual, but remember I was the science nerd in high school, and so to me it was quite natural. It was unusual though for an undergraduate. Most of my peers at UCLA who were interested in the life sciences were premeds. I was at first as well, but I realized early on that my passion was for science, not for medicine. And the premeds were more interested in getting good grades than in actually digging into the science. I realized that there was a community of scholars that I hadn’t known of, and so I wasn’t just the only science nerd around. [laughing] It turns out there were a lot of people who were passionate about science as well. So then as an undergraduate I developed an interest in chromosome replication, where I worked in Dan Ray’s lab studying this.

Hughes: What year was this?
Schekman: I was an undergraduate from 1966 to ‘70. And during this time I became very familiar with the work of the most prominent person in the field, Arthur Kornberg, who was a professor at Stanford. So I determined that I had to work with him as a graduate student. I was very fortunate. I got admitted to Stanford. It was the most selective program in the country at the time. They only admitted half a handful of graduate students each year, and everyone who had an offer from Stanford accepted in a virtual 100 percent at that time.

Hughes: It was the top-rated biochemistry department in the country, wasn’t it?

Schekman: Absolutely, absolutely. I think probably in the world. It was very focused on nucleic acid metabolism. I didn’t even think about Berkeley because there wasn’t really that emphasis here. Anyway, it was a trial by fire. Kornberg was a tough taskmaster, but he was a master, and I learned the trade from him, and it was wonderful.

Hughes: Were you interested in a reductive approach to science before you met Kornberg?

Schekman: Yes, oh yes, absolutely.

Hughes: Or was it Kornberg that instilled it in you?

Schekman: No. I was interested in reductionist science from the start. I learned a new aspect of it from him. What I had done before was more sort of molecular biology. But what I did with him was enzymology, isolating proteins, studying how they worked. And he was the best, so that was a great experience. I learned from him how to think about a problem, how to take it apart, in a way that I had not experienced before. In fact, it was during that period of time when I first saw Dan Koshland. He was a visitor to Stanford at the time. He was of course good friends with all the faculty there. I remember vividly he spoke in a symposium when I was in my last year [1974], and I remember him speaking at a seminar, and around that time he was talking about bacterial chemotaxis.

Hughes: I heard a lot about bacterial chemotaxis in the interviews with him![1]

Schekman: Yes. I didn’t meet him personally, but I became aware of him at that time and his role in the development of a molecular understanding of how bacteria respond to chemical gradients. These were really clever experiments where they were able to track bacteria swimming in three dimensions with a setup that now is so routine, but back then was really quite original. He had a postdoc by the name of [Robert M.] Macnab, who was instrumental in building a machine that would allow him manually to track bacteria as they swam in three dimensions.

Hughes: Is that principle still being used?

Schekman: Well, now one can track these things automatically. [laughing] Back then that was not possible. But he could do it manually and study their twiddling as they encountered gradients of chemoattractants or chemorepellents. I remember that vividly from his seminar and his presentation.

Hughes: And you were impressed.

Schekman: Yes, oh yes!

Hughes: I can tell. [laughter]

Schekman: I met a lot of impressive people then, and he was one of them, he was one of them.

Schekman: That was the great thing about that department. It was like the center of the universe, although of course most of time I was doing my own work. If I just sat in the library where they had seminars, day after day the best people in the world came by to talk about their work. Another great man who actually spoke at the same symposium with Koshland is a guy named Efraim Racker. Another really great biochemist, old friend of Kornberg’s. Not only did I meet him on that occasion, but he was at Stanford and attended my thesis presentation, my thesis defense! So it was just that kind of a place.

Hughes: Was that intimidating or exhilarating?

Schekman: Well, once you’ve dealt with Kornberg, nothing could be more intimidating than Kornberg.
Hughes: That’s true. [laughing]

Schekman: Racker was a sweetheart compared to Arthur.

Hughes: From talking to some of the UCSF people, comparing and contrasting the culture at Stanford Biochem and UCSF Biochem, the way it was portrayed by people who of course had a little bit of an axe to grind was that Kornberg’s department wasn’t cross-disciplinary in the sense that he didn’t willingly give joint appointments.

Schekman: No, that department was quite insular, in fact.

Hughes: Yes, and deliberately so.

Schekman: Oh yes. They were building an exceptional culture at an institution— When Kornberg was recruited from [Washington University] St. Louis to create a biochemistry department, the traditions in the basic life sciences were not that strong at Stanford. There were some excellent people, but he was a cut above anybody,

Hughes: And then of course he got the Nobel [1959] as soon as he arrived.

Schekman: Exactly, exactly. He hand-picked people, most of whom were his own postdocs, to join him, and so that aspect of exclusivity allowed him to continue at a very high level of performance. One felt as a student there that once you went beyond the double doors that bounded the department, you were in a different time zone. There were collegial interactions among the faculty, but rarely among the students. So much so that, I think, it hurt Stanford until later when they built up other strong departments. As an example, they had an outstanding young scholar at the time named Stan [N.] Cohen. Cohen had developed an interest in restriction enzymes and in their use in recombinant DNA and bacterial plasmids. He was kind of shunned by the Stanford biochemistry department, at least from my perspective. He would have had natural interactions with people like Ron [Ronald W.] Davis.

Hughes: He expected a joint appointment with Stanford Biochem because of contacts between Albert Einstein College of Medicine and Stanford. As you know, he

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2 Kornberg oral history: [http://content.cdlib.org/view?docId=kt6q2nb1tg&brand=calisphere&doc.view=entire_text](http://content.cdlib.org/view?docId=kt6q2nb1tg&brand=calisphere&doc.view=entire_text)
was in the Department of Medicine where there was nobody thinking molecularly the way he was, and he was very disappointed.

Schekman: Well, they didn’t think of him as one of them. He wasn’t a biochemist.

Hughes: Yet he was a biochemist!

Schekman: Was he by training?

Hughes: Well, I mean in his interests.

Schekman: Maybe in a broad sense but not in the Kornberg style of enzymology. You see, this is another thing and this is probably a digression. There used to be this division between what was called molecular biology and biochemistry. And Kornberg firmly believed that he was a biochemist and that molecular biologists were really kind of sloppy, imprecise geneticists, which for him was a pejorative. And so Cohen would have been considered a molecular biologist.

Hughes: Right. Well, you probably heard the story from Paul Berg that when he began to go into mammalian viruses—

Schekman: Oh yes, oh yes, Arthur criticized him.

Hughes: —Kornberg was horrified!

Schekman: Yes, yes.

Hughes: It had to be E. coli.

Schekman: Of course I did what Kornberg told me when I was a graduate student. [laughing]

But then when I came here and Dan Koshland was the chair of the Biochemistry Department—he hired me in 1975 and I started in ’76—my own career flourished as a result of being here rather than at Stanford. I remember when I was at Stanford I couldn’t imagine being in a better place, and I probably would have accepted an offer if they’d wanted me to come back. But in fact I think I was much better off being here, because in my own career I started with a genetic approach, but eventually it turned into biochemistry. I know for a fact that Kornberg was rather dismissive of my initial efforts, and
he would have given me no end of grief had I been there. So this was a much better place for me because even though I was in an actually quite classical biochemistry department, Koshland was here using bacterial genetics; Bruce Ames was here doing almost exclusively genetic and molecular biology. There were some who held this attitude about biochemistry as superior to molecular biology, but there were others among the younger group, more influential group, who clearly appreciated that genetics was an important tool for molecular discovery.

01-00:13:15 Hughes: Was Dan chair when you came?

01-00:13:17 Schekman: He was chair.

01-00:13:18 Hughes: From a personality standpoint, there’s quite a contrast between Koshland and Kornberg.

01-00:13:26 Schekman: Oh yes.

01-00:13:29 Hughes: Although they admired each other.

01-00:13:29 Schekman: Oh tremendously. But, you know, Arthur admired people who had different styles too. He was a big fan of a guy named George Palade who was a hero in my field, although he was not a biochemist at all. But Kornberg appreciated his vision and wisdom. Indeed, the year before I started in graduate school, Kornberg did a mini-sabbatical in Palade’s lab at Rockefeller [Institute for Medical Research].

01-00:13:57 Hughes: But you appreciated the greater freedom here?

01-00:14:01 Schekman: Yes, oh yes. As any good assistant professor, I had my own mind. I wanted to do what I wanted to do, and no one was going to tell me what to do. And they gave me the freedom here to fall on my face if that was what happened.

01-00:14:17 Hughes: Well, we’ve skipped over the two years at UC San Diego, which were pivotal, weren’t they, in redirecting your research?

01-00:14:28 Schekman: They were pivotal in many ways, yes. So as a graduate student as I was facing my future, I decided for various reasons that I wanted to do something completely different, not the least of which was I just saw how immensely powerful Kornberg was, and I just didn’t want to live in his shadow. I felt that
others who had continued in that area were always Kornberg protégés. I wasn’t embarrassed by having trained with him, but I wanted to be my own person. So I was greatly influenced by a fellow in Kornberg’s lab named Bill [William T.] Wickner, who was a postdoc in the lab who’d come from a prominent lipid lab led by Eugene Kennedy at Harvard Medical School. We would often talk till late in the evenings about membranes. I became really fascinated with the opportunities, felt that it was rather descriptive but there were things that I could do with my training that would maybe add something unique. And so in thinking about who to work with, I became aware of a fellow named Jonathan Singer, S.J. Singer, at UC San Diego, who was doing some very important work in visualizing proteins and membranes, but also just in generally thinking about how membranes were organized.

Hughes: At the biochemical level?

Schekman: Well, by the time I became aware of him, he was more what you’d call a cell biologist. But interestingly, the people in the Biochemistry Department at Stanford, or Kornberg, knew him from years earlier when he’d been actually a physical chemist. He was trained as a physical chemist, and they knew that work and really respected that work and thought that sounds like it’s good. What they weren’t aware of was that he’d become a very descriptive cell biologist. And so by the time I got there I realized very quickly that he and I thought differently about things, so it was actually kind of tense. But I benefited from the way he thought about things, but I really had no intention of pursuing in a longer term the kind of work that I did as a postdoc for that brief period. I found the system awkward and the approaches too limiting, and I really wanted to somehow plant my feet in something that I could do eventually using biochemistry.

Hughes: It wasn’t possible as long as he was your advisor?

Schekman: Well, you know, there was some argument back and forth over what I should do as a postdoc, and he eventually persuaded me to do a project that I worked on for a couple of years and didn’t see the future in it.

Hughes: Was that the mammalian cell approach?

Schekman: Yes, these were mammalian cells.

Hughes: Which you got away from.
Schekman: Which I got away from for a long time, and there were limitations in what one could do. I was spoiled; as a graduate student I was working with *E. coli*, where you could do everything—genetics, biochemistry. It was at the time the best system for a real reductionist. But it’s a prokaryote, and there’s a eukaryotic world, and particularly with membranes it’s really very different. And so I did what I thought I could do to learn from Singer, which was electron microscopy.

Hughes: Oh, was it?

Schekman: Yes. In retrospect—it doesn’t matter—in retrospect I would have been better off going to Palade’s lab. The ferment in the field was happening there at that time through the efforts of a postdoc of his by the name of Günter Blobel who would then become an independent person at Rockefeller. But I didn’t know enough about the field at the time because I was viewing it from a distance and Singer seemed to be— But it was fine, it was okay. Virtually immediately on getting there a job opening came up in the Biochemistry Department here, and I applied to it right away, even before I’d really even started my postdoc work. And so I was interviewing for the faculty position on the basis of my graduate work, not my postdoctoral work. Fortunately, it worked out. [laughing] I got the job. I think I wouldn’t have had such a good chance of getting a job on the basis of my postdoctoral work. So in a way I spent over a year—almost a year and a half of that two years—thinking about my future, which may have limited my effectiveness as a postdoc. It was actually a great opportunity to think: what do I want to do that blends what I did in Kornberg’s lab but studying membranes in eukaryotic cells?

Hughes: So the yeast came in when you arrived here?

Schekman: When I moved here, yes.

Hughes: Was anybody else working with yeast?

Schekman: Oh sure, oh yes. It was becoming the eukaryote of choice. People elsewhere in the world were working primarily on genetics, and here there was a senior biochemist by the name of Clinton Ballou. He was a carbohydrate chemist, but he was using genetics to study the biosynthesis of glycoproteins in yeast. He had a very different perspective on things, which was not where I was headed, but he was here and I thought, that will be helpful. I just liked the feel of the department. I liked the feel of the building. It was called the biochemistry building back then. It was a good, tight-knit community of...
people who clearly appreciated biochemistry and who I felt would be sympathetic to my aspirations.

Hughes: What about the relationship with the molecular biologists?

Schekman: Well, that was strained, although I didn’t really see that. I was interviewing in the Biochemistry Department, but when I arrived I could see there were strained relations.

Hughes: That was at Stanley Hall [where the virology department was located]?

Schekman: That was at Stanley Hall. Somebody who I became close to very quickly from that department is Howard Schachman, who is a wonderful man, really one of my great mentors here. He was a very close friend of Kornberg’s, much closer actually than Dan was. He had done a sabbatical with Kornberg in St. Louis in the mid-fifties. In fact, I realized he met Arthur even in the late forties, so they were very close. I remember seeing Schachman at Stanford on visits, and I met him at my job seminar here. But I could tell—it was unfortunate—but I could see there was tension. Unfortunately it was tension between Howard and Dan, which I could never understand.

Hughes: Was there?

Schekman: Oh yes, it was terrible. It was terrible. They would never shout at each other—they were too gentlemanly for that—but they were backbiting. There was backbiting all the time.

Hughes: What was the basis of that?

Schekman: Well, personality differences, although both of them were very funny guys, really very funny guys. Under other circumstances they would have been best friends, I think. But they had a very different upbringing—very different. Their politics was a little different. Howard is much more left than Dan was. But I think it boiled down to very different ideas about how proteins work. [laughing] It was a scientific clash over this theory called allostery, which Howard embraced but which Dan did not. So they had arguments about that, and just a different style. Dan was much more freewheeling and almost a dilettante in what he would choose to do. Howard was very focused. He was more like Arthur in being very focused and very precise.

Hughes: Well, that allostery business caused quite a bit of turmoil all over the place!
Yes, yes, yes. So Dan was an antagonist in that arena. It didn’t hurt him. Dan had a way with people. [laughing] He can charm his way out of any situation. I’ve seen it many, many times. But anyway, there was tension. And then the Department of Molecular Biology was essentially dysfunctional too. They had nuts in the department: Peter Duesberg and Heinz Fraenkel-Conrat.

Well, [Wendell M.] Stanley was a divisive person in his own right.

I never met him. I think he had died by the time I got here, so I didn’t know him. And [Gunther] Stent was an odd duck too.

One of my first scientific heroes was Gunther Stent. In high school I read a textbook that he’d written on bacterial viruses. So when I was thinking about college I actually thought briefly about Berkeley. I looked at the roster of the department, and I saw Gunther Stent, and I thought, wow, that would be great! But then in college I started to learn a little bit more about Stent and his frankly anti-reductionist views. I thought this guy’s just kind of a nut, and that was confirmed. [laughing]

Yes, and then his famous statement, however he put it, that all the great discoveries in molecular biology have been made. And there comes recombinant DNA.

He was a brilliant guy, but—I hate to say this—what he actually achieved in the laboratory was really minimal.

He turned himself more into a philosopher, don’t you think?

Yes, he was a man of letters really. I had some dealings with him over the years, not terribly favorable. But he was part of that molecular biology crowd, and it was unfortunate, because by the time I got here they were recruiting some really terrific people, like Liz Blackburn and Mike Botchan and Nick Cozzarelli, people whom I greatly admire and have been close to ever since. But they were kind of chomping at the bit being in that department, too.

Did you find interaction with molecular biology difficult? For one thing, there’s the geographic distance on campus.

Yes, one didn’t go up and down [the campus] so often back then. I remember I gave a seminar up there, and I’d barely ever been in the building. But now,
all the time. So yes, it was good that that department lost its identity during
the reorganization [of biology at Berkeley].³

01-00:25:25
Hughes: You came, as you said, to the Department of Biochemistry. But by 1989 you
were in the Department of Molecular and Cell Biology. Is that as a result of
the reorganization?

01-00:25:38
Schekman: Oh absolutely, yes. So the molecular biologists lost their identity—well, as did
Biochemistry at that time.

01-00:25:43
Hughes: You were getting kind of a preview of the multidisciplinary approach, weren’t
you?

01-00:25:48
Schekman: Yes.

01-00:25:48
Hughes: By encompassing people from separate departments.

01-00:25:54
Schekman: Well, I’m not sure it was pitched that way then. It was just a way of shaking
things up, reorganizing things, and redistributing people. But I think the
reassortment was done rationally to bring people of common interests into
divisions. So Botchan and Cozzarelli, and maybe Blackburn for a brief time,
became members of the Division of Biochemistry and Molecular Biology. But
the idea was not to be interdisciplinary at that time. It was just to rearrange
things, rearrange the deck in a maybe more rational way, because it had grown
up by an evolutionary process to be really dysfunctional, with a separate
Department of Molecular Biology, with a Department of Biochemistry, with a
Department of Zoology. There weren’t many universities left in the country
with a Department of Anatomy and Physiology, even though we didn’t have a
medical school. That was really a very weak department. So the
reorganization was an opportunity to shake things up, to give birth to a more
vigorous program in cell biology which had not been strong here for many
years, but which now was recreated in a Division of Cell and Developmental
Biology.

01-00:27:11
Hughes: Dan was one of the leaders of the reorganization effort.

01-00:27:15
Schekman: He was the leader, he was the leader.

I’m wondering if that had some kind of continuity in what we’re eventually going to get around to talking about, namely the HSI [Health Science Initiative]. He’d had experience in dramatically reorganizing biology on this campus. I’m thinking that must have played into his thinking when it came to HSI.

—Oh yes, Dan was instrumental, yes.

Yes, well he established himself as a master politician from that, and he realized how much influence he had, not that anyone was surprised. He created, as part of the reorganization, something called the Chancellor’s Advisory Committee on Biology.

Right.

It was kind of an overarching but virtual college of biology covering all of the departments and colleges in which life science work is done. He chaired this committee for some years and then passed the baton on to Bob Tjian, and he was the chair of that committee. I’ve been the chair now for some years. So Dan created this committee which had broad influence through the chancellor. I remember Dan served in that capacity over a succession of administrations, starting with [Chancellor Ira Michael] Heyman and then through [Chancellor Chang-Lin] Tien. I think probably he stepped down when Tien was still chancellor. But I remember Paul Gray, who was the EVC [Executive Vice Chancellor] at a certain point, still working with Dan. And when I became the chair of this committee, I remember Paul was still the EVC. He told me, he says, “I wake up every morning and I wonder: What can I do for Dan Koshland today?” [laughter] That’s how much influence Dan had! I think he won every award that the campus could muster—for many reasons, not the least of which was his wisdom.

So Dan created this committee which had great influence almost immediately, even before we had new buildings in the form of LSA [Life Sciences Annex] and the Koshland [Hall] building. What he did, which I think was really quite unique at the time and actually hasn’t been replicated—even on this campus it hasn’t been replicated—is that one of the key things this committee does is we more or less appoint the faculty who will sit on search committees for every life science job/recruitment on campus. Now, this committee is purely advisory, and initially there was some resistance to giving that influence to this committee.
Hughes: I can imagine.

Schekman: But it turns out, the deans are happy to— We’re not forcing their hand. We sit
around, and we think about who might help. And the principle is that every
committee must be interdisciplinary. There has to be at least one person from
outside the home department to serve on the committee, and that ensures that
the department isn’t simply going to replicate itself, that it’s going to strive for
new people, bring in other areas.

Hughes: Does the committee embrace the organismal biologists, the College of Natural
Resources people?

Schekman: Absolutely, absolutely. The dean of that sits on the committee. The jobs in
that college that are relevant to life science, because they’re not all relevant to
life science, are pretty diverse.

Hughes: I know from talking to Dan that there was considerable tension over the
reorganization with the organismal, the field biologists, whatever you want to
call them.

Schekman: With CNR, yes, sure. Yes, but it wasn’t just them, it was most of the life
science faculty. After all you have to think about this: when Dan started the
reorganization, there were sixteen different life science departments just
within L&S [College of Letters & Science]. And each one of those had a
chair, and the chairs— You know, they might have griped but they wanted
their authority. And this [reorganization] was a threat to their authority. So
many of them fought, but they didn’t fight effectively enough. I think the
crucial thing, that I only appreciated much later, was that Dan and really even
equally importantly Rod Park, who was then EVC, worked around the
Academic Senate in achieving the reorganization. Because the Academic
Senate was a very conservative force and would have prevented the
reorganization if they’d been engaged in the process. But they skillfully
manipulated around that committee and were able to do things that otherwise
couldn’t be done. Of course there was resistance. There’s always resistance.

Hughes: Well, they got their own college.

Schekman: Who got their own college?

Hughes: The College of Natural Resources.
Schekman: But that was long ago. I don’t know even when that happened. That was long before this.

Hughes: I thought it sprang from the reorganization.

Schekman: No, no, no. I don’t even know when that happened. That was certainly long before me.

Hughes: Okay.

There was some justification in arguing, particularly when you had a Dan Koshland at the top of the show, that molecular biology was probably going to be pretty dominant.

Schekman: Yes, but unlike Kornberg, Dan recognized there were other biological disciplines. Well, his wife [Marian “Bunny” Koshland] was an immunologist and also a reductionist but more cellular or maybe more attuned to physiology than Dan was. Dan handpicked—this is how he really manipulated the process—an external advisory committee of prominent people, including George Palade, from around the country, and Peter Raven who is a botanist. I remember Dan invited me—this was really fun—to a dinner the night before the external advisory committee was to meet to review the whole [Berkeley] program [in biology]. This was going back into the mid-eighties. And Dan knew how to work a room better than anybody else. [laughter] Maybe Obama can work a room better than Dan, but Dan was better at that than anybody else I ever knew. He went around and he worked the room, and he more or less gave these guys their marching orders, so that at the end of the day people who were already Nobel laureates, much more famous even than Dan Koshland, delivered. They delivered the desired external report.

Hughes: Which was that biology at Berkeley needed to be reorganized.

Schekman: Absolutely, absolutely. Well, I mean it was obvious. It was obvious.

Hughes: As I remember, one of the first clues that began to worry Dan and other people was the fact that the biological sciences on this campus were slipping.

Schekman: Yes, Robert Glaeser, the dean of biology at the time, gave some talk somewhere where he said Berkeley had slipped to number four in the Bay Area behind Stanford, UCSF, and Genentech. And those were fighting words.
I remember being interviewed by the external committee as a then-beginning assistant professor almost, or was it actually—

01-00:35:02
Hughes: What year was this?

01-00:35:03
Schekman: When was that external committee? Well, it was probably, oh gosh, ‘84? I don’t even remember now when that full process began. [The report of the External Biology Review Committee came out in April 1981.]

01-00:35:20
Schekman: Yes, well I was a young faculty member. Maybe I was already tenured at that point, but I was considered one of the younger faculty. [Some back and forth about correct date omitted.]

01-00:35:53
Hughes: Did you find some of this experience helpful?

01-00:36:01
Schekman: You mean watching Dan?

01-00:36:04
Hughes: Watching what Dan was doing, because the reorganization [of Berkeley biology] was a multifaceted operation, wasn’t it?

01-00:36:16
Schekman: Yes, yes.

01-00:36:16
Hughes: I’m imagining that you needed that skill when it came to HSI.

01-00:36:20
Schekman: Yes. Well, I learned a lot from Dan about how to do things. He and I shared a kind of edgy sense of humor, and we would always tease one another, and that was ongoing for decades. So I learned how effectively he used humor in getting people to do things that he wanted. I think that came naturally to me as well.

01-00:36:44
Hughes: I believe that you were social friends as well. It wasn’t just a matter of being colleagues.

01-00:36:56
Schekman: Oh yes.

01-00:36:58
Hughes: So you had a very human connection.
Very early on—I was just an assistant professor—he had a party at his house, and I hadn’t met his wife Bunny yet. And so he brought her over, introduced me—You must have visited with Bunny.

Schekman: Only very briefly.

Yes, very different than Dan. She was kind of a salty character actually. So the first words out of her mouth were, “Dan tells me you’re funny. Make me laugh.” Putting me on the spot! And I tried to deflect it. “No, no. Make me laugh.” [laughter] Very different than Kornberg. Although he had a sense of humor too, but it was different. Anyway, so right away Dan and I would use the opportunity of the annual Biochemistry Department retreats to poke at each other in front of the audience. I’ll treasure those memories.

Schekman: Was any of that recorded?

Oh gosh. I’d like to think that I have an old videotape of that somewhere. He was no longer chair, but he was at these meetings at Asilomar, and I’d pull stunts, and he’d get me back in front of everybody. I may have recorded it. I should try to resurrect that. Oh, God.

Schekman: If you ever choose to get rid of the videotape, the Bancroft Library would love to have it.

Yes. There might be some film in there of Koshland. Oh! That would be precious. Yes, I should think about how to resurrect that.

Schekman: All right. In 1990 you became a Howard Hughes [Medical Institute] Investigator. What was that about? How is that done?

Well, Dan again was instrumental. In 1987 the first Hughes investigators on this campus were appointed. That was Bob Tjian and Gerry [Gerald M.] Rubin, and that was Dan’s doing. At the time, Hughes had a policy of making appointments only at medical schools.

Schekman: So Berkeley was the first non-medical institution?

Well, maybe MIT at the same time. But Dan was pushing this—you could just see. He was editor of Science at the time, and there was some rumor that he had threatened to write an editorial about this if Hughes didn’t. But Purnell
Choppin, who was then the president of Hughes— I’ve recently had a conversation with him about this, and he said, “We loved Dan, but no, it was the IRS that was the reason for this policy, and we were able to get that changed.” And it was only with the caveat that all appointments here had to have a joint appointment at UCSF, and probably at MIT they had to have joint appointments at Harvard Medical School. That was just window dressing for the IRS.

Hughes: Why did the IRS care?

Schekman: Well, because Howard Hughes was considered a medical research organization, and this is part of the problem.

Hughes: And there was no medical school at Berkeley.

Schekman: Yes, yes, they said how can you be calling yourself a medical research organization, funding people at a place that’s not doing medical research? Well, that was also part of the reason for the Health Sciences Initiative, to clarify for others that we actually do biomedical research. We’ve always done biomedical research here.

Hughes: Well, I certainly noticed that theme. Jenny Cutting gave me access to some of the promotional material for the HSI, and one of the things that struck me was the medical theme: we’re doing basic science for medical application.

Schekman: Yes, and we’ve had enormous success. The most prominent success that we’ve had is with our former colleague Jim Allison. Do you know about his work?

Hughes: I did a retrospective oral history project on Bunny Koshland, and he was one of the people that I talked with.4

Schekman: She hired him, yes. He was at this obscure campus in the University of Texas system where he had discovered what’s called the T cell receptor, the protein molecule that is the receptor on T cells that allows the cells to attach to and kill cells that have some foreign substance on their surface. Others had cloned the gene for the T cell receptor, so he didn’t quite get in there at that very

early time, but he clearly had made a major discovery. Bunny recognized this as something important and lured him here, whereas other places took really no note of him. She had an eye for talent. So then he became eventually the leader of the immunology effort here on campus, became the division head of immunology, and then he and I were co-chairs of MCB [Molecular & Cell Biology] for several years.

During this time he developed an interest in how one might control or even boost the action of T cells in recognizing foreign antigens on cells. I don’t know the history of this, but apparently there’s a protein that cells secrete that serves as a break on the action of the T cells. It’s more something that would dampen a hyperactive T cell reaction. So it’s just part of the normal regulatory scheme of things. But he thought, well, what if we could transiently eliminate that protein, that break, and unleash T cells, maybe they’d be more effective in dealing with tumors. And so he started some experiments in mice, probably almost twenty years ago here at Berkeley, and got really dramatic responses—tumor regression—spent some time trying to mobilize interest in a biotech; got nowhere. He finally did find a smaller pharmaceutical firm that was interested in this approach, and then they had to start thinking seriously about clinical trials. So at that point he was lured away to Sloan-Kettering [Cancer Center] by Harold Varmus, who was then the president of Sloan-Kettering. That was really a terrible loss [for the campus].

Hughes: Oh, he’s gone! I didn’t realize that!

Schekman: Yes, he has been gone for ten years or so now. But fortunately the patent was issued here on the idea. And during those years at Sloan-Kettering the work really blossomed, and the trials progressed with good success. And eventually last year the FDA approved this pharmaceutical, and it’s now a very effective treatment for a fraction of people who suffer from melanoma.

Hughes: What company is commercializing it?

Schekman: Oh gosh, I don’t know. There was just a symposium here in Jim’s honor, and I’ve forgotten the name of the company. You can probably find this out, but I think it’s probably been taken over by some major pharmaceutical firm now. [Medarex in partnership with Bristol-Myers Squibb.] But anyway, the good news about it is that, although overall the increase in life expectancy (as usual for these drugs) is a few months, if you study the entire population of people who were treated, there’s a cohort of about 25 percent of the patients who benefit from what appears to be permanent remission, now over a decade, which is unheard of in melanoma.
He’s now moving back to Texas to M.D. Anderson [Cancer Center] where they have a huge cancer program. What he’s doing now is looking at what other backup systems may be mounted that could be taken down to make this even more effective. That is, to boost this from 25 percent survival up to 70-80 percent. When he spoke about this, and the others who spoke about this at the symposium, it struck me as a new paradigm shift. Most of us think now that you’d be treating one particular type of cancer at a time with a tailored drug like this drug Gleevec which affects a very specific type. But no, not necessarily. This treatment may actually be a general treatment for cancer.

Hughes: Wouldn’t that be exciting.

Schekman: Because it’s not attacking the tumor, and so it wouldn’t be tumor-specific. It’s boosting the immune system. So that would be monumental. So the campus has benefited from this in many ways, not the least of which is financially.

Hughes: Is the patent issued and making money for the campus?

Schekman: Oh, yes. The campus monetized this application for melanoma. They got, I don’t know, it was close to $100 million. The campus got a sizable chunk of that. Of course Allison has done quite well. Some of that has come to the MCB Department, and we’re living on that money now.

Hughes: I see. How does the patent royalty system work here? I’m familiar with the tripartite Stanford system.

Schekman: Yes, I think we have the same thing: a third to the investigator, a third to the department, a third to the institution—or a third to the college and a third to the institution. So some of the money was used to pay down the note on this building [Li Ka Shing]. Some of it has gone into a fund for us to help in recruiting. And the rest has gone to the campus and a lot to Allison.

Hughes: Well, your tenure here has been long enough that you probably have lived through the sea change in academic perspective on patenting, particularly in the biomedical sciences.

Schekman: Yes. I grew up at a time when one didn’t think about patenting ideas or techniques. There were probably people patenting things back when I was a student, but at Stanford it was all basic science. There was no interest in commercialization.
Hughes: Until recombinant DNA.

Schekman: That was after I left [1974]. So I wasn’t part of that. And I’ve never patented anything.

Hughes: Oh really?

Schekman: I’ve been involved in biotech companies, and I greatly regret not having patented certain things, but I was always too excited about the science and not anything practical.

Hughes: Well, Dan applied for patents late in life when he was working on something related to—

Schekman: He had an idea about biofuels and hydrocarbon metabolism. I don’t know what the idea was, and I don’t know whether it was ever feasible. But he was excited about it, I remember.

Hughes: But it was patented, wasn’t it?

Schekman: Was it patented? But even if it’s patented it doesn’t mean that it’s going to be taken up in a license.

Hughes: It was a notable switch in research orientation.

Schekman: Well, his DNA has business in it, even though he never did any himself. But he comes by it honestly. [Koshland’s father and uncle were executives of Levi Strauss & Co.]

Hughes: Later in life, Dan was very interested in application, and perhaps at a time when that wasn’t quite as common among his colleagues.

Schekman: Well, late in his life that was very common around here.

Hughes: True.

Schekman: People were patenting things and getting royalties, and the department had a certain royalty stream. I think that’s almost an expectation now. But it just
wasn’t when I was growing up, so I’ve never applied for a patent, and I have no business sense whatsoever. [laughter]

Hughes: Well, you’re good at what you chose to do. Let’s not spread you too thin.

Schekman: Well, I’m comfortable doing what I do and with sticking to that.

Hughes: All right. You have a phone call in twenty minutes. Let’s get full bore into the Health Sciences Initiative. Perhaps you could start with how the initiative first arose.

Schekman: You know, I don’t know exactly its origin. But I first became aware of the HSI when committees were formed, I think at Dan’s instigation, probably through the Chancellor’s Advisory Committee. Committees were formed to come up with proposals for building projects, and the campus was divided into quadrants. And it wasn’t just the life sciences by the way. There was a northwest quadrant and this corner of campus—I think the campus was divided into four, and each was challenged to catalyze the formation of a group to come up with a building proposal, with the intention being interdisciplinary. And I don’t know that it was even just strictly life science. It was interdisciplinary. On the more physical side the physicists were challenged to conceive of an interdisciplinary program. And I think it was reduced to bickering in many quarters.

The only two quadrants that came up with a viable plan for an interdisciplinary building were the Stanley corner of campus, including the life scientists up there, and this side of campus [the western edge]. Dan affixed himself to the corner up there, and they came up with a reasonable model for how they would organize it. This was very different actually than the reorganization of campus biology because it meant that you’d be taking existing departments and mixing them in the same building. It wouldn’t be that you’d change the boundaries of the departments. You’d just be creating a building that would have a bunch of different departments, and no one department or any department wouldn’t own space.

Hughes: Was it one building in the early stages?

Schekman: No, the thought was one or more buildings. It was just, what was going to be viable? Who is really going to buy into this idea of interdisciplinary? I think the Health Sciences Initiative grew out of that, because I don’t think it was necessarily health related at first. But you should ask Tij [Robert Tjian] about this. He was more embedded in that.
So anyway, that quadrant of campus came up with a plan first, and Dan— I don’t know at what point this was public, but he committed $50 million to the project in the form of a charitable remainder. So it wasn’t money in the bank; it was in his estate that would eventually come to the campus for this project. But the campus felt pretty secure about that.

Hughes: Was the money targeted at the new Stanley Hall?

Schekman: Yes. So that was a good nucleus to build on.

Hughes: Why did he keep it anonymous?

Schekman: [sighing] Well, he just didn’t want to draw attention. He did that with other things too. There are other funds from him on campus that were anonymous. But you can’t keep these things quiet. Do you know there was this big competition for a new campus on Roosevelt Island in New York that Bloomberg—You know about this?

Hughes: No.

Schekman: There’s this huge new initiative that Mayor Bloomberg started to create a school of science and engineering in New York City, on a space that the city owned on Roosevelt Island. This started a huge competition nationwide. Stanford was competing and Cornell and Columbia and other institutions. Eventually Stanford withdrew because they just weren’t used to dealing with New York City. [laughter] Cornell, which has a medical school in New York City, was very familiar with it. There was a very dramatic day a few months ago when [President John] Hennessy announced that Stanford was withdrawing from the competition. Within hours President [David J.] Skorton of Cornell announced that he had a donor— I don’t know, it was a $175 million so-called anonymous donor. [laughing] Well, there aren’t too many people who can remain anonymous and give that kind of a gift! So by the next news cycle they managed to figure out who this so-called anonymous donor was. I think that was more or less true of Koshland too. How many people are going to give, anonymously, $50 million for a building that he just happened to have a hand in. So I vaguely remember it being anonymous, but not really.

Anyway, that donation was a good nucleus. And other entities—I don’t remember who, but I think the Valley Foundation may have given money.

Hughes: Yes, the Valley Foundation and the Gettys.
Schekman: The Gettys have been friends all along. I think maybe Whitaker [International Program] did.

Hughes: I think so. I saw an application in the material from the Development Office.

Schekman: Because there was going to be bioengineering. So that got going pretty well. But inevitably these buildings are more expensive than what one can raise just from private funds, so the state eventually had to put in money.

Hughes: Was that a struggle?

Schekman: Well—yes. [laughing] It’s always a struggle.

Hughes: I had never heard of the California Institutes for Science and Innovation. Isn’t that a state entity?

Schekman: Oh yes. This was [former Governor] Gray Davis’s creation, these institutes. A guy named Richard Lerner at Scripps, who was the president of Scripps, was instrumental in convincing Gray Davis to use state funds to create these institutes for science. There was a big competition within the UC system for these institutes. They were distributed, and there was one at UCLA in nanotechnology. QB3 was created out of that, which was a joint function application from Berkeley, UCSF primarily, and then also Santa Cruz. And there are others. I don’t remember the other institutes.

Hughes: Did some of that state money also go to the actual buildings?

Schekman: Probably it did. I don’t remember that detail, but I wouldn’t be surprised if some of the QB3 money was used for the building. And then there’s another institute, CITRIS [Center for Information Technology Research in the Interest of Science], on the [Berkeley] campus that’s a state science initiative.

Hughes: Oh right, yes.

Schekman: So that building—it was enormously expensive—was patched together with private and public funds.

And likewise this building [Li Ka Shing]. This building took a little longer. Partly that was my fault, I’ll admit. I was at this time in Barker Hall; Barker Hall was falling apart. The campus recognized that it was seismically
unsound. In fact it was the least seismically sound building on campus at a
certain point. The campus then committed state funds for a seismic upgrade,
and they were going to keep us in the building while this was going on. I
learned from colleagues I met in the geophysics building that in no way
should I allow the campus to do this because in spite of their assurances that it
could be done consistent with continuing work it was an unmitigated disaster.
These things that they did to the building that they claimed would contain the
noise were just destructive.

I was the chair of the [MCB] department at the time, but I also more or less
appointed myself to be the faculty liaison for this seismic upgrade of Barker.
First of all I recognized there was no way that I was going to allow them to
keep us in the building. So then the campus had to find funds to disperse the
faculty within the building elsewhere on campus, and I fought that battle not
only with the campus but with my colleagues. The campus started to use
money that had been secreted away for the Health Sciences Initiative to now
backstop what I was doing at Barker. [laughing] And then once we actually
were safely out of the building, I told them, now that we were away and
you’re going to have this period to upgrade the building, you ought to have a
look at the lab; it’s a shambles. The building had not been well maintained.
The building systems were not going to be upgraded, and frankly it was
embarrassing. And so I fought with [Chancellor Robert] Berdahl— Well, I
didn’t fight with him, I pleaded with them to commit funds for the upgrading
of Barker. It was still going to be one of the key life science buildings on
campus. That was about a three-year battle. Eventually, at considerable
expense, that building was renovated. And that delayed the construction of
this building [Li Ka Shing] because they were siphoning funds that had been
set aside for this to upgrade Barker.

Hughes: And also taking your attention away from the HSI.

Schekman: Well, I wasn’t the principal in that at the time. I was the chair of MCB. Tij
was the chair of the Health Sciences Initiative at that point.

Hughes: You were talking before the break about the problems in Barker. So did the
project become HSI then and the new building here, rather than renovating
Barker?

Schekman: I think it delayed progress on this building because they were having to use
precious funds to cover the expense of the renovations that Barker required.
And my colleagues, I will say the administration, I think they bit their tongue
because my demands for renovation of Barker were causing a delay in the
new building project. But I think my colleagues were very supportive because Barker’s a significant building, and it was going to be many years before this new building was realized, and that building [Barker] was just a shambles. It suffered under this deferred maintenance problem on campus, and it was becoming really very difficult to work in it. My lab had been renovated because I was able to get [Howard] Hughes [Medical Institute] support for that purpose. So my lab was fine, but the other labs in the building were really pretty awful—depressing, just depressing. So I thought, we’re getting out of the building now. They’re going to upgrade it, so the building will be torn apart. So this is the best opportunity to also refurbish the building systems and the labs. And that’s impeccable logic but it costs. [laughing] And where was the pot of money? Well, it was building up for this [HSI] project. So Tij was very supportive. He was in control of the Health Sciences Initiative, and he was very supportive, so the campus reluctantly had to go along with that.

Hughes: You mean in taking some of the funding for Barker?

Schekman: Yes, well there was no separate pot of money for Barker except for the state money for the seismics.

Hughes: But Randy, is that legitimate when you have people donating for Li Ka Shing and then using it for Barker?

Schekman: Well, okay, I don’t know which accounts were taken for that purpose. Obviously they weren’t taking things away that had been specifically devoted to this building. They were maybe out raising more money generally for the Health Sciences [Initiative]. I think the position that I argued was that Barker should be embraced, should be part of the Health Sciences Initiative. It shouldn’t just be these two new buildings [Li Ka Shing and Stanley Hall]. And I think maybe that’s how they were able to get some more money for that purpose.

Hughes: And that came to be.

Schekman: Yes, yes.

Hughes: Does it really function that way?

Schekman: Well, the faculty who now are in this building, many of us were in Barker. So we didn’t suddenly change our identity moving over to the building. So yes, when the chancellor asked me to start the Stem Cell Center on campus it was in my capacity as a faculty member in Barker Hall. So this building took
longer to complete as a result of that, no doubt. If they had ignored Barker they would have had this building a few years earlier. I’m afraid it would have been without me, but anyway.

02-00:03:30 Hughes: What was your burden? Because it seems to me there are two main streams and maybe more in HSI. One obviously is the funding. But the other, and I’m surmising that you were part of it maybe more than the funding, is the academic/scientific vision—what are we trying to create here?

02-00:03:55 Schekman: Yes, yes, yes. Well, that vision, the way it’s realized is in faculty recruitment. Increasingly over the years we’ve been trying to strengthen areas where we were underrepresented at the recruitment level, in bringing people in who study mammals, and not just more people studying model organisms or at least lower organisms. So we’ve traditionally been very strong in model organisms—yeast, bacteria, worms, flies—but over the years we’ve been increasingly successful in bringing in people who study mammals where disease implications are more tangible.

02-00:04:45 Hughes: Are you saying that it was just filling a niche by bringing in the mammalian people? Or you saw them as doing research closer to application to the human?

02-00:05:03 Schekman: I think I would say the former. I think what I’ve really enjoyed about being at Berkeley all along is the emphasis on basic science. We are not a medical school. I don’t feel the pressure from clinicians to make my work relevant. Actually, I work mainly on mammalian cells now. I do things because I see scientific opportunities, not because I’m looking necessarily to cure diseases. But inevitably when you do good things, people find applications for them, and that was true even more so when I was working on yeast.

02-00:05:43 Hughes: Does the administration appreciate that? Because the emphasis on application is about raising money and public support.

02-00:05:53 Schekman: Yes, well you have to be politically savvy and know where the support will come from. But I know [Chancellor] Bob Birgeneau, and he’s a basic scientist, and it’s the scholarship that comes first. And there’s no compromising on that.

02-00:06:13 Hughes: What about [Chancellor] Berdahl who is an historian?

02-00:06:20 Schekman: Well, I thought less of his passion for scholarship. He was a very effective public speaker, perhaps more so than Birgeneau. I think some of the
difficulties that Birgeneau’s had recently might not have happened to Berdahl because he comes from that culture of the humanities. But in his passion for science Birgeneau has been much stronger. He’s been a more effective fundraiser. But Berdahl was important during that phase when we were raising funds for this building and for Stanley. The economy was much better, so in fact we did raise more money.

Hughes: Well, what did happen with the dot-com bust? Because HSI arose about that time.

Schekman: Yes. Well, that cut into fundraising for the campus.

Hughes: I can imagine.

Schekman: Well, let’s see, Birgeneau started as chancellor after the dot-com bust, but Birgeneau was simply much more aggressive in fundraising than Berdahl was. He of his own initiative went out and got this Hewlett money for these chairs.

Hughes: Oh, did he?

Schekman: Absolutely. That’s 100 percent him. He was also absolutely instrumental in the BP [British Petroleum] business, against significant and not unexpected campus opposition. But he was there; he went along with the team to London for the presentation of the case for Berkeley—Berkeley and Illinois. He was right there involved in that, and it was one of his great victories, I think. I don’t recall Berdahl making that kind of an effort. Berdahl was compromised in some sense because he had family health problems that kept him from being as effective as he might. His daughter was dying of cancer. He himself had prostate cancer while he was chancellor, I think his wife had had breast cancer. So his emotions were diverted during much of the time. And that really made it difficult.

Hughes: Bringing the subject back to Dan: not only because of his family background but because of who he was, Dan had many, many contacts, not only in the world of science but also in philanthropy. Did that come into play in the fundraising for HSI? Did he play a heavy hand in that?

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55 In 2007, BP selected UC Berkeley and Lawrence Berkeley Laboratory and the University of Illinois at Urbana-Champaign to lead a $500 million research effort to develop new sources of energy.
Oh yes, yes. Other than of course his own obvious direct philanthropy, of course he knew these people. I’m sure he knew the Gettys, and he may have even known Gordon Moore.

But as much as Dan was important for that period of fundraising, I actually give even more credit to Tij, who didn’t know these people but has made a point of becoming very close to many of them. Tij has gifts that even exceed Dan’s in his ability to cultivate connections to campus among this very select group, and actually he’s really remarkable in his skill in that respect. So we really miss him now that he’s not here as much, but he’s still working on our behalf. He’s still cultivating these people. He goes fly-fishing with Gordon Moore, and he’s got people that Dan didn’t even know that he’s reeling in. And he comes by it honestly too. His family was also a very wealthy family, not on the level of a Koshland. [telephone interruption] He didn’t make the initial contact with Li Ka-shing. But I’ll never forget Tij heroically, during the thick of the SARS epidemic which hit Hong Kong almost along with Toronto, flew directly into Hong Kong, took a limousine from the airport to meet Mr. Li, took the limousine back, and flew right out of Hong Kong, all in one day! He deserved war payments for that. Stunning. Put yourself back at that time—people were petrified because it was not clear where this epidemic was going. But there was a crucial time of negotiations where Tij felt a personal touch was critical, and he just flew into danger to do it.

Do you think that Tij’s Chinese background helped in that relationship?

Oh yes, oh yes.

But you’re saying that they didn’t know each other before this.

Not at all. I think others had made that contact with Mr. Li, and you’ll have to ask Tij who else. [laughing] I think various other people take credit for that, but Tij was important after the initial contact was made. [Chang-Lin] Tien, when he was chancellor, identified Tij as a leader and cultivated him, and I think the Asian connection was very important. Tij, if he were inclined, would make a great chancellor here, but I’m afraid he’s not willing to leave his current position for that.6

Well, it’s pretty important to have him there too, don’t you agree?

6 In 2008, Tjian was elected president of the Howard Hughes Medical Institute.
Schekman: Yes. And he’s made a commitment there. He’s only been president of HHMI a couple of years, but he would be a natural as chancellor here.

Hughes: Well, let’s talk about you. What do you feel was your main contribution to the HSI initiative?

Schekman: Well, I don’t think I had an instrumental role in it other than building up Barker. I will claim that as my achievement even though it wasn’t initially a part of the HSI. Looking back, I’m pleased with the way that developed, and without that I think we wouldn’t have been able to recruit during the time the funds were being built up for Li Ka Shing. I think Stanley very quickly filled up with faculty who were already here. There was some space for growth, but I think they didn’t keep enough space for growth.

Hughes: This building—we’ve kept space open. We’ve kept people out of the building who were perfectly good scientists and who wanted to be in the building because we want to keep space open for new recruiting.

Hughes: Was that deliberate on the part of people organizing Stanley Hall or was it an oversight?

Schekman: Well, Graham Fleming was in charge of that. That was the first new building up there on the northeast side of campus for a long time, and I think he was buffeted by lots of forces demanding to be put in that building, some who should have been resisted. I won’t name names, but the Chemistry Department is very prominent, has very strong-willed people. They had at considerable expense renovated beautiful labs within Lewis [Hall]. There are some beautiful new labs in Lewis that were renovated by the Chemistry Department, and many of the biochemists up there were in that space. And frankly they should have been kept in that space.

Schekman: Which was close to Stanley geographically.

Schekman: Yes. But many of these people pushed to get into Stanley, and that took space that could otherwise have been used for new recruits. I think that was a big mistake, and I don’t know who’s to blame for that. Graham Fleming was in charge at the time and had to deal with these pressures.

Hughes: So they’re handicapped now in their recruiting effort?
Schekman: Yes, there’s an occasional space that opens up, but they’re saturated. I’m now in charge of this building [Li Ka Shing], and I’ve been pretty single-minded about keeping people out that don’t fit the mission, primarily to keep space open for recruiting, because this is a very attractive place to recruit people.

Hughes: Does the stem cell group have the most space?

Schekman: Well, we have several groups who do embryonic stem cell work who have come in from other buildings. There’s a floor in Stanley that is stem cell. There are a couple of floors here that were built with support from the CIRM [California Institute for Regenerative Medicine], and there are stem cell people who are going to be in here. We’re recruiting one now. We’re hopeful to recruit somebody very shortly who would be on this floor. (Added by Schekman in reviewing the transcript: A new stem cell faculty member was recruited and has now joined us in LKS). David Schaffer, who’s in Chemical Engineering and Bioengineering is moving part of his lab here. He’s now the director of the stem cell program. Daniela Kaufer, from Integrative Biology, is a stem cell biologist. She’s on this floor. We’ve recruited somebody from the Salk Institute, a senior person, who will be on this floor, who does stem cell work. So we’re building up an emphasis here. That was crucially dependent on CIRM funds.

Hughes: Because there still are restrictions on federal funds, are there not?

Schekman: Well, not so much, not so much. There aren’t too many people here who are deriving new stem cell lines or who need to derive them. That’s really the crux of the federal problem. If you actually intend to derive new lines then you need to have private funding. I don’t know how vigilant the government is on that anymore. The people who were first doing this type of research, a guy at Harvard named Doug Melton, really did have to have completely separate lab facilities. At a certain point, he even had a bodyguard because of threats to him because of his work. That’s kind of quieted down. What’s changed really dramatically is a breakthrough made by a Japanese scientist, Shinya Yamanaka, who’s figured out how to make embryonic-like stem cells from adult tissues.

Hughes: So there goes the ethical problem.

Schekman: Well, most of it but not all of it, not all of it. Because these synthetic stem cells are not perfect replicas of authentic stem cells from embryos, so one still needs some source of these authentic cells, at least for comparison purposes.
Hughes: I see. We haven’t talked in any detail about QB3. Do you want to talk about that?

Schekman: Yes. QB3 is an interesting development. There have been some, I would say, cross-campus rivalries that have not been fully satisfied. UCSF took the lead in that, and they’ve got a very effective director whom I’ve known for much longer than I’ve been a faculty member here. Reg[K] Kelly was a postdoc with Kornberg, and I first learned about his work when I was an undergraduate. It was through his work that I became interested in Kornberg’s lab, and I met him when I was an undergraduate, in 1969.

Hughes: And then he went to UCSF Biochem.

Schekman: He’s been at UCSF for many years. So anyway, he’s really a terrific leader. But he has been, naturally, promoting UCSF’s vision more than anyone else’s, and there’s been some tension here as a result of that. Graham Fleming chafed under that, as does Susan Marqusee now. But both Fleming and—

Hughes: Are they the leaders?

Schekman: They’re the local [UCB] leaders of the QB3. But in spite of that both were very effective in building up QB3 here and getting many program project grants to outfit Stanley. Graham was really instrumental in that and mobilizing groups of people to apply for funds. And they were actually quite successful because these state science initiatives QB3 came with very little money, almost nothing. So really it was more or less a license to go out and raise money, and Fleming did that very well. Susan has now taken over from him and is running the show. She’s also very effective as well in keeping the facilities going.

Hughes: In the promotional materials that I read about the HSI, one of the points was to encourage interaction with industry.

Schekman: Yes.

Hughes: Doesn’t UCSF have a step-up in that regard?

Schekman: Yes, they do very well in that respect, and it’s always an advantage that a medical school has.
Hughes: Right, and also the Mission Bay campus is supposed to be next to a budding biotech industry.

Schekman: Yes, I don’t know how effective that’s been. I’m not sure that’s working out so well, but that hasn’t kept Reg from really aggressively reaching out to pharmaceutical companies to set up collaborations.

Hughes: And has that worked?

Schekman: I think that’s worked, so they have money from that.

Hughes: I go back to what you said in terms of your own orientation towards basic science. Isn’t that more appropriate for Berkeley?

Schekman: Well, why is that? Not necessarily. Look at our engineering school.

Hughes: Well, that’s true.

Schekman: They’re very well connected to industry, very well, as well connected as Stanford, and they rely on that. So why are we [in basic science] not more effective in that respect? I think we should be. I would fault us and myself for not being more entrepreneurial. A lot of basic science departments in non-medical schools—look at MIT. Their life science faculty is much more entrepreneurial than we are in really seeking out biotech connections. That has not been part of our culture here. So some, like Tij, are great. [telephone rings] That’s my call at quarter till.

Hughes: We’ll have to stop. Thank you.

[End of Interview]